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VARIETIES OF POLITICAL EXPRESSION IN SOCIOLOGY

Insiders and Outsiders—Merton

Radical Politics and Sociological Research—Becker and Horowitz

The Politics of American Sociologists—Lipset and Ladd

Professionalization of Sociology—Janowitz

On Gouldner's Crisis—Rhoads

Political Judgments and Social Perceptions—Dibble

Early Marxist Thought—Coser

With an Introduction by Bottomore and an Epilogue by Baltzell

REVIEW ESSAY

Merton's 17th-Century England—Nelson

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IN THIS ISSUE

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- Davis, K. 1963*a*. "The Theory of Change and Response in Modern Demographic History." *Population Index* 29 (October): 345-66.
- . 1963*b*. "Social Demography." In *The Behavioral Sciences Today*, edited by Bernard Berelson. New York: Basic Books.
- Goode, W. J. 1967. "The Protection of the Inept." *American Sociological Review* 32 (February): 5-19.
- Moore, Wilbert E., and Arnold S. Feldman. 1960. *Labor Commitment and Social Change in Developing Areas*. New York: Social Science Research Council.
- Sanford, Nevitt, ed. 1962. *The American College*. New York: Wiley.

Introduction

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It would be possible to comment on these essays from various points of view. From one aspect, they are explorations in the sociology of knowledge which—mercifully—deal directly with the social influences upon knowledge instead of raising yet again questions about the theoretical possibility of a sociology of knowledge.

More narrowly, however, most of the essays are concerned in some way with the social context of political thought, especially radical thought, and in this respect they mark out, and contribute to, a field of inquiry which has been much neglected. The study of conservative thought which Karl Mannheim published in 1927 has never been adequately complemented by a structural and historical analysis of radical thought, except perhaps in the little known work by C. Bouglé, *Les idées égalitaires* (actually published two years earlier, in 1925), which provides, at the same time, an interesting contrast with Mannheim's approach, inasmuch as it is inspired by Durkheimian rather than Marxist ideas. Marxism itself is an obvious candidate for this kind of investigation, and Lewis Coser's essay in the present symposium is noteworthy for initiating a study along these lines, although I have reservations about his actual procedure which I shall set out later in this paper.

Last, a large part of the symposium is concerned especially with recent radical thought in the United States, not only from the point of view of its social context but also in terms of the relationships—of mutual influence, suspicion, antagonism, or conflict—which may exist between radical political doctrines on one side and sociological theory on the other.

In the following comments I shall limit myself mainly to the second and third set of issues which I have just outlined. For this reason I shall discuss less fully than it deserves the essay by Robert Merton, which approaches the broader questions raised by the sociology of knowledge in a fresh and illuminating way, by examining some of the implications of the contrast between "knowledge from the inside" and "knowledge from the outside." Of course, some of Merton's observations bear directly upon current problems. He notes that in times of acute social conflict and great social change the perspectives offered by the various sociologies of knowledge are closely linked with the problems agitating society. "As the society becomes polarized, so do the contending claims to truth." In short, the sociology of knowledge also has a social context.

Merton illustrates the contrast between "insiders" and "outsiders" par-

ticularly by reference to the black movement, and the claims sometimes advanced, in connection with Black Studies, that *only* black historians can truly understand black history, only black sociologists can understand the social life of blacks, etc. Sociologically, this corresponds, he suggests, with a need for group self-affirmation in a situation of conflict and change. Epistemologically, it produces difficulties; for there are multiple group affiliations, and if there are reasons for saying that only blacks can understand blacks, the same reasons would indicate that only women can understand women, only young people can understand young people, and so on. Thus, we should have to admit, for example, that young black women could only be studied properly by other young black women. Any kind of general social science is thereby ruled out. So is all historical writing, since the historian is always an outsider. Merton might have noted that a pure "insider" doctrine would also eliminate a considerable part of Black Studies, for a black historian teaching Afro-American history is no more of an insider (in the sense of a participant in the events) than is a white, yellow, or brown historian.

If it is then argued simply that a black historian would embark on his study from a distinct perspective and would bring a special insight to it, this is to assert a weaker version of the insider doctrine; and the claim would have to be assessed against the claims made for the virtues of the "outsider" approach, namely, the special kind of insight into a situation which the external observer, the "stranger"—who is detached from local prejudices and animosities and able to make comparisons with other situations—can bring to his study.

The problems which Merton explores are posed in somewhat different terms in several of the essays. Obviously Marxism is, in some versions, a form of "insider" doctrine. If Marxism is conceived *only* as the world view of the proletariat, then it is true for this class but not for other classes; it is the self-consciousness of the proletariat in capitalist society, providing an insight into the structure and development of society which the members of other classes cannot acquire. But this idea has usually been qualified in various ways. On one side, Marxism is also presented as an objective science of society—that is, as a body of theory and empirical descriptions formulated from the standpoint of an external observer. On the other side, some Marxists (notably Lukács and Lenin), in discussing the question of "false consciousness," have argued that a *correct* class consciousness is brought to the proletariat by socialist intellectuals, or, in other words, by "outsiders."

Thus, Marxist thought has often embodied a fruitful tension between the "inside" and the "outside" view; and the same is true of other kinds of radical thought, which are partisan from one aspect in that they investigate social life from the viewpoint of the interests and aspirations of particular social groups, but are also objective insofar as they base their

interpretations upon the evidence of actual conditions and events. Becker and Horowitz, however, conceive recent radical thought mainly in its partisan character. Unlike Merton, who sees the "insider" and the "outsider" as complementing each other, they are more inclined to emphasize a total disjunction between "radical" sociology, which is an ideological orientation, and "good" sociology, which is an objective science conforming with publicly acknowledged standards of scientific evidence and argument.

Of course, there are grounds for making such a sharp distinction. A good deal of radical thought, in the past decade, has amounted to little more than what Alain Touraine has called the "repetition of ideology." Becker and Horowitz refer to this characteristic by observing that most radical sociology has been "programmatic" and has not taken shape in a body of empirical studies. This is also a characteristic of Marxism, over a much longer period, and there is an interesting contrast between sociology and history in this respect. There have been many good Marxist historians, especially in the fields of economic and social history, who have worked quite comfortably in accordance with the general standards of historical scholarship, however distinctive the conceptual scheme which they have brought to their work. There have been far fewer Marxist sociologists, and their contribution is less impressive, although it is evident that Marxism has inspired, in a diffuse way, much sociological theory and inquiry. The difference seems to arise in the first place from the fact that Marxism and sociology can be conceived as alternative global schemes of interpretation, and this rivalry has produced controversies which are largely methodological and philosophical in character, especially on the Marxist side, where a preoccupation with the inner structure of Marxist thought, and criticism of "bourgeois" thought, tend to assume preeminence over any study of the external world. This tendency is reinforced by the dogmatic element in Marxism; as sociology is very largely concerned with the study of modern (including present-day) societies, and as Marxism already contains some fundamental propositions about the character of modern capitalism, the revolutionary role of the working class, the more or less necessary transition to a socialist society, it may well appear, from an extreme ideological and dogmatic Marxist perspective, that research into such matters is otiose.

Becker and Horowitz point to some other influences which have inhibited the development of radical sociology in a more positive direction: conventional techniques of research, common-sense standards of the credibility of explanations, and agency sponsorship. It would obviously be worthwhile to investigate such influences more fully and, in particular, to look at the consequences of agency financing and sponsorship. The kind of analysis which Vernon Dibble makes of the research reports published by the German Verein für Sozialpolitik could very usefully be extended to other scholarly associations and research bodies, in an attempt to reveal some of

the factors which give a particular direction to social research in various countries at different times, and which impart to the evaluation of research findings a more radical or more conservative cast.

There are, however, more general influences which have held back sociological research of a radical kind. One, which I have already noted, is the extremely strong pressure of ideology. Another, perhaps the most important, seems to me to arise from a distinguishing feature of radical thought, namely, its orientation toward the future. In many ways it is much easier to be a conservative sociologist, engaged in the description and analysis of existing social institutions and of the various forces which have given them their present form. A radical sociologist, on the other hand, must try to discern those trends and movements, in the present, which seem likely to bring about a future social transformation, and he has to be concerned, in part, at least, with sketching the characteristics of institutions which do not yet exist. Thus, radical thought has, inevitably, a more speculative character, and it is less easy to embody in programs of research.

But this is not to say that a great deal more empirical research could not be undertaken. There are, after all, many precursors of new social institutions—in the organization of industrial work, in the provision of social services, and so on—which have emerged from the revolutions of this century and yet have been very inadequately studied. There are also other kinds of problems which do not involve, to the same extent, speculation about the future form of society. One set of problems emerges from the failure of socialism in a number of countries in the course of this century. There is now an accumulated historical experience of what can go wrong with the efforts to create a more equal, less coercive type of society; and radical sociologists might well undertake more thorough critical studies of this experience, so that they and others can learn from it. Another problem, which I have always considered extremely important, though few radicals have ever shown much interest in it, is that presented by the sectarian tendencies in radical movements themselves. In some societies, during certain periods, there has been a notable proliferation of radical sects; and the competition, or conflict, among them has generally impeded the development, and the political success, of a broad radical movement. One factor in this situation is probably the degree of social differentiation in modern societies; Merton, in his paper, alludes to the problem of achieving unity in large social movements when their members are differentiated by cross-cutting status sets. But there may well be other influences at work, and it would not be inappropriate, for example, to introduce some hypothetical comparisons between religious and political sects. Once again, research along these lines might prove to be of considerable value to radicals themselves, by revealing such possibilities as there are for overcoming internal divisions.

Whatever the reasons may be, it is apparent that the radical thought of the recent past (and of some earlier periods) confined itself mainly to criticism, rather than establishing an alternative body of knowledge.¹ Even in their critical function, however, radical doctrines are themselves open to criticism. Lewis Coser, in his study of Marxist thought in the first quarter of this century, asserts that "Marxist theorists have contended that Marxism is exempted from the claims of the sociology of knowledge that all thought structures and ideas need to be investigated in relation to . . . existential conditions and social structures." This is not strictly true. Lukács, in *History and Class Consciousness*, recognized explicitly that historical materialism had to be applied to itself and described it, in relativistic terms, as the "self-consciousness of capitalist society," existentially determined by the situation of the proletariat. What is true is that Marxists have not generally employed the sociology of knowledge, or the more specifically Marxist theory of ideology, in detailed studies of the diversity of Marxist thought itself and, still more widely, of socialist thought. There have been some attempts along these lines—for example, in the formulation of a connection between "reformism" and the "labor aristocracy"—and there is the outline of a more coherent theory in Gramsci's writings on the intellectuals. Nevertheless, the greater part of Marxist discussion of the divisions within Marxist thought has taken the form of theoretical controversy about the "correctness" or otherwise of a particular interpretation of Marx.

Coser illustrates the possibility of a more detailed interpretation of Marxist thought from the perspective of the sociology of knowledge by examining two forms of Marxist theory—the positivistic and the voluntaristic—in relation to the social milieu in which they originated and developed. However, the approach which Coser adopts, relying upon an analysis of the work of individual thinkers, seems to me inadequate and in some respects misleading. Merton offers some pertinent observations on this point, when he brings against a total "insider" doctrine the objection that it assumes "total coincidence between social position and individual perspectives" and thus "exaggerates into error the conception of structural analysis which maintains that there is a *tendency for, not a full determination of*, socially patterned differences in the perspectives, preferences, and behavior of people variously located in the social structure." Coser, I think, commits this error, and there are many particular objections which can be

¹ There are, of course, some notable exceptions. C. Wright Mills, and after him G. William Domhoff and others, carried out serious research on classes and elites, and in this field there is an empirically founded radical theory. Recently, Richard Flacks, in "Towards a Socialist Sociology" (*Insurgent Sociologist* 2 [Spring 1972]: 18–27), outlined an array of research problems for radicals. Although this is still "programmatic," it is so in a very practical way, in the style of an earlier, neglected statement of radical social science, Robert Lynd's *Knowledge for What?*

brought against his analysis. For instance, why emphasize so strongly the formative influence of Gramsci's Sardinian childhood, and not his student years in the industrial city of Turin, or his deep involvement with the factory councils? Again, if Lenin's "voluntarism" was determined by his milieu, how are we to account for the fact that the same milieu produced Bukharin's much more "positivistic" version of Marxism, which both Lukács and Gramsci criticized for its positivism? And if the character of Rosa Luxemburg's Marxism is traceable to her Polish origins, how shall we explain the equally "voluntaristic" version of Marxism expounded by Karl Korsch, whose ideas were developed in the environment of the "industrial heartland"? The most convincing part of Coser's study is that which deals, not with the social determination of the ideas of individual thinkers, but with the influences which affected the acceptance of particular versions of Marxist theory by large social groups. Thus, he draws attention to some differences between the Social Democratic movement in north and south Germany which help to account for the more favorable reception of Bernstein's reformism in the south. Along such lines there are obviously many valuable studies to be made of the social influences upon the historical fluctuations as well as the geographical divergences, in the acceptance of particular formulations of Marxist doctrine (for example, the rise of "humanist Marxism" in the 1960s), and also comparisons with societies in which Marxism never became an important political force at all.

Much of the counter-criticism of radical thought has been concerned with radical accounts of the conservative orientation of postwar sociology. Two essays take up this issue. Rhoads argues, in the latter part of his essay, that Parsons's theory is not fundamentally conservative because it does allow for tensions, conflict, and change. The depiction of a condition of social equilibrium and of conformity to a normative order, he says, is an "ideal-type"; "the continuation of stability is a theoretical assumption, . . . and harmony is only a point of reference for the analysis of empirical events." In practice, however, the "ideal-type" merges into the "ideal"; and the work of Parsons and his followers, as is well known by this time, consistently emphasizes harmony, equilibrium, and the integration of disturbing elements as *empirical* characteristics of modern industrial societies, while persistently neglecting major social conflicts and those social movements which might produce a fundamental change in the social system. In those cases where Parsons has commented on empirical events and problems, as in his essay on the situation of black Americans, he is still primarily concerned with integration into the present social system, and not with any hypothesis so radical as the idea that the system itself might have to be changed in order to solve the problems.

It may well be, as Lipset and Ladd claim, that Parsons's attitude to many social problems is liberal rather than conservative, but this may reflect a

disjunction between the theory which he produces as a thinker, and the practical judgments which he makes as a member of society. This supposition is confirmed to some extent by the fact that when Parsons does analyze current social problems, he does not usually begin from any propositions derived from his own general theory but places his discussion within a conceptual framework borrowed from social theorists who differ from him profoundly (in the essay on black Americans, for example, he uses the concepts of T. H. Marshall and Gunnar Myrdal).

There is another, quite startling, example of such disjunction, mentioned by Lipset and Ladd. A survey of Japanese sociologists apparently revealed that those under 30, who were more radical in politics than older age groups, also referred to Parsons more frequently as a non-Japanese sociologist worthy of considerable attention, and did not refer to Marx at all. Findings as surprising as this ought not to be taken simply at face value but should provoke deeper inquiries. However, insofar as they do describe authentically a state of mind, they could be regarded as showing the extent to which academic social scientists have come to accept a separation between the discipline which they teach and any personal conceptions of social life, not in the acceptable sense of making a distinction between facts and values, but in the sense of isolating in separate compartments of the mind two different theories of society—one public and academically acknowledged, the other private.

These examples lead to a wider issue, which is one of the principal themes discussed by Lipset and Ladd. The two questions which they raise in their paper are, first, whether functionalism, and especially the Parsonian version of it, has been the dominant theoretical orientation in American sociology over the past few decades, and, second, if this is so, and if Parsonian theory is as conservative as has been alleged, how it could become so influential in a profession which is markedly "left-wing" or "liberal" compared with other intellectual professions and with the general population. On the first point they conclude, in my view rightly, that neither Parsons's theory nor any other theory was really "dominant." Nevertheless, Parsons was clearly influential, as were the functionalists in general; and the question remains concerning the favorable reception of their theories by sociologists who were liberal or radical in politics. The answer suggested by Lipset and Ladd is that Parsonian and functionalist theories are not fundamentally conservative. But two other answers are possible. One is along the lines that I indicated above, namely, that individual sociologists came to lead a double life and to practice "double think," accepting one theory in their academic role, and another in their private political role. Another answer would be to say that functionalism did have a generally conservative influence, and that sociologists might otherwise have been more radical than they were; a corollary would be that those sociologists who rejected func-

tionalism altogether were in fact more consistently radical (or else that in order to be radical they found it necessary to reject functionalism). Although the data provided by Lipset and Ladd are not sufficient to decide this question, there are some indications that such was the case. The most radical sociologists, notably C. Wright Mills, did find the sociological theory of functionalism incompatible with a radical political theory.

Lipset and Ladd, therefore, do not settle definitively the question they raise but present us, rather, with a series of new questions about the connections between sociological theories and political doctrines. One might say that the symposium as a whole opens up, in a similar way, new fields of inquiry; and I should like to end this comment by indicating some of the main issues which seem to deserve closer attention. In the first place, it would be illuminating to examine in much greater detail the conservative orientation in social and political thought which developed in the Western industrial countries during the 1950s, in an attempt to see how theories in sociology and other social sciences influenced and were influenced by general political doctrines and attitudes, and how these in turn were affected by social conditions. A second issue concerns the rise of a new radicalism during the 1960s, on which much has already been written, but without the kind of structural analysis which would link the appearance of social movements and doctrines with changes in society, in the composition and relationships of groups. From these two questions arises the most difficult problem of all, namely, the succession of radical and conservative orientations in social thought and political action. A comprehensive sociology of political knowledge would have to explore in many different directions the causes of such fluctuations, taking account of the succession of generations, cultural fashions, and longer-term changes in the social structure.

Insiders and Outsiders: A Chapter in the Sociology of Knowledge¹

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The sociology of knowledge has long been regarded as a complex and esoteric subject, remote from the urgent problems of contemporary social life. To some of us, it seems quite the other way.² Especially in times of great social change, precipitated by acute social conflict and attended by much cultural disorganization and reorganization, the perspectives provided by the various sociologies of knowledge bear directly upon problems agitating the society. It is then that differences in the values, commitments, and intellectual orientations of conflicting groups become deepened into basic cleavages, both social and cultural. As the society becomes polarized, so do the contending claims to truth. At the extreme, an active and reciprocal distrust between groups finds expression in intellectual perspectives that are no longer located within the same universe of discourse. The more deep-seated the mutual distrust, the more does the argument of the other appear so palpably implausible or absurd that one no longer inquires into its substance or logical structure to assess its truth claims. Instead, one confronts the other's argument with an entirely different sort of question: how does it happen to be advanced at all? Thought and its products thus become altogether functionalized, interpreted only in terms of their presumed social or economic or psychological sources and functions. In the political arena, where the rules of the game often condone and sometimes support the practice, this involves reciprocated attacks on the integrity of

¹ A first edition of this paper was read on November 6, 1969 to the seminar celebrating the 50th anniversary of the department of sociology at the University of Bombay, India. A second edition was read at the Centennial Symposium of Loyola University (of Chicago) on January 5, 1970 and at the annual meetings of the Southwestern Sociological Association in Dallas, Texas, on March 25, 1971. This third edition was presented at the annual meeting of the American Sociological Association in Denver, Colorado, September 1, 1971. Any errors I have retained after the critical examinations of the paper by Walter Wallace and Harriet Zuckerman are of course entirely my own. Aid from the National Science Foundation is gratefully acknowledged, as is indispensable help of quite another kind provided by Hollon W. Farr, M.D.

² As witness the spate of recent writings in and on the sociology of knowledge, including far too many to be cited here. Some essential discussions and bibliography are provided by Berger and Luckmann (1966), Stark (1958), Wolff (1965), Curtis and Petras (1970). The application of the sociology of knowledge to the special case of sociology itself has also burgeoned since 1959 when the Fourth World Congress of Sociology held by the International Sociological Association focused on the social contexts of sociology. See, for prime examples, Gouldner (1970), Friedrichs (1970), Tiryakian (1971).

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the opponent; in the academic forum, where normative expectations are somewhat more restraining, it leads to reciprocated ideological analyses (which often deteriorate into barely concealed *ad hominem* innuendos). In both, the process feeds upon and nourishes collective insecurities.³

SOCIAL CHANGE AND SOCIAL THOUGHT

This conception of the social sources of the intensified interest in the sociology of knowledge and some of the theoretical difficulties which they foster plainly has the character, understandably typical in the sociology of scientific knowledge, of a self-exemplifying idea. It posits reciprocal connections between thought and society, in particular the social conditions that make for or disrupt a common universe of intellectual discourse within which the most severe disagreements can take place. Michael Polanyi (1958, 1959, 1964, 1967) has noted, more perceptively than anyone else I know,⁴ how the growth of knowledge depends upon complex sets of social relations based on a largely institutionalized reciprocity of trust among scholars and scientists. In one of his many passages on this theme, he observes that

in an ideal free society each person would have perfect access to the truth: to the truth in science, in art, religion, and justice, both in public and private life. But this is not practicable; each person can know directly very little of truth and must trust others for the rest. Indeed, to assure this process of mutual reliance is one of the main functions of society. It follows that such freedom of the mind as can be possessed by men is due to the services of social institutions, which set narrow limits to man's freedom and tend to threaten it even within those limits. The relation is analogous to that between mind and body: to the way in which the performance of mental acts is restricted by limitations and distortions due to the medium which makes these performances possible. [1959, p. 68]

But as cleavages deepen between groups, social strata or collectivities of whatever kind, the social network of mutual reliance is at best

³ This passage on the conditions making for intensified interest in the sociology of knowledge and for derivative problems of theoretical analysis in the field has not been written for this occasion. It is largely drawn from my paper in Gurwitsch and Moore (1945, but now out of print) and reprinted in Merton (1968, pp. 510-14). Since the cognitive orientations of group members and nonmembers has long been a problem of enduring interest to me, I shall have occasion to refer to my writings throughout this paper.

⁴ Polanyi's detailed development of this theme over the years represents a basic contribution to the sociology of science by providing a model of the various overlapping cognitive and social structures of intellectual disciplines. Ziman (1968) has useful observations along these lines and Campbell (1969) has contributed some typically Campbellian (i.e., imaginative and evocative) thinking on the subject, in developing his "fish-scale model" of overlapping disciplines.

strained and at worst broken. In place of the vigorous but intellectually disciplined mutual checking and rechecking that operates to a significant extent, though never of course totally, within the social institutions of science and scholarship, there develops a strain toward separatism, in the domain of the intellect as in the domain of society. Partly grounded mutual suspicion increasingly substitutes for partly grounded mutual trust. There emerge claims to group-based truth: Insider truths that counter Outsider untruths and Outsider truths that counter Insider untruths.

In our day, vastly evident social change is being initiated and funneled through a variety of social movements. These are formally alike in their objectives of achieving an intensified collective consciousness, a deepened solidarity and a new or renewed primary or total allegiance of their members to certain social identities, statuses, groups, or collectivities. Inspecting the familiar list of these movements centered on class, race, ethnicity, age, sex, religion, and sexual disposition, we note two other instructive similarities between them. First, the movements are for the most part formed principally on the basis of ascribed rather than acquired statuses and identities, with eligibility for inclusion being in terms of who you are rather than what you are (in the sense of status being contingent on role performance). And second, the movements largely involve the public affirmation of pride in statuses and solidarity with collectivities that have long been socially and culturally downgraded, stigmatized, or otherwise victimized in the social system. As with group affiliations generally, these newly reinforced social identities find expression in various affiliative symbols of distinctive speech, bodily appearance, dress, public behavior patterns and, not least, assumptions and foci of thought.

THE INSIDER DOCTRINE

Within this context of social change, we come upon the contemporary relevance of a long-standing problem in the sociology of knowledge: the problem of patterned differentials among social groups and strata in access to certain types of knowledge. In its strong form, the claim is put forward as a matter of epistemological principle that particular groups in each moment of history have *monopolistic access* to particular kinds of knowledge. In the weaker, more empirical form, the claim holds that some groups have *privileged access*, with other groups also being able to acquire that knowledge for themselves but at greater risk and cost.

Claims of this general sort have been periodically introduced. For one imposing and consequential example, Marx, a progenitor of the sociology of knowledge as of much else in social thought, advanced the claim that after capitalistic society had reached its ultimate phase of development,

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the strategic location of one social class would enable it to achieve an understanding of the society that was exempt from false consciousness.⁵ For another, altogether unimposing but also consequential example involving ascribed rather than achieved status, the Nazi *Gauleiter* of science and learning, Ernest Krieck (1935), expressed an entire ideology in contrasting the access to authentic scientific knowledge by men of unimpeachable Aryan ancestry with the corrupt versions of knowledge accessible to non-Aryans. Krieck could refer without hesitation to "Protestant and Catholic science, German and Jewish science." And, in a special application of the Insider doctrine, the Nazi regime could introduce the new racial category of "white Jews" to refer to those Aryans who had defiled their race by actual or symbolic contact with non-Aryans. Thus, the Nobel Prize physicist, Werner Heisenberg, became the most eminent member of this new race by persisting in his declaration that Einstein's theory of relativity constituted "an obvious basis for further research." While another Nobel laureate in physics, Johannes Stark, could castigate not only Heisenberg but his other great scientific contemporaries—Planck, von Laue, and Schrödinger—for accepting what Stark described as "the Jewish physics of Einstein" (Merton 1968, pp. 538–41).

For our purposes, we need not review the array of elitist doctrines which have maintained that certain groups have, on biological or social grounds, monopolistic or privileged access to new knowledge. Differing in detail, the doctrines are alike in distinguishing between Insider access to knowledge and Outsider exclusion from it.

SOCIAL BASES OF INSIDER DOCTRINE

The ecumenical problem of the interaction between a rapidly changing social structure and the development of Insider and Outsider doctrines is examined here in a doubly parochial fashion. Not only are my observations largely limited to the United States in our time but they are further limited to the implications of doctrines advocated by spokesmen for certain black social movements, since these movements have often come to serve as prototypical for the others (women, youth, homosexuals, other ethnics, etc.).

Although Insider doctrines have been intermittently set forth by white elitists through the centuries, white male Insiderism in American sociology

⁵ Observations on the advantaged position of the proletariat for the perception of historical and social truth are threaded throughout Marx's writings. For some of the crucial passages, see his *Poverty of Philosophy* (1847, e.g., pp. 125–26). On Marx's thinking along these lines, Georg Lukács, in spite of his own disclaimers in the new introduction to his classic work, *History and Class Consciousness*, remains of fundamental importance (1971, esp. pp. 47–81, 181–209).

during the past generations has largely been of the tacit or de facto rather than doctrinal or principled variety. It has simply taken the form of patterned expectations about the appropriate selection of specialties and of problems for investigation. The handful of Negro sociologists were in large part expected, as a result of social selection and self-selection, to study problems of Negro life and relations between the races just as the handful of women sociologists were expected to study problems of women, principally as these related to marriage and the family.

In contrast to this de facto form of Insiderism, an explicitly doctrinal form has in recent years been put forward most clearly and emphatically by some black intellectuals. In its strong version, the argument holds that, as a matter of social epistemology, *only* black historians can truly understand black history, *only* black ethnologists can understand black culture, *only* black sociologists can understand the social life of blacks, and so on. In the weaker form of the doctrine, some practical concessions are made. With regard to programs of Black Studies, for example, it is proposed that some white professors of the relevant subjects might be brought in since there are not yet enough black scholars to staff all the proliferating programs of study. But as Nathan Hare, the founding publisher of the *Black Scholar*, stated several years ago, this is only on temporary and conditional sufferance: "Any white professors involved in the program would have to be black in spirit in order to last. The same is true for 'Negro' professors."⁶ Apart from this kind of limited concession, the Insider doctrine maintains that there is a body of black history, black psychology, black ethnology, and black sociology which can be significantly advanced only by black scholars and social scientists.

In its fundamental character, this represents a major claim in the sociology of knowledge that implies the balkanization of social science, with separate baronies kept exclusively in the hands of Insiders bearing their credentials in the shape of one or another ascribed status. Generalizing the specific claim, it would appear to follow that if only black scholars can understand blacks, then only white scholars can understand whites. Generalizing further from race to nation, it would then appear, for example, that only French scholars can understand French society and, of course, that only Americans, not their external critics, can truly understand American society. Once the basic principle is adopted, the list of Insider claims to a monopoly of knowledge becomes indefinitely expandable to all manner of social formations based on ascribed (and, by extension, on some achieved) statuses. It would thus seem to follow that only women can understand women—and men, men. On the same principle, youth alone is

⁶ Nathan Hare as quoted by Bunzel (1968, p. 32).

capable of understanding youth just as, presumably, only the middle aged are able to understand their age peers.⁷ Furthermore, as we shift to the hybrid cases of ascribed and acquired statuses in varying mix, on the Insider principle, proletarians alone can understand proletarians and presumably capitalists, capitalists; only Catholics, Catholics; Jews, Jews, and to halt the inventory of socially atomized claims to knowledge with a limiting case that on its face would seem to have some merit, it would then plainly follow that only sociologists are able to understand their fellow sociologists.⁸

In all these applications, the doctrine of extreme Insiderism represents a new credentialism.⁹ This is the credentialism of ascribed status, in which understanding becomes accessible only to the fortunate few or many who are to the manner born. In this respect, it contrasts with the credentialism of achieved status that is characteristic of meritocratic systems.¹⁰

Extreme Insiderism moves toward a doctrine of *group* methodological solipsism.¹¹ In this form of solipsism, each group must in the end have a monopoly of knowledge about itself just as according to the doctrine of

⁷ Actually, the case of age status is structurally different from that of other ascribed statuses. For although, even in this time of advanced biotechnology, a few men become transformed into women and vice versa, this remains a comparatively rare instance of the ordinarily ascribed status of sex becoming an achieved status. But in contrast to sex and other ascribed statuses, each successive age status has been experienced by suitably long-lived social scientists (within the limits of their own inexorably advancing age cohorts). On the basis of a dynamic Insider doctrine, then, it might even be argued that older social scientists are better able than very young ones to understand the various other age strata. As context, see the concept of the reenactment of complementary roles in the life cycle of scientists in Zuckerman and Merton (1972).

⁸ As we shall see, this is a limiting type of case that merges into quite another type, since as a fully acquired status, rather than an ascribed one, that of the sociologist (or physician or physicist) presumably presupposes functionally relevant expertise.

⁹ I am indebted to Harriet Zuckerman for these observations on the new credentialism of ascribed status. The classic source of meritocracy remains Young (1958); on the dysfunctions of educational credentialism, see Miller and Roby (1970, chap. 6).

¹⁰ But as we shall see, when the extreme Insider position is transformed from a doctrine of assumptions-treated-as-established-truth into a set of questions about the distinctive roles of Insiders and Outsiders in intellectual inquiry, there develops a convergence though not coincidence between the assumptions underlying credentials based on ascribed status and credentials based on achieved status. In the one, early socialization in the culture or subculture is taken to provide readier access to certain kinds of understanding; in the other, the component in adult socialization represented by disciplined training in one or another field of learning is taken to provide a higher probability of access to certain other kinds of understanding.

¹¹ As Agassi (1969, p. 421) reminds us, the term "methodological solipsism" was introduced by Rudolf Carnap to designate the theory of knowledge known as sensationism: "the doctrine that all knowledge—of the world and of one's own self—derives from sensation." The belief that all one *really* knows is one's subjective experience is sometimes described as the "egocentric predicament."

individual methodological solipsism each individual has absolute privacy of knowledge about him- or her-self. The Insider doctrine can be put in the vernacular with no great loss in meaning: you have to be one in order to understand one. In somewhat less idiomatic language, the doctrine holds that one has monopolistic or privileged access to knowledge, or is wholly excluded from it, by virtue of one's group membership or social position. For some, the notion appears in the form of a question-begging pun: Insider as Insider, one endowed with special insight into matters necessarily obscure to others, thus possessed of penetrating discernment. Once adopted, the pun provides a specious solution but the serious Insider doctrine has its own rationale.

We can quickly pass over the trivial version of that rationale: the argument that the Outsider may be incompetent, given to quick and superficial forays into the group or culture under study and even unschooled in its language. That this kind of incompetence can be found is beyond doubt but it holds no principled interest for us. Foolish men (and women) or badly trained men (and women) are to be found everywhere, and anthropologists and sociologists and psychologists and historians engaged in study of groups other than their own surely have their fair share of them.¹² But such cases of special ineptitude do not bear on the Insider *principle*. It is not merely that Insiders also have their share of incompetents. The Insider principle does not refer to stupidly designed and stupidly executed inquiries that happen to be made by stupid Outsiders; it maintains a more fundamental position. According to the doctrine of the Insider, the Outsider, no matter how careful and talented, is excluded in principle from gaining access to the social and cultural truth.

In short, the doctrine holds that the Outsider has a structurally imposed incapacity to comprehend alien groups, statuses, cultures, and societies. Unlike the Insider, the Outsider has neither been socialized in the group nor has engaged in the run of experience that makes up its life, and therefore cannot have the direct, intuitive sensitivity that alope makes empathic understanding possible. Only through continued socialization in the life of a group can one become fully aware of its symbolisms and socially shared realities; only so can one understand the fine-grained meanings of behavior, feelings, and values; only so can one decipher the unwritten grammar of conduct and the nuances of cultural idiom. Or, to take a specific expression of this thesis by Ralph W. Conant (1968): "Whites are not and never will be as sensitive to the black community

¹² As I have noted in the first edition of this paper, the social scientists of India, for one example, have long suffered the slings and arrows of outrageously unprepared and altogether exogenous social scientists engaging in swift, superficial inquiries into matters Indian (Merton 1971, p. 456).

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precisely because they are not part of that community." Correlatively, Abd-l Hakimu Ibn Alkalimat (Gerald McWorter) draws a sharp contrast between the concepts of "a black social science" and "a white social science" (1969, p. 35).

A somewhat less stringent version of the doctrine maintains only that Insider and Outsider scholars have significantly different foci of interest. The argument goes somewhat as follows. The Insiders, sharing the deepest concerns of the group or at the least being thoroughly aware of them, will so direct their inquiries as to have them be relevant to those concerns. So, too, the Outsiders will inquire into problems relevant to the distinctive values and interests which they share with members of *their* group. But these are bound to differ from those of the group under study if only because the Outsiders occupy different places in the social structure.

This is a hypothesis which has the not unattractive quality of being readily amenable to empirical investigation. It should be possible to compare the spectrum of research problems about, say, the black population in the country that have been investigated by black sociologists and by white ones, or say, the spectrum of problems about women that have been investigated by female sociologists and by male ones, in order to find out whether the foci of attention in fact differ and if so, to what degree and in which respects. The only inquiry of this kind I happen to know of was published more than a quarter-century ago. William Fontaine (1944) found that Negro scholars tended to adopt analytical rather than morphological categories in their study of behavior, that they emphasized environmental rather than biological determinants of that behavior, and tended to make use of strikingly dramatic rather than representative data. All this was ascribed to a caste-induced resentment among Negro scholars. But since this lone study failed to examine the frequency of subjects, types of interpretation, and uses of data among a comparable sample of white scholars at the time, the findings are somewhat less than compelling. All the same, the questions it addressed remain. For there is theoretical reason to suppose that the foci of research adopted by Insiders and Outsiders and perhaps their categories of analysis as well will tend to differ. At least, Max Weber's notion of *Wertbeziehung* suggests that differing social locations, with their distinctive interests and values, will affect the selection of problems for investigation (Weber 1922, pp. 146-214).

Unlike the stringent version of the doctrine which maintains that Insiders and Outsiders must arrive at different (and presumably incompatible) findings and interpretations even when they do examine the same problems, this weaker version argues only that they will not deal with the same questions and so will simply talk past one another. With the two versions combined, the extended version of the Insider doctrine can also be put in the vernacular: one must not only be one in order to understand

one; one must be one in order to understand what is most worth understanding.

Clearly, the social epistemological doctrine of the Insider links up with what Sumner (1907, p. 13) long ago defined as ethnocentrism: "the technical name for [the] view of things in which one's own group is the center of everything, and all others are scaled and rated with reference to it." Sumner then goes on to include as a component of ethnocentrism, rather than as a frequent correlate of it (thus robbing his idea of some of its potential analytical power), the belief that one's group is superior to all cognate groups: "each group nourishes its own pride and vanity, boasts itself superior, exalts its own divinities, and looks with contempt on outsiders" (p. 13). For although the practice of seeing one's own group as the center of things is empirically correlated with a belief in its superiority, centrality and superiority need to be kept analytically distinct in order to deal with patterns of alienation from one's membership group and contempt for it.¹³

Supplementing the abundance of historical and ethnological evidence of the empirical tendency for belief in one's group or collectivity as superior to all cognate groups or collectivities—whether nation, class, race, region, or organization—is a recent batch of studies of what Theodore Caplow (1964, pp. 213–16) has called the aggrandizement effect: the distortion upward of the prestige of an organization by its members. Caplow examined 33 different kinds of organizations—ranging from dance studios to Protestant and Catholic churches, from skid row missions to big banks, and from advertising agencies to university departments—and found that members overestimated the prestige of their organization some "eight times as often as they underestimated it" (when compared with judgments by Outsiders). More in point for us, while members tended to disagree with Outsiders about the standing of their own organization, they tended to agree with them about the prestige of the other organizations in the same

¹³ By introducing their useful term "xenocentrism" to refer to both basic *and* favorable orientations to groups other than one's own, Kent and Burnight (1951) have retained Sumner's unuseful practice of prematurely combining centrality and evaluation in the one concept rather than keeping them analytically distinct. The analytical distinction can be captured terminologically by treating "xenocentrism" as the generic term, with the analytically distinct components of favorable orientation to nonmembership groups (as with the orientation of many white middle-class Americans toward blacks) being registered in the term "xenophilia" and the unfavorable orientation by Pareto's term "xenophobia." The growing theoretical interest in nonmembership reference groups (a concept implying a type of Outsider) (Hyman 1968; Merton and Rossi 1950) and the intensified spread of both ethnocentrism and xenocentrism in our times have given the term xenocentrism greater relevance than ever and yet, for obscure reasons, it has remained largely sequestered in the pages of the *American Journal of Sociology* where it first appeared 20 years ago. Caplow (1964, p. 216) and Horton (1965) are the only ones I know to have made good use of the term, but their uncustomed behavior only emphasizes its more general neglect.

set. These findings can be taken as something of a sociological parable. In these matters at least, the judgments of "Insiders" are best trusted when they assess groups other than their own; that is, when members of groups judge as Outsiders rather than as Insiders.

Findings of this sort do not testify, of course, that ethnocentrism and its frequent spiritual correlate, xenophobia, fear and hatred of the alien, are incorrigible. They do, however, remind us of the widespread tendency to glorify the ingroup, sometimes to that degree in which it qualifies as chauvinism: the extreme, blind, and often bellicose extolling of one's group, status, or collectivity. We need not abandon "chauvinism" as a concept useful to us here merely because it has lately become adopted as a vogue word, blunted in meaning through indiscriminate use as a rhetorical weapon in intergroup conflict. Nor need we continue to confine the scope of the concept, as it was in its origins and later by Lasswell (1937, p. 361) in his short, incisive discussion of it, to the special case of the *state or nation*. The concept can be usefully, not tendentiously, extended to designate the extreme glorification of *any* social formation.

Chauvinism finds its fullest ideological expression when groups are subject to the stress of acute conflict. Under the stress of war, for example, scientists have been known to violate the values and norms of universalism in which they were socialized, allowing their status as nationals to dominate over their status as scientists. Thus, at the outset of World War I, almost a hundred German scholars and scientists—including many of the first rank, such as Brentano, Ehrlich, Haber, Eduard Meyer, Ostwald, Planck, and Schmoller—could bring themselves to issue a manifesto that impugned the contributions of the enemy to science, charging them with nationalistic bias, logrolling, intellectual dishonesty and, when you came right down to it, the absence of truly creative capacity. The English and French scientists were not far behind in advertising their own brand of chauvinism.¹⁴

Ethnocentrism, then, is not a historical constant. It becomes intensified under specifiable conditions of acute social conflict. When a nation, race, ethnic group, or any other powerful collectivity has long extolled its own admirable qualities and, expressly or by implication, deprecated the qualities of others, it invites and provides the potential for counterethnocentrism. And when a once largely powerless collectivity acquires a socially validated sense of growing power, its members experience an intensified need for self-affirmation. Under such circumstances, collective self-glorifi-

¹⁴ Current claims of Insiderism still have a distance to go, in the academic if not the political forum, to match the chauvinistic claims of those days. For collections of such documents, see Pettit and Leudet (1916), Duhem (1915), Kellermann (1915), Kherkhof (1933).

cation, found in some measure among all groups, becomes a predictable and intensified counterresponse to long-standing belittlement from without.¹⁵

So it is that, in the United States, the centuries-long institutionalized premise that "white (and for some, presumably only white) is true and good and beautiful" induces, under conditions of revolutionary change, the counterpremise that "black (and for some, presumably only black) is true and good and beautiful." And just as the social system has for centuries operated on the tacit or explicit premise that in cases of conflict between whites and blacks, the whites are presumptively right, so there now develops the counterpremise, finding easy confirmation in the long history of injustice visited upon American Negroes, that in cases of such conflict today, the blacks are presumptively right.

What is being proposed here is that the epistemological claims of the Insider to monopolistic or privileged access to social truth develop under particular social and historical conditions. Social groups or strata on the way up develop a revolutionary élan. The new thrust to a larger share of power and control over their social and political environment finds various expressions, among them claims to a unique access to knowledge about their history, culture, and social life.

On this interpretation, we can understand why this Insider doctrine does not argue for a Black Physics, Black Chemistry, Black Biology, or Black Technology. For the new will to control their fate deals with the social environment, not the environment of nature. There is, moreover, nothing in the segregated life experience of Negroes that is said to sensitize them to the subject matters and problematics of the physical and life sciences. An Insider doctrine would have to forge genetic assumptions about racial modes of thought in order to claim, as in the case of the Nazi version they did claim, monopolistic or privileged access to knowledge in these fields of science. But the black Insider doctrine adopts an essentially social-environmental rationale, not a biologically genetic one.

The social process underlying the emergence of Insider doctrine is reasonably clear. Polarization in the underlying social structure becomes reflected in the polarization of claims in the intellectual and ideological domain, as groups or collectivities seek to capture what Heidegger called the "public interpretation of reality."¹⁶ With varying degrees of intent, groups in conflict want to make their interpretation the prevailing one of how things were and are and will be. The critical measure of success occurs when the interpretation moves beyond the boundaries of the ingroup to be

¹⁵ This is not a prediction after the fact. E. Franklin Frazier (1949, 1957) repeatedly made the general point and Merton (1968, p. 485) examined this pattern in connection with the self-fulfilling prophecy.

¹⁶ Heidegger (1927) as cited and discussed by Mannheim (1952, pp. 196 ff.).

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accepted by Outsiders. At the extreme, it then gives rise, through identifiable processes of reference-group behavior, to the familiar case of the converted Outsider validating himself, in his own eyes and in those of others, by becoming even more zealous than the Insiders in adhering to the doctrine of the group with which he wants to identify himself, if only symbolically (Merton 1963, pp. 405-6). He then becomes more royalist than the king, more papist than the pope. Some white social scientists, for example, vicariously and personally guilt ridden over centuries of white racism, are prepared to outdo the claims of the group they would symbolically join. They are ready even to surrender their hard-won expert knowledge if the Insider doctrine seems to require it. This type of response was perhaps epitomized in a televised educational program in which the white curator of African ethnology at a major museum engaged in discussion with a black who, as it happens, had had no prolonged ethnological training. All the same, at a crucial juncture in the public conversation, the distinguished ethnologist could be heard to say: "I realize, of course, that I cannot begin to understand the black experience, in Africa or America, as you can. Won't you tell our audience about it?" Here, in the spontaneity of an unrehearsed public discussion, the Insider doctrine has indeed become the public interpretation of reality.

The black Insider doctrine links up with the historically developing social structure in still another way. The dominant social institutions in this country have long treated the racial identity of individuals as actually if not doctrinally relevant to all manner of situations in every sphere of life. For generations, neither blacks nor whites, though with notably differing consequences, were permitted to forget their race. *This treatment of a social status (or identity) as relevant when intrinsically it is functionally irrelevant constitutes the very core of social discrimination.* As the once firmly rooted systems of discriminatory institutions and prejudicial ideology began to lose their hold, this meant that increasingly many judged the worth of ideas on their merits, not in terms of their racial pedigree.

What the Insider doctrine of the most militant blacks proposes on the level of social structure is to adopt the salience of racial identity in every sort of role and situation, a pattern so long imposed upon the American Negro, and to make that identity a total commitment issuing from within the group rather than one imposed upon it from without. By thus affirming the universal saliency of race and by redefining race as an abiding source of pride rather than stigma, the Insider doctrine in effect models itself after doctrine long maintained by white racists.

Neither this component of the Insider doctrine nor the statement on its implications is at all new. Almost a century ago, Frederick Douglass (1966) hinged his observations along these lines on the distinction between collective and individual self-images based on ascribed and achieved status:

One of the few errors to which we are clinging most persistently and, as I think, most mischievously has come into great prominence of late. It is the cultivation and stimulation among us of a sentiment which we are pleased to call race pride. I find it in all our books, papers, and speeches. For my part I see no superiority or inferiority in race or color. Neither the one nor the other is a proper source of pride or complacency. Our race and color are not of our own choosing. We have no volition in the case one way or another. The only excuse for pride in individuals or races is in the fact of their own achievements. . . . I see no benefit to be derived from this everlasting exhortation of speakers and writers among us to the cultivation of race pride. On the contrary, I see in it a positive evil. It is building on a false foundation. Besides, what is the thing we are fighting against, and what are we fighting for in this country? What is the mountain devil, the lion in the way of our progress? What is it, but American race pride; an assumption of superiority upon the ground of race and color? Do we not know that every argument we make, and every pretension we set up in favor of race pride is giving the enemy a stick to break over our heads?

In rejecting the cause of racial chauvinism, Douglass addressed the normative rather than the cognitive aspect of Insiderism. The call to total commitment requiring one group loyalty to be unquestionably paramount is most apt to be heard when the particular group or collectivity is engaged in severe conflict with others. Just as conditions of war between nations have long produced a strain toward hyperpatriotism among national ethnocentrics, so current intergroup conflicts have produced a strain toward hyperloyalty among racial or sex or age or religious ethnocentrics. Total commitment easily slides from the solidarity doctrine of "our group, right or wrong" to the morally and intellectually preemptive doctrine of "our group, always right, never wrong."

Turning from the normative aspect, with its ideology exhorting prime loyalty to this or that group, to the cognitive, specifically epistemological aspect, we note that the Insider doctrine presupposes a particular imagery of social structure.

SOCIAL STRUCTURE OF INSIDERS AND OUTSIDERS

From the discussion thus far, it should be evident that I adopt a structural conception of Insiders and Outsiders. In this conception, Insiders are the members of specified groups and collectivities or occupants of specified social statuses; Outsiders are the nonmembers.¹⁷ This structural concept comes closer to Sumner's usage in his *Folkways* than to various meanings assigned the Outsider by Nietzsche, Kierkegaard, Sartre, Camus (1946)

¹⁷ This is not the place to go into the theoretical problems of identifying the boundaries of groups, the criteria of group membership, and the consequent varieties of members and nonmembers. For an introduction to the complexities of these concepts, see Merton (1968, pp. 338-54, 405-7).

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or, for that matter, by Colin Wilson (1956) just as, to come nearer home, it differs from the usages adopted by Riesman, Denny, and Glazer (1950), Price (1965, pp. 83–84), or Howard S. Becker (1963). That is to say, Insiders and Outsiders are here defined as categories in social structure, not as inside dopesters or the specially initiated possessors of esoteric information on the one hand and as social-psychological types marked by alienation, rootlessness, or rule breaking, on the other.

In structural terms, we are all, of course, both Insiders and Outsiders, members of some groups and, sometimes derivatively, not of others; occupants of certain statuses which thereby exclude us from occupying other cognate statuses. Obvious as this basic fact of social structure is, its implications for Insider and Outsider epistemological doctrines are apparently not nearly as obvious. Else, these doctrines would not presuppose, as they typically do, that human beings in socially differentiated societies can be sufficiently located in terms of a single social status, category, or group affiliation—black or white, men or women, under 30 or older—or of several such categories, taken seriatim rather than conjointly. This neglects the crucial fact of social structure that individuals have not a single status but a status set: a complement of variously interrelated statuses which interact to affect both their behavior and perspectives.

The structural fact of status sets, in contrast to statuses taken one at a time, introduces severe theoretical problems for total Insider (and Outsider) doctrines of social epistemology. The array of status sets in a population means that aggregates of individuals share some statuses and not others; or, to put this in context, that they typically confront one another simultaneously as Insiders and Outsiders. Thus, if only whites can understand whites and blacks, blacks, and only men can understand men, and women, women, this gives rise to the paradox which severely limits both premises: for it then turns out, by assumption, that some Insiders are excluded from understanding other Insiders with white women being condemned not to understand white men, and black men, not to understand black women,¹⁸ and so through the various combinations of status subsets.

Structural analysis in terms of shared and mutually exclusive status sets will surely not be mistaken either as advocating divisions within the ranks of collectivities defined by a single prime criterion or as predicting that such collectivities cannot unite on many issues, despite their internal

¹⁸ The conflicts periodically reported by black women—for example, the debate between Mary Mebane [Liza] and Margaret Sloan (in defense of Gloria Steinem)—between identification with black liberation and the women's liberation movement, reflect this sociological fact of crosscutting status sets. The problem of coping with these structurally induced conflicts is epitomized in Margaret Sloan's (1971) "realization that I was going to help the brothers realize that as black women we cannot allow black men to do [to] us what white men have been doing to their women all these years."

divisions. Such analysis only indicates the bases of social divisions that stand in the way of enduring unity of any of the collectivities and so must be coped with, divisions that are not easily overcome as new issues activate statuses with diverse and often conflicting interests. Thus, the obstacles to a union of women in England and North Ireland resulting from national, political, and religious differences between them are no less formidable than the obstacles, noted by Marx, confronting the union of English and Irish proletarians. So, too, women's liberation movements seeking unity in the United States find themselves periodically contending with the divisions between blacks and whites within their ranks, just as black liberation movements seeking unity find themselves periodically contending with the divisions between men and liberated women within their ranks (Chisholm 1970; LaRue 1970).

The problem of achieving unity in large social movements based on any one status when its members are differentiated by crosscutting status sets is epitomized in these words about women's liberation by a black woman where identification with race is dominant: "Of course there have been women who have been able to think better than they've been trained and have produced the canon of literature fondly referred to as 'feminist literature': Anais Nin, Simone de Beauvoir, Doris Lessing, Betty Friedan, etc. And the question for us arises: how relevant are the truths, the experiences, the findings of white women to Black women? Are women after all simply women? I don't know that our priorities are the same, that our concerns and methods are the same, or even similar enough so that we can afford to depend on this new field of experts (white, female). It is rather obvious that we do not. It is obvious that we are turning to each other" (Cade 1970, p. 9).

Correlatively, the following passage epitomizes the way in which internal differentiation works against unity of the black liberation movement where dominant identification with sex status is reinforced by further educational differentiation:

Seems to me the Brother does us all a great disservice by telling her to fight the man with the womb. Better to fight with the gun and the mind. . . . The all too breezy no-pill/have-kids/mess-up-the-man's-plan notion these comic-book-loving Sisters find so exciting is very seductive because it's a clear-cut and easy thing for her to do for the cause since it nourishes her sense of martyrdom. If the thing is numbers merely, what the hell. But if we are talking about revolution, creating an army for today and tomorrow, I think the Brothers who've been screaming these past years had better go do their homework. [Cade 1970, pp. 167-68]

The internal differentiation of collectivities based on a single status thus provides structural bases for diverse and often conflicting intellectual and moral perspectives within such collectivities. Differences of religion or

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age or class or occupation work to divide what similarities of race or sex or nationality work to unite. That is why social movements of every variety that strive for unity—whether they are establishmentarian movements whipped up by chauvinistic nationals in time of war or antiestablishmentarian movements designed to undo institutionalized injustice—press for total commitments in which all other loyalties are to be subordinated, on demand, to the dominant one.

This symptomatic exercise in status-set analysis may be enough to indicate that the idiomatic expression of total Insider doctrine—one must be one in order to understand one—is deceptively simple and sociologically fallacious (just as we shall see is the case with the total Outsider doctrine). For, from the sociological perspective of the status set, “one” is not a man *or* a black *or* an adolescent *or* a Protestant, *or* self-defined and socially defined as middle class, and so on. Sociologically, “one” is, of course, all of these and, depending on the size of the status set, much more. Furthermore, as Simmel (1908, pp. 403–54; Coser 1965, pp. 18–20) taught us long ago, the individuality of human beings can be sociologically derived from social differentiation and not only psychologically derived from intrapsychic processes. Thus, the greater the number and variety of group affiliations and statuses distributed among individuals in a society, the smaller, on the average, the number of individuals having precisely the same social configuration.

Following out the implications of this structural observation, we note that, on its own assumptions, the total Insider doctrine should hold only for highly fragmented small aggregates sharing the same status sets. Even a truncated status set involving only three affiliations—WASPS, for example—would greatly reduce the number of people who, under the Insider principle, would be able to understand their fellows (WASPS). The numbers rapidly decline as we attend to more of the shared status sets by including such social categories as sex, age, class, occupation, and so on, toward the limiting case in which the unique occupant of a highly complex status set is alone qualified to achieve an understanding of self. The tendency toward such extreme social atomization is of course damped by differences in the significance of statuses which vary in degrees of dominance, saliency, and centrality.¹⁹ As a result, the fragmentation of the capacity for understanding that is implied in the total Insider doctrine will not empirically reach this extreme. The structural analysis in terms of status sets, rather than in the fictional terms of individuals being identi-

¹⁹ This is not the place to summarize an analysis of the dynamics of status sets that takes up variation in key statuses (dominant, central, salient) and the conditions under which various statuses tend to be activated, along lines developed in unpublished lectures by Merton (1955–71). For pertinent uses of these conceptions in the dynamics of status sets, particularly with regard to functionally irrelevant statuses, see Epstein (1970, esp. chap. 3).

fied in terms of single statuses, serves only to push the logic of Insiderism to its ultimate methodological solipsism.

The fact of structural and institutional differentiation has other kinds of implications for the effort to translate the Insider claim to solidarity into an Insider epistemology. Since we all occupy various statuses and have group affiliations of varying significance to us, since, in short, we individually link up with the differentiated society through our status sets, this runs counter to the abiding and exclusive primacy of any one group affiliation. Differing situations activate different statuses which then and there dominate over the rival claims of other statuses.

This aspect of the dynamics of status sets can also be examined from the standpoint of the differing margins of functional autonomy possessed by various social institutions and other social subsystems. Each significant affiliation exacts loyalty to values, standards, and norms governing the given institutional domain, whether religion, science, or economy. Sociological thinkers such as Marx and Sorokin, so wide apart in many of their other assumptions, agree in assigning a margin of autonomy to the sphere of knowledge²⁰ even as they posit their respective social, economic, or cultural determinants of it. The alter ego of Marx, for example, declares the partial autonomy of spheres of thought in a well-known passage that bears repetition here:

According to the materialist conception of history the determining element in history is *ultimately* the production and reproduction in real life. More than this neither Marx nor I have ever asserted. If therefore somebody twists this into the statement that the economic element is the *only* determining one, he transforms it into a meaningless, abstract and absurd phrase. The economic situation is the basis, but the various elements of the superstructure—political forms of the class struggle and its consequences, constitutions established by the victorious class after a successful battle, etc.—forms of law—and then even the reflexes of all these actual struggles in the brains of the combatants: political, legal, philosophical theories, religious ideas and their further development into systems of dogma—also exercise their influence upon the course of the historical struggles and in many cases preponderate in determining their *form*. There is an interaction of all these elements in which . . . the economic movement finally asserts itself as necessary. Otherwise the application of the theory to any period of history one chooses would be easier than the solution of a simple equation of the first degree. [Engels 1936, p. 381; see also p. 392]

We can see structural differentiation and institutional autonomy at work in current responses of scholars to the extreme Insider doctrine. They

²⁰ For a detailed discussion of the partial autonomy of subsystems in the conceptions of Marx and Sorokin, see Merton and Barber (1963, pp. 343–49; Merton 1968, pp. 521 ff.). On the general notion of functional autonomy as advanced by Gordon W. Allport in psychology, see the discussion and references in Merton (1968, pp. 15–16); on functional autonomy in sociology, see Gouldner (1958, 1959).

reject the monopolistic doctrine of the Insider that calls for total ideological loyalty in which efforts to achieve scholarly detachment and objectivity become redefined as renegadism just as ideological reinforcement of collective self-esteem becomes redefined as the higher objectivity. It is here, to continue with our case in point, that Negro scholars who retain their double loyalty—to the race and to the values and norms of scholarship—part company with the all-encompassing loyalty demanded by the Insider doctrine. Martin Kilson (1969), for example, repudiates certain aspects of the doctrine and expresses his commitment to both the institutionalized values of scholarship and to the black community in these words:

I am opposed to proposals to make Afro-American studies into a platform for a particular ideological group, and to restrict these studies to Negro *students and teachers*. For, and we must be frank about this, what this amounts to is racism in reverse—black racism. I am certainly convinced that it is important for the Negro to know of his past—of his ancestors, of their strengths and weaknesses—and they should respect this knowledge, when it warrants respect, and they should question it and criticize it, when it deserves criticism. But it is of no advantage to a mature and critical understanding or appreciation of one's heritage if you approach that heritage with the assumption that it is intrinsically good and noble, and intrinsically superior to the heritage of other peoples. That is, after all, what white racists have done; and none of my militant friends in the black studies movement have convinced me that racist thought is any less vulgar and degenerate because it is used by black men. . . . What I am suggesting here is that the serious study of the heritage of any people will produce a curious mixture of things to be proud of, things to criticize and even despise and things to be perpetually ambivalent toward. And this is as it should be: only an ideologically oriented Afro-American studies program, seeking to propagate a packaged view of the black heritage, would fail to evoke in a student the curious yet fascinating mixture of pride, criticism and ambivalence which I think *is, or ought to be the product of serious intellectual and academic activity*. [Pp. 329–30; italics added]

Along with the faults of neglecting the implications of structural differentiation, status sets, and institutional autonomy, the Insider (and comparable Outsider) doctrine has the further fault of assuming, in its claims of monopolistic or highly privileged status-based access to knowledge, that social position wholly determines intellectual perspectives. In doing so, it affords yet another example of the ease with which truths can decline into error merely by being extended well beyond the limits within which they have been found to hold. (There *can* be too much of a good thing.)

A long-standing conception shared by various "schools" of sociological thought holds that differences in the social location of individuals and groups tend to involve differences in their interests and value orientations

(as well as the sharing of some interests and values with others). Certain traditions in the sociology of knowledge have gone on to assume that these structurally patterned differences should involve, on the *average*, patterned differences in perceptions and perspectives. And these, so the convergent traditions hold—their convergence being often obscured by diversity in vocabulary rather than in basic concept—should make for discernible differences, on the average, in the definitions of problems for inquiry and in the types of hypotheses taken as points of departure. So far, so good. The evidence is far from in, since it has also been a tradition in the sociology of scientific knowledge during the greater part of the past century to prefer speculative theory to empirical inquiry. But the idea, which can be taken as a general orientation guiding such inquiry, is greatly transformed in Insider doctrine.

For one thing, that doctrine assumes total coincidence between social position and individual perspectives. It thus exaggerates into error the conception of structural analysis which maintains that there is a *tendency for, not a full determination of*, socially patterned differences in the perspectives, preferences, and behavior of people variously located in the social structure. The theoretical emphasis on tendency, as distinct from total uniformity, is basic, not casual or niggling. It provides for a range of variability in perspective and behavior among members of the same groups or occupants of the same status (differences which, as we have seen, are ascribable to social as well as psychological differentiation). At the same time, this structural conception also provides for patterned differences, *on the whole*, between the perspectives of members of different groups or occupants of different statuses. Structural analysis thus avoids what Dennis Wrong (1961) has aptly described as “the oversocialized conception of man in modern sociology.”²¹

²¹ Wrong's paper is an important formulation of the theoretical fault involved in identifying structural position with individual behavior. But, in some cases, he is preaching to the long since converted. It is a tenet in some forms of structural analysis that differences in social location *make for* patterned differences in perspectives and behavior *between* groups while still allowing for a range of variability *within* groups and thus, in structurally proximate groups, for considerably overlapping ranges of behavior and perspective. On the general orientation of structural analysis in sociology, see Barbano (1968); for some specific terminological clues to the fundamental distinction between social position and actual behavior or perspective as this is incorporated in structural analysis, see Merton (1968, *passim*) for the key theoretical expressions that “*structures exert pressures*” and structures “*tend*” to generate perspectives and behaviors. For specific examples: “people in the various occupations *tend* to take different parts in the society, to have different shares in the exercise of power, both acknowledged and unacknowledged, and to *see* the world differently” (p. 180). “Our primary aim is to discover how some *social structures exert a definite pressure upon certain persons in the society to engage in nonconforming rather than conforming conduct*. If we can locate groups peculiarly subject to such pressures, we should expect to find fairly high rates of deviant behavior in those groups” (p. 186). And for immediate rather than general theoretical bearing on the specific problems here under review, see Merton

Important as such allowance for individual variability is for general structural theory, it has particular significance for a sociological perspective on the life of the mind and the advancement of science and learning. For it is precisely the individual differences among scientists and scholars that are often central to the development of the discipline. They often involve the differences between good scholarship and bad; between imaginative contributions to science and pedestrian ones; between the consequential ideas and stillborn ones. In arguing for the monopolistic access to knowledge, Insider doctrine can make no provision for individual variability that extends beyond the boundaries of the ingroup which alone can develop sound and fruitful ideas.

Insofar as Insider doctrine treats ascribed rather than achieved statuses as central in forming perspectives, it tends to be static in orientation. For with the glaring exception of age status itself, ascribed statuses are generally retained throughout the life span. Yet sociologically, there is nothing fixed about the boundaries separating Insiders from Outsiders. As situations involving different values arise, different statuses are activated and the lines of separation shift. Thus, for a large number of white Americans, Joe Louis was a member of an outgroup. But when Louis defeated the Nazified Max Schmeling, many of the same white Americans promptly redefined him as a member of the (national) ingroup. National self-esteem took precedence over racial separatism. That this sort of drama in which changing situations activate differing statuses in the status set is played out in the domain of the intellect as well is the point of Einstein's ironic observation in an address at the Sorbonne: "If my theory of relativity is proven successful, Germany will claim me as a German and France will declare that I am a citizen of the world. Should my theory prove untrue, France will say that I am a German and Germany will declare that I am a Jew."²²

Like earlier conceptions in the sociology of knowledge, recent Insider

(1957): "In developing this view, I do not mean to imply that scientists, any more than other men [and women] are merely obedient puppets doing exactly what social institutions require of them. But I do mean to say that, like men [and women] in other institutional spheres, scientists tend to develop the values and to channel their motivations in directions the institution defines for them" (p. 640).

²² On the general point of shifting boundaries, see Merton (1968, pp. 338-42, 479-80). Einstein was evidently quite taken with the situational determination of shifts in group boundaries. In a statement written for the London *Times* at a time (November 28, 1919) when the animosities of World War I were still largely intact, he introduced slight variations on the theme: "The description of me and my circumstances in the *Times* shows an amusing flare of imagination on the part of the writer. By an application of the theory of relativity to the taste of the reader, today in Germany I am called a German man of science and in England I am represented as a Swiss Jew. If I come to be regarded as a '*bête noire*' the description will be reversed, and I shall become a Swiss Jew for the German and a German for the English" (Frank 1963, p. 144).

doctrines maintain that, in the end, it is a special category of Insider—a category that generally manages to include the proponent of the doctrine—that has sole or privileged access to knowledge. Mannheim (1936, pp. 10, 139, 232), for example, found a structural warranty for the validity of social thought in the “classless position” of the “socially unattached intellectuals” (*sozialfreischwebende Intelligenz*). In his view, these intellectuals can comprehend the conflicting tendencies of the time since, among other things, they are “recruited from constantly varying social strata and life-situations.” (This is more than a little reminiscent of the argument in the *Communist Manifesto* which emphasizes that “the proletariat is recruited from all classes of the population.”)²³ Without stretching this argument to the breaking point, it can be said that Mannheim in effect claims that there is a category of socially free-floating intellectuals who are both Insiders and Outsiders. Benefiting from their collectively diverse social origins and transcending group allegiances, they can observe the social universe with special insight and a synthesizing eye.

INSIDERS AS “OUTSIDERS”

In an adaptation of this same kind of idea, what some Insiders profess as Insiders they apparently reject as Outsiders. For example, when advocates of black Insider doctrine engage in analysis of “white society,” trying to assay its power structure or to detect its vulnerabilities, they seem to deny in practice what they affirm in doctrine. At any rate, their behavior testifies to the assumption that it is possible for self-described “Outsiders” to diagnose and to understand what they describe as an alien social structure and culture.

This involves the conception that there is a special category of people in the system of social stratification who have distinctive, if not exclusive, perceptions and understanding in their capacities as *both* Insiders and Outsiders. We need not review again the argument for special access to knowledge that derives from being an Insider. What is of interest here is the idea that special perspectives and insights are available to that category of Outsiders who have been systematically frustrated by the social system: the disinherited, deprived, disenfranchised, dominated, and exploited Outsiders. Their run of experience in trying to cope with these problems serves to sensitize them—and in a more disciplined way, the trained social scientists among them—to the workings of the culture and social structure that are more apt to be taken for granted by Insider social scientists drawn from social strata who have either benefited from the going social system or have not greatly suffered from it.

²³ For further discussion of the idea of social structural warranties of validity, see Merton (1968, pp. 560–62).

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This reminder that Outsiders are not all of a kind and the derived hypothesis in the sociology of knowledge about socially patterned differences in perceptiveness is plausible and deserving of far more systematic investigation than it has received. That the white-dominated society has long imposed social barriers which excluded Negroes from anything remotely like full participation in that society is now known to even the more unobservant whites. But what many of them have evidently not noticed is that the high walls of segregation do not at all separate whites and blacks symmetrically from intimate observation of the social life of the other. As socially invisible men and women, blacks at work in white enclaves have for centuries moved through or around the walls of segregation to discover with little effort what was on the other side. This was tantamount to their having access to a one-way screen. In contrast, the highly visible whites characteristically did not want to find out about life in the black community and could not, even in those rare cases where they would. The structure of racial segregation meant that the whites who prided themselves on "understanding" Negroes knew little more than their stylized role behaviors in relation to whites and next to nothing of their private lives. As Arthur Lewis has noted, something of the same sort still obtains with the "integration" of many blacks into the larger society during the day coupled with segregation at night as blacks and whites return to their respective ghettos. In these ways, segregation can make for asymmetrical sensitivities across the divide.

Although there is a sociological tradition of reflection and research on marginality in relation to thought, sociologists have hardly begun the hard work of seriously investigating the family of hypotheses in the sociology of knowledge that derive from this conception of asymmetrical relations between diverse kinds of Insiders and Outsiders.

OUTSIDER DOCTRINE AND PERSPECTIVES

The strong version of the Insider doctrine, with its epistemological claim to a monopoly of certain kinds of knowledge, runs counter, of course, to a long history of thought. From the time of Francis Bacon, to reach back no further, students of the intellectual life have emphasized the corrupting influence of group loyalties upon the human understanding. Among Bacon's four Idols (or sources of false opinion), we need only recall the second, the Idol of the Cave. Drawing upon Plato's allegory of the cave in the *Republic*, Bacon undertakes to tell how the immediate social world in which we live seriously limits what we are prepared to perceive and how we perceive it. Dominated by the customs of our group, we maintain received opinions, distort our perceptions to have them accord with these opinions, and are thus held in ignorance and led into error which we

parochially mistake for the truth. Only when we escape from the cave and extend our visions do we provide for access to authentic knowledge. By implication, it is through the iconoclasm that comes with changing group affiliations that we can destroy the Idol of the Cave, abandon delusory doctrines of our own group, and enlarge our prospects for reaching the truth. For Bacon, the dedicated Insider is peculiarly subject to the myopia of the cave.

In this conception, Bacon characteristically attends only to the dysfunctions of group affiliation for knowledge. Since for him access to authentic knowledge requires that one abandon superstition and prejudice, and since these stem from groups, it would not occur to Bacon to consider the possible functions of social locations in society as providing for observability and access to particular kinds of knowledge.

In a far more subtle style, the founding fathers of sociology in effect also argued against the strong form of the Insider doctrine *without turning to the equal and opposite error of advocating the strong form of the Outsider doctrine* (which would hold that knowledge about groups, unprejudiced by membership in them, is accessible only to outsiders).

The ancient epistemological problem of subject and object was taken up in the discussion of historical *Verstehen*. Thus, first Simmel and then, repeatedly, Max Weber symptomatically adopted the memorable phrase: "one need not be Caesar in order to understand Caesar."²⁴ In making this claim, they rejected the extreme Insider thesis which asserts in effect that one *must* be Caesar in order to understand him just as they rejected the extreme Outsider thesis that one *must not* be Caesar in order to understand him.

The observations of Simmel and Weber bear directly upon implications of the Insider doctrine that reach beyond its currently emphasized scope.

²⁴ Thanks to Donald N. Levine (1971, p. xxiii), I learn that in often attributing the aphorism, with its many implications for social epistemology, to Weber, I had inadvertently contributed to a palimpsestic syndrome: assigning a striking idea or formulation to the author who first introduced us to it when in fact that author had simply adopted or revived a formulation that he (and others versed in the same tradition) knew to have been created by another. As it happens, I first came upon the aphorism in Weber's basic paper on the categories of a *verstehende* sociology published in 1913. In that passage, he treats the aphorism as common usage which he picks up for his own analytical purposes: "Man muss, wie oft gesagt worden ist, 'nicht Cäsar sein, um Cäsar zu verstehen.'" Alerted by Levine's note, I now find that Weber made earlier use of the aphorism back in 1903-6 (1951, pp. 100-101) as he drew admiringly upon Simmel's *Probleme der Geschichtsphilosophie* to which he attributes the most thoroughly developed beginnings of a theory of *Verstehen*. Properly enough, Weber devotes a long, long note to the general implications of Simmel's use of the aphorism, quoting it just as we have seen but omitting the rest of Simmel's embellished version: "Und kein zweiter Luther, um Luther zu begreifen." In his later work, Weber incorporated the aphorism whenever he examined the problem of the "understandability" of the actions of others.

The Insider argues that the authentic understanding of group life can be achieved only by those who are directly engaged as members in the life of the group. Taken seriously, the doctrine puts in question the validity of just about all historical writing, as Weber clearly saw ([1922] 1951, p. 428).²⁵ If direct engagement in the life of a group is essential to understanding it, then the only authentic history is contemporary history, written in fragments by those most fully involved in making inevitably limited portions of it. Rather than constituting only the raw materials of history, the documents prepared by engaged Insiders become all there is to history. But once the historian elects to write the history of a time other than his own, even the most dedicated Insider, of the national, sex, age, racial, ethnic, or religious variety, becomes the Outsider, condemned to error and misunderstanding.

Writing some 20 years ago in another connection, Claude Lévi-Strauss noted the parallelism between history and ethnography. Both subjects, he observed,

are concerned with societies *other* than the one in which we live. Whether this *otherness* is due to remoteness in time (however slight) or to remoteness in space, or even to cultural heterogeneity, is of secondary importance compared to the basic similarity of perspective. All that the historian or ethnographer can do, and all that we can expect of either of them, is to enlarge a specific experience to the dimensions of a more general one, which thereby becomes accessible *as experience* to men of another country or another epoch. And in order to succeed, both historian and ethnographer, must have the same qualities: skill, precision, a sympathetic approach and objectivity.²⁶

Our question is, of course, whether the qualities required by the historian and ethnographer as well as other social scientists are confined to or largely concentrated among Insiders or Outsiders. Simmel (1908), and after him, Schütz (1944), and others have pondered the roles of that incarnation of the Outsider, the stranger who moves on.²⁷ In a fashion oddly reminiscent of the anything-but-subtle Baconian doctrine, Simmel develops the thesis that the stranger, not caught up in commitments to the group, can more readily acquire the strategic role of the relatively objective inquirer. "He is freer, practically and theoretically," notes Simmel (1950), "he surveys conditions with less prejudice; his criteria for them

²⁵ Having quoted the Caesar aphorism, Weber goes on to draw the implication for historiography: "Sonst wäre alle Geschichtsschreibung sinnlos."

²⁶ The essay from which this is drawn was first published in 1949 and is reprinted in Lévi-Strauss (1963, p. 16).

²⁷ It is symbolically appropriate that Simmel should have been attuned to the role of the stranger as outsider. For as Lewis Coser (1965, pp. 29-39) has shown, Simmel's style of sociological work was significantly influenced by his role as "The Stranger in the Academy."

are more general and more objective ideals; he is not tied down in his action by habit, piety, and precedent" (pp. 404-5). Above all, and here Simmel departs from the simple Baconian conception, the objectivity of the stranger "does not simply involve passivity and detachment; it is a particular structure composed of distance and nearness, indifference and involvement." It is the stranger, too, who finds what is familiar to the group significantly unfamiliar and so is prompted to raise questions for inquiry less apt to be raised at all by Insiders.

As was so often the case with Simmel's seminal mind, he thus raised a variety of significant questions about the role of the stranger in acquiring sound and new knowledge, questions that especially in recent years have begun to be seriously investigated. A great variety of inquiries into the roles of anthropological and sociological fieldworkers have explored the advantages and limitations of the Outsider as observer.²⁸ Even now, it appears that the balance sheet for Outsider observers resembles that for Insider observers, both having their distinctive assets and liabilities.

Apart from the theoretical and empirical work examining the possibly distinctive role of the Outsider in social and historical inquiry, significant episodes in the development of such inquiry can be examined as "clinical cases" in point. Thus, it has been argued that in matters historical and sociological the prospects for achieving certain kinds of insights may actually be somewhat better for the Outsider. Soon after it appeared in 1835, Tocqueville's *Democracy in America* was acclaimed as a masterly work by "an accomplished foreigner." Tocqueville himself expressed the opinion that "there are certain truths which Americans can only learn from strangers." These included what he described as the tyranny of majority opinion and the particular system of stratification which even in that time involved a widespread preoccupation with relative status in the community that left "Americans so restless in the midst of their prosperity." (This is Tocqueville, not Galbraith, writing.) All the same, this most perceptive Outsider did not manage to transcend many of the deep-seated racial beliefs and myths he encountered in the United States of the time.

²⁸ Many of these inquiries explicitly take off from Simmel's imagery of the roles and functions of the stranger. From the large and fast-growing mass of publications on fieldwork in social science, I cite only a few that variously try to analyze the roles of the Outsider as observer and interpreter. From an earlier day dealing with "stranger value," see Oeser (1939), Nadel (1939), Merton (1947), and Paul (1953). For more recent work on the parameters of adaptation by strangers as observers, see especially the imaginative analysis by Nash (1963) and the array of papers detailing how the sex role of women anthropologists affected their access to field data (Golde 1970). On comparable problems of the roles of Insiders and Outsiders in the understanding of complex public bureaucracies, see the short, general interpretation by Merton (1945) and the comprehensive, detailed one by Frankel (1969).

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Having condemned the Anglo-Americans whose "oppression has at one stroke deprived the descendants of the Africans of almost all the privileges of humanity" (Tocqueville [1858] 1945, 1:332);

having described slavery as mankind's greatest calamity and having argued that the abolition of slavery in the North was "not for the good of the Negroes, but for that of the whites" (ibid., 1:360-61);

having identified the marks of "oppression" upon both the oppressed Indians and blacks *and* upon their white oppressors (ibid., vol. 1, chap. 18, *passim*);

having noted "the tyranny of the laws" designed to suppress the "unhappy blacks" in the states that had abolished slavery (ibid., 1:368);

having approximately noted the operation of the self-fulfilling prophecy in the remark that "to induce the whites to abandon the opinion they have conceived of the moral and intellectual inferiority of their former slaves, the Negroes must change; but as long as this opinion subsists, to change is impossible" (ibid., 1:358, n.);

having also approximated the idea of relative deprivation in the statement that "there exists a singular principle of relative justice which is very firmly implanted in the human heart. Men are much more forcibly struck by those inequalities which exist within the circle of the same class, than with those which may be remarked between different classes" (ibid., 1:373-74);

having made these observations and judgments, this talented Outsider nevertheless accepts the doctrine, relevant in his time, that racial inequalities "seem to be founded upon the immutable laws of nature herself" (ibid., 1:358-59); and, to stop the list of particulars here, assumes, as an understandable and inevitable rather than disturbing fact that "the Negro, who earnestly desires to mingle his race with that of the European, cannot effect it" (ibid., 1:335).²⁹

Without anachronistically asking, as a Whig historian might, for altogether prescient judgments from this Outsider who was, after all, recording his observations in the early 19th century, we can nevertheless note that the role of Outsider apparently no more guarantees emancipation from the myths of a collectivity than the role of the Insider guarantees full insight into its social life and beliefs.

What was in the case of Tocqueville an unplanned circumstance has since often become a matter of deliberate decision. Outsiders are sought out to observe social institutions and cultures on the premise that they are more apt to do so with detachment. Thus, in the first decade of this century, the Carnegie Foundation for the Advancement of Teaching, in

²⁹ Tocqueville also assumes that "fatal oppression" has resulted in the enslaved blacks becoming "devoid of wants," and that "plunged in this abyss of evils, [he] scarcely feels his own calamitous situation," coming to believe that "even the power of thought . . . [is] a useless gift of Providence" (1:333). Such observations on the dehumanizing consequences of oppression are remarkable for the time. As Oliver Cromwell Cox (1948) observes about part of this same passage, Tocqueville's point "still has a modicum of validity" (p. 369, n.).

its search for someone to investigate the condition of medical schools, reached out to appoint Abraham Flexner, after he had admitted never before having been inside a medical school. It was a matter of policy to select a total Outsider who, as it happened, produced the uncompromising Report which did much to transform the state of American medical education at the time.

Later, casting about for a scholar who might do a thoroughgoing study of the Negro in the United States, the Carnegie Corporation searched for an Outsider, preferably one, as they put it, drawn from a country of "high intellectual and scholarly standards but with no background or traditions of imperialism." These twin conditions of course swiftly narrowed the scope of the search. Switzerland and the Scandinavian countries alone seemed to qualify, with the quest ending, as we know, with the selection of Gunnar Myrdal. In the preface to *An American Dilemma*, Myrdal (1944, pp. xviii-xiv) reflected on his status as an Outsider who, in his words, "had never been subject to the strains involved in living in a black-white society" and who "as a stranger to the problem . . . has had perhaps a greater awareness of the extent to which human valuations everywhere enter into our scientific discussion of the Negro problem."

Reviews of the book repeatedly alluded to the degree of detachment from entangling loyalties that seemed to come from Myrdal's being an Outsider. J. S. Redding (1944), for one, observed that "as a European, Myrdal had no American sensibilities to protect. He hits hard with fact and interpretation." Robert S. Lynd (1944), for another, saw it as a prime merit of this Outsider that he was free to find out for himself "without any side glances as to what was politically expedient." And for a third, Frank Tannenbaum (1944) noted that Myrdal brought "objectivity in regard to the special foibles and shortcomings in American life. As an outsider, he showed the kind of objectivity which would seem impossible for one reared within the American scene." Even later criticism of Myrdal's work—for example, the comprehensive critique by Cox (1948, chap. 23)—does not attribute imputed errors in interpretation to his having been an Outsider.

Two observations should be made on the Myrdal episode. First, in the judgment of critical minds, the Outsider, far from being excluded from the understanding of an alien society, was able to bring needed perspectives to it. And second, that Myrdal, wanting to have both Insider and Outsider perspectives, expressly drew into his circle of associates in the study such Insiders, engaged in the study of Negro life and culture and of race relations, as E. Franklin Frazier, Arnold Rose, Ralph Bunche, Melville Herskovits, Otto Klineberg, J. G. St. Clair Drake, Guy B. Johnson, and Doxey A. Wilkerson.

It should be noted in passing that other spheres of science, technology,

and learning have accorded distinctive and often related roles to both the Insider and the Outsider (Zuckerman and Merton 1972, pp. 311-14). As long ago as the 17th century, Thomas Sprat, the historian of the Royal Society, for example, took it "as evident, that divers sorts of Manufactures have been given us by men who were not bred up in Trades that resembled those which they discover'd. I shall mention Three; that of Printing, [Gun]Powder, and the Bow-Dye." Sprat goes on to expand upon the advantages of the Outsider for invention, concluding with the less-than-science-based observation that "as in the Generation of Children, those are usually observ'd to be most sprightly, that are the stollen Fruits of an unlawful Bed; so in the Generations of the Brains, those are often the most vigorous, and witty, which men beget on other Arts, and not on their own" (Sprat 1959, pp. 391-93).

In our own time, Gilfillan (1935, p. 88) reported that the "cardinal inventions are due to men outside the occupation affected, and the minor, perfective inventions to insiders." And in a recent and more exacting inquiry, Joseph Ben-David (1960) found that the professionalization of scientific research "does not in itself decrease the chances of innovation by outsiders to the various fields of science." For the special case of outsiders to a particular discipline, Max Delbrück (1963, p. 13), himself a founding father of molecular biology, notes that although "nuclear physics was developed almost exclusively within the framework of academic institutes at universities, molecular biology, in contrast, is almost exclusively a product of outsiders, of chemists, physicists, medical microbiologists, mathematicians and engineers."

The cumulative point of this variety of intellectual and institutional cases is not—and this needs to be repeated with all possible emphasis—is *not* a proposal to replace the extreme Insider doctrine by an extreme and equally vulnerable Outsider doctrine. The intent is, rather, to transform the original question altogether. We no longer ask whether it is the Insider or the Outsider who has monopolistic or privileged access to social truth; instead, we begin to consider their distinctive and interactive roles in the process of truth seeking.

INTERCHANGE, TRADE OFFS, AND SYNTHESSES

The actual intellectual interchange between Insiders and Outsiders—in which each adopts perspectives from the other—is often obscured by the rhetoric that commonly attends intergroup conflict. Listening only to that rhetoric, we may be brought to believe that there really is something like antithetical "black knowledge" and "white knowledge," "man's knowledge" and "woman's knowledge," etc., of a sort that allows no basis for

judging between their differing claims to knowledge. Yet the boundaries between Insiders and Outsiders tend to be far more permeable than this allows. Just as with the process of competition generally, so with the competition of ideas. Competing or conflicting groups take over ideas and procedures from one another, thereby denying in practice the rhetoric of total incompatibility. Even in the course of social polarization, conceptions with cognitive value are utilized all apart from their source. Concepts of power structure, co-optation, the dysfunctions of established institutions and findings associated with these concepts have for some time been utilized by social scientists, irrespective of their social or political identities. Nathan Hare (1967), for example, who remains one of the most articulate exponents of the Insider doctrine, made use of the notion of the self-fulfilling prophecy in trying to explain how it is that organizations run by blacks find it hard to work out.³⁰ As he put it, "White people thought that we could not have any institutions which were basically black which were of good quality. This has the effect of a self-fulfilling prophecy, because if you think that black persons cannot possibly have a good bank, then you don't put your money in it. All the best professors leave black universities to go to white universities as soon as they get the chance. The blacks even do the same thing. And this makes your prediction, which wasn't true in the beginning, come out to be true" (p. 65). Such diffusion of ideas across the boundaries of groups and statuses has long been noted. In one of his more astute analyses, Mannheim (1952) states the general case for the emergence and spread of knowledge that transcends even profound conflicts between groups:

Syntheses owe their existence to the same social process that brings about polarization; groups take over the modes of thought and intellectual achievements of their adversaries under the simple law of 'competition on the basis of achievement.' . . . In the socially-differentiated thought process, even the opponent is ultimately forced to adopt those categories and forms of thought which are most appropriate in a given type of world order. In the economic sphere, one of the possible results of competition is that one competitor is compelled to catch up with the other's technological advances. In just the same way, whenever groups compete for having their interpretation of reality accepted as the correct one, it may happen that one of the groups takes over from the adversary some fruitful hypothesis or category—anything that promises cognitive gain. . . . [In due course, it becomes

³⁰ Elsewhere, Hare treats certain beliefs of "Negro dignitaries" as a self-fulfilling prophecy (1970, p. 44). A recent work (Hole and Levine 1971) on women's liberation movements, both new and old, also observes: "Feminists argue further that there is a self-fulfilling prophecy component: when one group dominates another, the group with power is, at best, reluctant to relinquish its control. Thus in order to keep woman in 'her place,' theories are propounded which presume that her place is defined by nature" (p. 193).

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possible] to find a position from which both kinds of thought can be envisaged in their partial correctness, yet at the same time also interpreted as subordinate aspects of a higher synthesis. [Pp. 221-23]

The essential point is that, with or without intent, the process of intellectual exchange takes place precisely because the conflicting groups are in interaction. The extreme Insider doctrine, for example, affects the thinking of sociologists, black and white, who reject its extravagant claims. Intellectual conflict sensitizes them to aspects of their subject that they have otherwise not taken into account.

Social Sadism and Sociological Euphemism

As a case in point of this sort of sensitization through interaction, I take what can be described as a composite pattern of social sadism and sociological euphemism. "Social sadism" is more than a metaphor. The term refers to social structures which are so organized as to systematically inflict pain, humiliation, suffering, and deep frustration upon particular groups and strata. This need have nothing at all to do with the psychic propensities of individuals to find pleasure in cruelty. It is an objective, socially organized, and recurrent set of situations that has these cruel consequences, however diverse its historical sources and whatever the social processes that maintain it.

This type of sadistic social structure is readily overlooked by a perspective that can be described as that of the sociological euphemism. This term does not refer to the obvious cases in which ideological support of the structure is simply couched in sociological language. Rather, it refers to the kind of conceptual apparatus that, once adopted, requires us to ignore such intense human experiences as pain, suffering, humiliation, and so on. In this context, analytically useful concepts such as social stratification, social exchange, reward system, dysfunction, symbolic interaction, etc., are altogether bland in the fairly precise sense of being unperturbing, suave, and soothing in effect. To say this is not to imply that the conceptual repertoire of sociology (or of any other social science) must be purged of such impersonal concepts and filled with sentiment-laden substitutes. But it should be noted that analytically useful as these impersonal concepts are for certain problems, they also serve to exclude from the attention of the social scientist the intense feelings of pain and suffering that are the experience of some people caught up in the social patterns under examination. By screening out these profoundly human experiences, they become sociological euphemisms.

Nor is there any easy solution to the problem of sociological euphemism. True, we have all been warned off the Whiteheadian fallacy of misplaced

concreteness, the fallacy of assuming that the particular concepts we employ to examine the flow of events capture their entire content. No more than in other fields of inquiry are sociological concepts designed to depict the concrete entirety of the psychosocial reality to which they refer. But the methodological rationale for conceptual abstraction has yet to provide a way of assessing the intellectual costs as well as the intellectual gains of abstraction. As Paul Weiss (1971) has put the general issue: "How can we ever retrieve information about distinctive features once we have tossed it out?" (p. 213).

Consider some outcomes of the established practice of employing bland sociological concepts that systematically abstract from certain elements and aspects of the concreteness of social life. It is then only a short step to the further tacit assumption that the aspects of psychosocial reality which these concepts help us to understand *are the only ones worth trying to understand*. The ground is then prepared for the next seemingly small but altogether conclusive step. The social scientist sometimes comes to act as though the aspects of the reality which are neglected in his analytical apparatus *do not even exist*. By that route, even the most conscientious of social scientists are often led to transform their concepts and models into scientific euphemisms.

All this involves the special irony that the more intellectually powerful a set of social science concepts has proved to be, the less the incentive for trying to elaborate it in ways designed to catch up the humanly significant aspects of the psychosocial reality that it neglects.

It is this tendency toward sociological euphemism, I suggest, that some (principally but not exclusively black) social scientists are forcing upon the attention of (principally but not exclusively white) social scientists. No one I know has put this more pointedly than Kenneth Clark (1965): "More privileged individuals may understandably need to shield themselves from the inevitable conflict and pain which would result from acceptance of the fact that they *are* accessories to profound injustice. The tendency to discuss disturbing social issues such as racial discrimination, segregation, and economic exploitation in detached, legal, political, socio-economic, or psychological terms as if these persistent problems did not involve the suffering of actual human beings is so contrary to empirical evidence that it must be interpreted as a protective device" (p. 75).

From Social Conflict to Intellectual Controversy

Perhaps enough has been said to indicate how Insider and Outsider perspectives can converge, in spite of such differences, through reciprocal adoption of ideas and the developing of complementary and overlapping

foci of attention in the formulation of scientific problems. But these intellectual potentials for synthesis are often curbed by social processes that divide scholars and scientists. Internal divisions and polarizations in the society at large often stand in the way of realizing those potentials. Under conditions of acute conflict, each hostile camp develops highly selective perceptions of what is going on in the other. Perspectives become self-confirming as both Insiders and Outsiders tend to shut themselves off from ideas and information at odds with their own conceptions. They come to see in the other primarily what their hostile dispositions alert them to see and then promptly mistake the part for the whole. The initial interaction between the contending groups becomes reduced in response to the reciprocal alienation that follows upon public distortions of the others' ideas. In the process, each group becomes less and less motivated to examine the ideas of the other, since there is manifestly small point in attending to the ideas of those capable of such distortion. The members of each group then scan the outgroup's writings just enough to find ammunition for new fusillades.

The process of increased selective inattention to ideas of the other produces rigidified all-or-none doctrines. Even intellectual orientations that are not basically contradictory come to be regarded as though they were. *Either* the Insider *or* the Outsider has access to the sociological truth. In the midst of such polarized social conflict, there is little room for the third party uncommitted in the domain of knowledge to, for them, situationally irrelevant group loyalties, who try to convert that conflict into intellectual criticism. Typically, these would-be noncombatants are caught in the crossfire between hostile camps. Depending on the partisan vocabulary of abuse that happens to prevail, they are tagged as intellectual mugwumps, pharisees or renegades, or somewhat more generously, as "mere eclectics" with the epithets making it unnecessary to examine the substance of what is being asserted or to consider how far it holds true. Perhaps most decisively, they are defined as mere middle-of-the-roaders who, through timidity or expediency, will not see that they try to escape the fundamental conflict between unalloyed sociological good and unalloyed sociological evil.³¹

When a transition from social conflict to intellectual controversy is achieved, when the perspectives of each group are taken seriously enough to be carefully examined rather than rejected out of hand, there can develop trade offs between the distinctive strengths and weaknesses of Insider and Outsider perspectives that enlarge the chances for a sound and relevant understanding of social life.

³¹ The foregoing two paragraphs are drawn almost verbatim from a not easily accessible source: Merton 1961, pp. 21-46.

Insiders, Outsiders, and Types of Knowledge

If indeed we have distinctive contributions to make to social knowledge in our roles as Insiders or Outsiders—and it should be repeated that all of us are both Insiders and Outsiders in various social situations—then those contributions probably link up with a long-standing distinction between two major kinds of knowledge, a basic distinction that is blurred in the often ambiguous use of the word “understanding.” In the language of William James (1932, pp. 11–13), drawn out of John Grote (1865, p. 60), who was in turn preceded by Hegel (1961 [1807]),³² this is the distinction between “acquaintance with” and “knowledge about.” The one involves direct familiarity with phenomena that is expressed in depictive representations; the other involves more abstract formulations which do not at all “resemble” what has been directly experienced (Merton 1968, p. 545). As Grote noted a century ago, the distinction has been imbedded in contrasting pairs of terms in various languages as shown below.

| “Acquaintance with” | “Knowledge about” |
|---------------------|-------------------|
| noscere | scire |
| kennen | wissen |
| connaître | savoir |

These interrelated kinds of understanding may turn out to be distributed, in varying mix, among Insiders and Outsiders. The introspective meanings of experience within a status or a group may be more readily accessible, for all the seemingly evident reasons, to those who have shared part or all of that experience. But authentic awareness, even in the sense of acquaintance with, is not guaranteed by social affiliation, as the concept of false consciousness is designed to remind us. Determinants of social life—for an obvious example, ecological patterns and processes—are not necessarily evident to those directly engaged in it. In short, sociological understanding involves much more than acquaintance with. It includes an empirically confirmable comprehension of the conditions and often complex processes in which people are caught up without much awareness of what is going on. To analyze and understand these requires a theoretical and technical competence which, as such, transcends one’s status as Insider or Outsider. The role of social scientist concerned with achieving knowledge about society requires enough detachment and trained capacity to know how to assemble and assess the evidence without regard for what the analysis seems to imply about the worth of one’s group.

³² Hegel catches the distinction in his aphorism: “Das Bekannte überhaupt ist darum, weil es bekannt ist, nicht erkannt.” Polanyi (1959, 1967) has made a significant effort to synthesize these modes of understanding, principally in his conception of “tacit knowing.”

Other attributes of the domain of knowledge dampen the relevance of Insider and Outsider identities for the validity and worth of the intellectual product. It is the character of an intellectual *discipline* that its evolving rules of evidence are adopted *before* they are used in assessing a particular inquiry. These criteria of good and bad intellectual work may turn up to differing extent among Insiders and Outsiders as an artifact of immediate circumstance, and that is itself a difficult problem for investigation. But the margin of autonomy in the culture and institution of science means that the intellectual criteria, as distinct from the social ones, for judging the validity and worth of that work transcend extraneous group allegiances. The acceptance of criteria of craftsmanship and integrity in science and learning cuts across differences in the social affiliations and loyalties of scientists and scholars. Commitment to the intellectual values dampens group-induced pressures to advance the interests of groups at the expense of these values and of the intellectual product.

The consolidation of group-influenced perspectives and the autonomous values of scholarship is exemplified in observations by John Hope Franklin who, for more than a quarter-century, has been engaged in research on the history of American Negroes from their ancient African beginnings to the present.³³ In the first annual Martin Luther King, Jr., Memorial Lecture at the New School for Social Research, he observes in effect how great differences in social location of both authors and audiences can make for profound differences in scholarly motivation and orientation. Franklin notes that it was the Negro teacher of history, "outraged by the kind of distorted history that he was required to teach the children of his race," who took the initiative in the 19th century to undo what one of them described as "the sin of omission and commission on the part of white authors, most of whom seem to have written exclusively for white children" (1969, p. 4). The pioneering revisionist efforts of W.E.B. DuBois and others found organized expression in the founding in 1915 of the Association for the Study of Negro Life and History and, a year later, of the *Journal of Negro History* by Carter G. Woodson and his associates. This institutionalization of scholarship helped make for transfer and interchange of knowledge between Insiders and Outsiders, between black historians and white. In Franklin's words, the study of Negro history became "respectable. Before the middle of the twentieth century it would entice not only a large number of talented Negro scholars to join in the quest for a revised and more valid American history, but it would also bring into its fold a considerable number of the ablest white historians who could no longer tolerate biased, one-sided American history. Thus, Vernon Wharton's *The Negro in Mississippi*, Kenneth Stampp's *The Peculiar Institution*, Louis Harlan's *Sepa-*

³³ Perhaps the best known of Franklin's many writings is *From Slavery to Freedom*, now in its third edition.

rate But Unequal and Winthrop Jordan's *White Over Black*—to mention only four—rank among the best of the efforts that any historians, white or black, have made to revise the history of their own country. In that role they, too, became revisionists of the history of Afro-Americans" (1969, pp. 5–6).

These efforts only began to counter the "uniformed, arrogant, uncharitable, undemocratic, and racist history [which] . . . spawned and perpetuated an ignorant, self-seeking, superpatriotic, ethnocentric group of white Americans who can say, in this day and time, that they did not know that Negro Americans had a history" (1969, p. 9). But much needed counterdevelopments can induce other kinds of departure from scholarly standards. Franklin notes that the recent "great renaissance" of interest in the history of Negro Americans has found proliferated and commercialized expression. "Publishers are literally pouring out handbooks, anthologies, workbooks, almanacs, documentaries, and textbooks on the history of Negro Americans. . . . Soon, we shall have many more books than we can read; indeed, many more than we should read. Soon, we shall have more authorities on Negro history than we can listen to; indeed, many more than we should listen to" (1969, pp. 10–11).

Franklin's application of exacting, autonomous and universalistic standards culminates in a formulation that, once again, transcends the statuses of Insiders and Outsiders:

Slavery, injustice, unspeakable barbarities, the selling of babies from their mothers, the breeding of slaves, lynchings, burnings at the stake, discrimination, segregation, these things too are a part of the history of this country. If the Patriots were more in love with slavery than freedom, if the Founding Fathers were more anxious to write slavery into the Constitution than they were to protect the rights of men, and if freedom was begrudgingly given and then effectively denied for another century, these things too are a part of the nation's history. It takes a person of stout heart, great courage, and uncompromising honesty to look the history of this country squarely in the face and tell it like it is. But nothing short of this will make possible a reassessment of American history and a revision of American history that will, in turn, permit the teaching of the history of Negro Americans. And when this approach prevails, the history of the United States and the history of the black man can be written and taught by any person, white, black, or otherwise. For there is nothing so irrelevant in telling the truth as the color of a man's skin. [1969, pp. 14–15]

Differing profoundly on many theoretical issues and empirical claims, Cox (1948; also introduction to Hare 1970) and Frazier (1957, 1968) are agreed on the relative autonomy of the domain of knowledge and, specifically, that white scholars are scarcely barred from contributing to what Frazier described as a "grasp of the condition and fate of American Negroes." Recognition of what has been called "the mark of oppression,"

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Frazier notes, "was the work of two white scholars that first called attention to this fundamental aspect of the personality of the American Negro. Moreover, it was the work of another white scholar, Stanley M. Elkins, in his recent book on *Slavery*, who has shown the psychic trauma that Negroes suffered when they were enslaved, the pulverization of their social life through the destruction of their clan organization, and annihilation of their personality through the destruction of their cultural heritage" (Frazier 1968 p. 272). And Cox, in his strong criticism of what he describes as "the black bourgeoisie school" deriving from Frazier's work, emphasizes the distorting effects of the implicitly black nationalist ideology of this school on the character of its work (Cox 1970, pp. 15-31).

It should now be evident that structural analysis applied to the domain of knowledge provides an ironically self-exemplifying pattern. For just as the union of any other collectivity based on a single status—of Americans or of Nigerians, of blacks or of whites, of men or of women—is continuously subject to the potential of inner division owing to the other statuses of its members, so with the collectivities often described as the scientific community and the community of scholars. The functional autonomy of science and learning is also periodically subject to great stress, owing in part to the complex social differentiation of the population of scientists and scholars that weakens their response to external pressures. The conditions and processes making for the fragility or resiliency of that autonomy constitute one of the great questions in the sociology of knowledge.

It is nevertheless that autonomy which still enables the pursuit of truth to transcend other loyalties, as Michael Polanyi (1959), more than most of us, has long recognized: "People who have learned to respect the truth will feel entitled to uphold the truth against the very society which has taught them to respect it. They will indeed demand respect for themselves on the grounds of their own respect for the truth, and this will be accepted, even against their own inclinations, by those who share these basic convictions" (pp. 61-62).³⁴

A paper such as this one needs no peroration. Nevertheless, here is mine. Insiders and Outsiders in the domain of knowledge, unite. You have nothing to lose but your claims. You have a world of understanding to win.

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³⁴ I have taken the liberty of modifying Polanyi's pronouns in this passage in order to preserve his meaning within the context of the subject of this paper.

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Radical Politics and Sociological Research: Observations on Methodology and Ideology

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With the increasing polarization of political opinions and positions in the United States, sociologists have become increasingly concerned about the political import of their work. Until recent years, most sociologists probably believed that sociology was somehow above politics, even though sociologists had often engaged in political activity, and political and sociological discussion had often overlapped. Events have now made it impossible to leave that belief uninspected. The disclosure that social scientists have undertaken research designed to further the interests of the powerful at the expense of those of the powerless (e.g., riot control at home and "civic action" abroad) showed how even apparently innocent research might serve special political interests. Prison research has for the most part been oriented to problems of jailers rather than those of prisoners; industrial research, to the problems of managers rather than those of workers; military research, to the problems of generals rather than those of privates. Greater sensitivity to the undemocratic character of ordinary institutions and relationships (ironically fostered by social scientists themselves) has revealed how research frequently represents the interests of adults and teachers instead of those of children and students; of men instead of women; of the white middle class instead of the lower class, blacks, chicanos, and other minorities; of the conventional straight world instead of freaks; of boozers instead of potheads. Wherever someone is oppressed, an "establishment" sociologist seems to lurk in the background, providing the facts which make oppression more efficient and the theory which makes it legitimate to a larger constituency.

The belief that members of the sociological discipline are guilty as charged helps to account for the way many sociologists have responded to the attacks. They have not dismissed the charges. On the contrary, professional associations, scientific societies, the periodical literature, and foundations from Ford to Russell Sage have reviewed the political tenor of sociological work. Younger men have debated whether it was moral to be affiliated with the sociological enterprise. Older sociologists have

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searched their work and their consciences to see if, far from being the political liberals they imagined themselves, they were in fact lackeys of capitalist repression.

In the midst of these reconsiderations, positions hardened. The language of scholarly journals became increasingly polemical. Meetings thought to be scientific were disrupted by political protest and discussion. Presidential addresses at national and regional meetings were interrupted. All this was accompanied by, and in some cases was intimately connected with, political uprisings on entire campuses, in more than one of which sociology students played a key role. Some teachers found themselves unable to bear the discourtesies of their radical students. Some professors saw attempts to change the hierarchical relations of a department as an attack on the very idea of scholarship. They assumed that a student who called their ideas "bullshit" was attacking rational thought, and not simply using in public a critical rhetoric usually reserved for private meetings. Sometimes they were right, for some students seemed intent on cutting off debate and substituting for the free play of intellect a vocabulary designed exclusively to conform to a political position. The distinction was not always easy to make, and those making affronts were often as unsure of what they were about as those receiving them.

In situations of collective upheaval, persons and groups move to maximize their private interests. In this case, some sociologists tried to further their professional careers by judiciously taking one side or the other. Groups moved to secure power within professional associations. Some radicals seriously discussed taking over professional associations or university departments, having convinced themselves that worthwhile political goals might be served by such acts—though the resemblance of such maneuvers to similar careerist actions by doctrinaire groups of quite different persuasions in the same associations and departments was obvious. Elder sociological statesmen of every political stripe appeared, trying to gather "unpredictable youth" into their own sphere of influence. The rhetoric of radicalism appeared in every area of sociology.

Participants in these events found themselves confused. Members of the older "straight" factions, however, failed to note the confusion. They saw the actions as concerted expressions of radical or left sentiment. They could not see the conflicts of interests among radicals, blacks, chicanos, women, and other "liberation" groups. The persistent emergence of differences among these groups made it obvious that the mere assertion of radical sympathies guaranteed neither concerted political action nor a uniform style of sociological analysis. The differences and confusions demonstrate the need for a clearer analysis of the political meaning and relevance of sociology.

Good sociology is often radical. A sociology which is not good, however, cannot be radical in any larger sense. But moral sentiments do not determine scientific quality. The reverse is more often true: the quality of sociological work determines the degree to which it has a radical thrust.

We insist on the isomorphism between radical sociology and good sociology in order to dissuade those who think political sloganeering can substitute for knowledge based on adequate evidence and careful analysis; to persuade others that their work has suffered from a conventional social and political attitude, expressed in the way they frame problems and in the methods of research they choose; and to demonstrate that there is a tradition of good sociology worth preserving, that the expression "good sociology" has meaning, and that the possibility of doing good sociology is not tied irrevocably to contemporary academic institutions.

GOOD SOCIOLOGY

Good sociology is sociological work that produces meaningful descriptions of organizations and events, valid explanations of how they come about and persist, and realistic proposals for their improvement or removal. Sociology based on the best available evidence should provide analyses that are likely to be true in the linguistic sense of not being falsifiable by other evidence, and also in the ontological sense of being "true to the world."

In the first sense, generations of methodologists have developed procedures and techniques by which approximate truth can be reached. The sociologist achieves partial truths, always open to correction. While methodologists have dealt only with a small part of the problem of arriving at propositions and inferences likely to be true, the techniques they recommend as warranted are all we have; we will have to use them until we invent something better. With all their faults, interviews, participant observation, questionnaires, surveys, censuses, statistical analysis, and controlled experiments can be used to arrive at approximate truth. While the results to date are modest, some things are known because sociologists have employed these techniques.

Sociologists have done less well by truth in the second sense. While they know some things well, they can predict few things with accuracy. Humanists and scientists alike complain that sociology tells them only a tiny part of what they really want to know. Men want to know how the world is; sociologists give them correlation coefficients. Coefficients do help us know how the world is, and one need not accept the humanistic contention that unless sociology can reproduce the world in full living color it is worthless. Nonetheless, the charge stings. Sociologists' knowledge about real problems in society does not take them far.

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If essentials are left out, the work cannot pass the tests that science poses for itself. The work cannot, to use the language of statistical analysis, account for much of the variance in the phenomena under study. In addition, sociological work loses its potential practical importance if it does not encompass the major processes and actors involved in those parts of the world to be changed. Therefore, work that is not true to the world has neither scientific nor practical value.

Why does so much sociological work fail to be true both to its own scientific standards and to the larger world? Some radical sociologists have insisted that political ideologies blind us to the truth because our political masters have paid us to produce research that will be useful in a different direction, or (more subtly) because our standard methods and concepts, reflecting political biases and pressures, prevent us from seeing what would be politically inconvenient. Many failures in sociological research result from simple ignorance, having little to do with either ideological bias or utopian fantasy. But we should examine those instances in which sociological research has been severely blemished not so much by ignorance as by bias.

Consider the charge that the concept of "accommodation," applied to racial relations, had a conservative effect (Myrdal 1944). It implied that blacks accepted their lower position in American society and that therefore, because blacks did not complain, the situation was not unjust. But to say that racial relations in a given place at a given time were accommodative means only that the racial groups involved had achieved a *modus vivendi* and does not imply that the actors were happy or the system just. Whether any or all actors considered the system pleasant or righteous is a matter for empirical investigation. If the description of the situation as accommodative were true to the world, no evidence of conflict and resistance could be discovered because none would exist. To assume consensus would be bad sociology insofar as it assumed that, since there is evidence of accommodation, we can rule out the possibility of conflict. The concept of accommodation can be objectionable only if we insist that its use will necessarily cause sociologists to overlook or ignore conflict, exploitation, or resistance to change where they occur. But a full exploration of possibilities, as in Robert Park's description of the race relations cycle (Park and Burgess 1921), applied evenhandedly, should spare sociologists such errors.

Much contemporary sociology is not true even in the narrow scientific sense. It is falsifiable by evidence contained in its own data or by evidence that could have been obtained had the investigator bothered to look for it. Sociologists tend to ignore the degree to which they fail to abide by their own methodological standards and consequently fail to achieve the sci-

entific rationality to which they pretend. Where sociology allows political biases and generalized expressions of wishful thinking to affect its conclusions, it lacks truth in either of the two meanings discussed.

RADICAL SOCIOLOGY

An immense variety of political positions have been announced as radical. Since the actual consequences of the label are so important to all the people involved, one cannot expect any definition to go undisputed. But most arguments over definitions turn on questions of the means by which agreed-on goals can be achieved or of the correct diagnosis of the ills that afflict society, rather than on the goals that radicals ought to strive for. Thus, most radicals will agree that a key feature of any radical political program is the reduction and eventual removal of inequalities in society, whether the inequality is of power, economic resources, life chances, or knowledge. Likewise, most radicals will agree that a radically reconstructed society should maximize human freedom, especially when that is conceived as dialectically related to social order.

Radicals may not so universally agree on the necessity of permanent change and revolution as an ideal. We ourselves believe that every society and every set of social arrangements must be inspected for their potential inequalities and interferences with freedom, even those which seem to conform to one or another blueprint for a socialist utopia. The radical, so defined normatively, is never satisfied, never prepared to abandon the struggle for an even more egalitarian and free society. At the least, the better is the critic of the good.

Where circumstances compel a choice between individual interests, self-expression, and personal welfare, on the one hand, and social order, stability, and the collective good, on the other, such a radical politics acts for the person as against the collectivity. It acts to maximize the number and the variety of options people have open to them, at the expense of neatness, order, peace, and system. It regards conflict as a normal concomitant of social life and a necessary element in political action. Clearly, some definitions of radicalism are based precisely on collectivism. While we look for the convergence of personal and public goals, when we are compelled to make a choice, it is on behalf of persons.

The radical sees change as permanent and inevitable, but he need not accept all changes as good. Rather, he sides with the powerless against the powerful, and renounces coercion, terror and control as methods of establishing truths about the world.

The posture of a radical sociology overlaps considerably with that of a radical politics. Radical sociology also rests on a desire to change society in a way that will increase equality and maximize freedom, and it makes a

distinctive contribution to the struggle for change. On the one hand, it provides the knowledge of how society operates, on the basis of which a radical critique of inequality and lack of freedom can be made. On the other hand, it provides the basis for implementing radical goals, constructing blueprints for freer, more egalitarian social arrangements, the working plans for radical utopias. These constructive aspects are rooted in the positivist tradition, just as the critical aspects are rooted in the Marxist tradition. Both involve an explanation of radical goals, and both involve a repudiation of all forms of mystical, theological, and supernaturalist interpretations of events.

A radical sociology thus looks for explanations of social life and theories of society which assume that radical change is at least possible, and resists those theories which root inequality in "inescapable facts" of biology or social structure (Horowitz 1968). Since such assumptions are seldom subjected to empirical test, a radical sociology can just as reasonably assume possibilities that more conservative or pessimistic sociologies do not.

In the controversy between Davis and Moore (1945, pp. 242-49) and Tumin (1953, pp. 387-93), the real difference of opinion was not over the fact of social stratification in American life, or even over the existence of inequality. There were indeed differences of opinion at the factual level, but the core controversy concerned the tendencies of American society: whether the direction of the democratic society carried with it as a central agenda item the reduction and finally the elimination of inequality. If inequality is rooted in the nature of man, with only the forms of inequality changing and the types of oppressors shifting, then the goal of equality is itself suspect. Any radical sociology must explore the nature of inequality fully, and beyond that must assume the possibility of abolishing inequality and describing the machinery necessary to implement a more egalitarian social order (Dumont 1969).

It might be objected that this equation of radicalism and the search for equity itself represents a liberal "bourgeois" model rather than a radical paradigm, that true radicalism must uphold the banner of a particular social system, such as socialism. While this formulation is abstractly appealing and in fact is often employed by radical theorists, it omits the most important fact of our times: the need for a social scientific judgment of *all* available political systems. Any equation of radical perspectives with the demand for equity implies the universal claim to priority of equality in socialist systems such as the Soviet Union, no less than in capitalist systems such as the United States. To demand allegiance to any social system as the mark of a radical perspective is to ignore the 100-year history of inequality within what has passed for socialism, as well as the far longer history of inequality under capitalism.

But the search for equity is only one side of the radical thrust. At least

equal in importance is the investigation of the ability of society, as presently organized, to deliver equity. It is the assessment of that ability that divides radical and liberal analysts. The ability of established society to absorb new social demands of disenfranchised groups becomes a major concern of the radical. The historic concern of radicalism with problems of revolution expresses a pessimistic view of the present social and economic order's ability to absorb change.

No matter which of these several tasks a radical sociology undertakes, it finds itself providing the facts, theories, and understandings that a radical politics requires for its implementation. For a radical political posture without reliable facts and analyses is no more than insurrectionary art incapable of predicting its own successes or failures. Radical sociology provides relevant and trustworthy data, intellectual resources for measuring costs as well as benefits in realizing the insurrectionary act.

CONFLICT BETWEEN RADICAL SOCIOLOGY AND RADICAL POLITICS

Radical sociology may create a tension with radical politics, pure and simple, by indicating the high cost of some desired act. For example, it may analyze the special features of the Cuban-Castro revolution and produce an explanation of why guerrilla insurgency was successful in Cuba in 1959 but tragically unsuccessful in Bolivia in 1969. It is by no means a simple matter to counsel the gathering of evidence instead of performing a revolutionary act. But the gathering of evidence distinguishes a radical sociology from a radical politics, without necessarily destroying the basis for their mutual interaction.

A radical sociology will base itself either explicitly or implicitly on the premises of a radical politics. In either case, it will produce knowledge that serves the purposes of radical politics, in any of a variety of ways.

Every group in power is bent on the protection of privilege. Therefore, every radical sociology must expose the nature of such privilege, unmasking forms of domination. This unmasking process creates dilemmas. It implies a ruthless stripping away of all mystery and cant, not just that of the Department of Defense but also that of the Nation of Islam. One task of a radical sociology is thus to persuade the oppressed and radicals of the need for as total a dedication to what is true as to what they may deem good. It is here too that the issue between most contemporary forms of radical sociology and radical political action becomes enmeshed in controversy, since many forms of radical politics are themselves bound to canons of secrecy, perhaps more benign than conservative politics but ultimately no less destructive of the search for truth in society.

Every status quo—societal, organizational, or factional—thrives on myth and mystification. Every group in power—in a nation, a government,

an economy, a political party, or a revolutionary cadre—tells its story as it would like to have it believed, in the way it thinks will promote its interests and serve its constituencies. Every group in power profits from ambiguity and mystification, which hide the facts of power from those over whom power is exerted and thus make it easier to maintain hegemony and legitimacy. A sociology that is true to the world inevitably clarifies what has been confused, reveals the character of organizational secrets, upsets the interests of powerful people and groups. And while uncovering error does not necessarily aid the interests of those exploited by an organization or society, it does at least permit equal access to the evidence upon which action must be based. Only if sociological work is good in the sense of explaining actual relationships of power and authority can it provide a force for change. Thus, work which is true to the world and explains the actual relations of power and privilege that envelop and determine what goes on in society will be politically useful to radicals, even though (importantly) those who do such work may not themselves be committed to radical political goals.

Sociologists already know the difficulties that come from doing work which exposes the operations of powerful groups in society (see the various accounts in Vidich, Bensman, and Stein 1964). In operating explicitly in behalf of radical goals and in cooperation with people engaged in radical political action, the sociologist will experience other characteristic difficulties. For instance, a good sociological analysis, explored fully for its political implications, may undermine one's own position of superiority and privilege. Thus, a radical sociological analysis of universities entails exposing the myths by which the professor who makes the analysis supports his own privileged position of tenure and income. Similarly, white sociologists find themselves producing work which undermines their unequal privileges vis-à-vis blacks; men undermine the bases of their social superiority to women; and so on. This is why the poor and downtrodden are never "radical"—what they do is "natural," in keeping with their "interests." The radical violates the canon of self-interest or group interest. The good sociologist carries radicalism a step further: he makes it a principle to transcend parochialism and patriotism in investigating the social context.

The radical sociologist will also find that his scientific "conservatism"—in the sense of being unwilling to draw conclusions on the basis of insufficient evidence—creates tension with radical activists. This results from the differing time scales of the two activities. The social scientist takes time to collect evidence, but the political activist must often make decisions prior to the compilation of adequate evidence. Under such circumstances, the political man will act; the sociologist can give him the best available evidence. Radical activism is not the same as the know-nothing-

ism underlining the irrationalist "will to act," but rather a recognition that action may be induced by needs that cannot possibly await the supply of the social scientist's information. The lag between action and information explains, in part, the peculiar tension between the political man and the social scientist, a tension that often leads the activist to disregard the sociologist's advice and, correspondingly, often leads the sociologist to an overconservative estimate of the potential success of a dramatic political action.

Since radical politics and radical sociology are not the same, the two may conflict. What is the relation between political radicalism and sociological radicalism? Rosa Luxemburg acted as a revolutionist and as a leader of the Spartacists of the German Left Socialist movement, but at the same time she functioned as a radical, as a critic of Lenin and of the dogma of proletarian dictatorship. She did this at a moment of revolutionary euphoria when serious thinking was at a premium. It is her criticism which is now best remembered. The same can be said of Eugene V. Debs, whose importance in the Socialist movement lay precisely in his being above the fratricidal struggles for control of the Socialist party apparatus. Debs, the radical man, had little organizational power in American socialism. Far less concerned with organization than DeLeon, Debs alone emerged as the ecumenical figure for the Socialists (see Ginger 1949.)

Radicalism, then, entails a critique of organizational constraints. Yet revolution can only be made on the basis of a theory of organization. This is why the roles of radical sociologist and revolutionary activist, while they may coexist, cause considerable tension within the person and between the organization and the individual. If the activist joins forces with other advocates for rapid change, the sociologist points out how limited the practical effects of these changes may be. The activist, achieving his goals, seeks to enjoy the fruits of his victory; the radical sociologist looks for new sources of inequality and privilege to understand, expose, and uproot.

The difference between political radicalism and sociological radicalism deserves further elaboration. While the two can be linked, they can also occur independently and may be quite distinct. Radical action and rhetoric are one thing, and a radically informed sociology is another. Confusing the two opens leftism to any professional opportunism of the moment. Political sloganeers can easily tailor their doctrine to the changing fortunes of political sects. Serious sociologists find it much harder to change their sociological practice to match their changing political beliefs. To teach the same courses in theory and method one taught 20 years ago, while shifting from support for the government to opposition, does little

to change the political thrust of contemporary social science. One can use radical rhetoric and engage in radical political action while one's sociology, because of its failure to be good, leaves established myths and institutions untouched. This is only the radical manifestation of the dualist distinction between "fact" and "value" adhered to by most conservatives. The "values" shift and become anti-establishment rather than pro-establishment. But the world of "fact," or, as is more nearly true, the fantasy that passes for sociological fact, remains unaltered.

RADICALISM AND CAUSAL ANALYSIS

The intersection of sociological and political analysis, the common ground which allows a characterization of various kinds of sociology as having one or another political cast, lies in their mutual concern with causes of events. It seems clear that any necessary condition for the occurrence of an event may be considered a cause of that event, at least in the limited sense that if the condition were not present, the event would not occur. From this point of view, there is an infinite or at least a very large number of causes of any event. To use a *reductio ad absurdum*, the presence of oxygen in the atmosphere is a cause of class exploitation, since without oxygen there would be no people, and without people there could be no exploitation. All such physical conditions of human action are, in this extended and vacuous sense, causes. In a more restricted and less trivial way, the actions of every person and group that contribute, however remotely, to a social event occurring in the way it did can be seen as a contributing cause of the event, since in their absence things would have occurred differently. To take a not-so-absurd example, the actions of slaves constitute one of the causes of slavery, since they could (and sometimes did) refuse to act as slaves (even though the price might be death).

Even though there are a multitude of causes of any event, both scientific and political analysis concentrate on only a few of them—different analyses emphasizing different causes. How do sociologists choose from among the many possible causes those they will emphasize in their political analysis or investigate in their research? Sometimes they look for those potential causes which vary, or might vary, in the specific cases observed. Thus, social scientists ignore the presence of oxygen as a cause of social events, since it is a constant in human affairs (except, of course, for those rare situations in which its presence becomes problematic, as in a recent study of social relationships among the men who climbed Mount Everest [Emerson 1966]). Sometimes they choose causes for investigation with an eye to the "usefulness" of results. Insofar as analysis is meant to be

useful as a guide to someone's action, sociologists look to causal analysis for the clues as to how things might be changed, how they might be kept the same, and what the cost of either course is.

These guidelines help somewhat but do not go far enough in cutting down the number of causes the analyst pays attention to. On further inspection, we can see that the assignment of causes to events has a political aspect. The way sociologists assign causes, both in setting up hypotheses to be studied and in announcing conclusions, exhibits the influence of a political point of view, however implicit or hazy.

When sociologists link a cause to an event or a state of affairs, they at the same time assign blame for it. An event occurred because certain actors did something that helped to make it occur; had they acted differently, the event would not have occurred as it did. If the event is judged to be morally or politically reprehensible, the sociological analysis, by isolating those actors as the cause of the event, blames them for its occurrence.

An analysis may also implicitly or explicitly place the blame for events on impersonal forces beyond personal control—human nature, the human condition, or the social system—and thus excuse the people whose actions appeared to be morally suspect by suggesting that they could not help doing what they did. Deterministic sociologies of every description perform this service for the villains they identify.

If sociology allows for choice on the part of human actors, then it can blame, by the way it assigns causes, any of the people involved, since they could have chosen not to do what they did. This has consequences for the political character of a sociological analysis. Volitional sociologies perform this service for the heroes they identify.

The sociological analysis of causes has practical importance. When some object or action is labeled as the cause of the event or situation, the analysis suggests what would have to be influenced or altered in order to make a significant change in that event or situation. Some things will be easier to change than others. The analysis may suggest that, under the circumstances, it is virtually impossible to change what must be changed in order to affect the situation. Alternatively, the analysis may focus on things easily changed in themselves but which have little chance of changing the situation. Every combination of the feasibility of intervention and of the magnitude of the expected effect can occur in a particular analysis.

When sociologists, in their investigation of causes, implicitly or explicitly assign blame for events and when they suggest what must be done to cause meaningful social change, they speak of matters that are also the subject of political analyses. Their analyses can be judged to be radical, liberal, or conservative by the same criteria used to judge political analyses.

In general, radicals will judge a sociological analysis as radical when its assignment of causes, and thus of blame, coincides with the preferred demonology of the political group making the judgment. Radicals will denounce analyses as conservative (and conservatives will denounce analyses as radical) when the assignment of causes blames people who "don't deserve it." Similarly, radicals may criticize analyses that suggest causes which, when we take action, are too easily influenced and will not produce sufficiently profound results (right-wing reformism or opportunism), or are too difficult to influence, thus leading to disillusionment and low morale (left adventurism).

Since radical political positions are more "unusual" and thus more visible in contemporary social science, it is radical sociologists who are most aware of these political connotations of sociological work. Most discussions of the problem have therefore been conducted by sociologists who conceive of themselves, or would like to conceive of themselves, as radical and who therefore focus on ferreting out the political implications of work that is not politically self-conscious. Both because of our own political position and for the sake of congruence with current discussions, we will take the same tack. It should be understood, however, that in a society where some version of radical politics was more common and dominated research in an unselfconscious way, a similar critique might be mounted from the center or right. In our own society, political judgments of the results of sociological work could as easily be made from those positions, though they could scarcely be designed to uncover hidden radical assumptions, since radical sociologists tend to make these quite explicit.

Examples of the political import of causal analysis are easily available. It is common knowledge that most black Americans live less well than most white Americans. Something ought to be done about it; people mostly agree on that as well. What causes this situation? Some explanations explicitly blame the victims themselves, by finding, for instance, that their own inherited defects lead to all their trouble (see the critique in Ryan 1971, pp. 3-30). Many people found fault with Moynihan's explanation that some of the trouble lay in the disorganization of the black family (Rainwater and Yancey 1967). That explanation seemed implicitly to blame blacks for their own troubles by suggesting that they need not have been so disorganized. It did not emphasize the causes of that disorganization, which, when revealed, placed the blame on their oppression by the white community. The same analysis further suggested that it would be difficult to change things because it is quite difficult to change family patterns. The Moynihan analysis might thus be interpreted as having a conservative political thrust.

Consider the rash of ideological interpretations of student protest movements. Investigators may locate the causes of those protests in some

characteristic of students themselves (e.g., Shils 1969; Feuer 1969), and thus implicitly suggest that it is the actions of students which, without the help of any of the other involved parties, produce all the trouble. Students are to blame while, by implication, others whose behavior we do not regard as a cause are not to blame. Alternatively, we can interpret campus disorders as political phenomena which arise in the same way as other political phenomena, and serve as a mechanism by which subordinate groups make hierarchical superiors pay attention to their demands for change (e.g., Becker 1970; Horowitz and Friedland 1970). In such a case, the difficulty can be located in the disparity between what one group wants and what the other group is willing to give, and it becomes equally possible to blame those who refuse to give students what they want, since that refusal is one of the necessary conditions for the occurrence of the disorderly events.

Political and sociological analyses both operate under a potent constraint, which is that actions based on them should have the anticipated consequences. That remains a major test of any *scientific* proposition. If an analysis is factually incorrect, then political predictions will not come to pass and strategies will be discredited. Science will not validate propositions just because they appear ethically worthwhile; the propositions must be correct in the real world. In this sense, radicalism is a necessary, but not a sufficient, condition of good sociology.

The production of factually correct analyses involves a paradox. What sociologists need to know about any institution or organization in order to achieve radical goals is usually similar to what they must know to achieve conservative ends as well. Consider research on consumer behavior. Advertising and marketing experts, presumably lackeys of the capitalist system, have done research to discover how to make advertising more effective, that is, how to manipulate people so that they will buy what they might not have bought otherwise. Simultaneously, radicals have complained, though they have not done research on the topic, that advertising makes people desire commodities they do not need. Radicals agree that advertising works the way marketing people say it does. Radical sociologists presumably want to know how to lessen the impact of advertising and make people's choices free; they might be interested in how the process of choice would work in a situation devoid of the artificial influence of advertising.

Apart from the difference in the moral animus of the language used by opposing groups, both conservative businessmen and radical activists need, to further their opposing ends, the same knowledge about the process by which consumers choose products. If we had a decent theory of consumer behavior, empirically validated, then the radical, knowing how advertising works, would know where to intervene so that it would not work, and the marketing expert would know why his techniques fail and how to improve

them. An adequate analysis of how things stay the same is thus at the same time an analysis of how to change them. Conventional, presumably conservative, analyses often fail to take into account matters radicals think important. If those matters are indeed important, then the conservative analysis which ignores them will be faulty and its predictions will not prove true.

Political commitment is revealed by the kind of causes sociologists include in their analyses, by the way blame is assigned and the possibilities of political action evaluated. It is revealed most clearly by ignoring causes conceived of as incapable of change when in fact they could be changed under certain conditions, and by regarding a situation as easily subject to change when in fact there are substantial forces perpetuating it. Such false assumptions make it likely that plans of action resting on them will fail. In fact, although it is often charged that American social science is (presumably successfully) engaged in helping oppressors keep subject populations in their place, the actions which are supposed to be based on these analyses often fail, precisely because they have failed to take into account important causes suggested by more radical sociological analyses.

OBSTACLES TO RADICAL SOCIOLOGY

If the foregoing analysis of causality is correct, it ought to be no more difficult to create radical sociology than other varieties already available. Yet, for all the stated need for a more radical sociology, we find mostly programmatic statements and little substantive work that could reasonably be so labeled. It cannot be that there are no radical sociologists, for they have made their presence known. Indeed, as we have suggested, even those who call themselves radical have trouble knowing what their sociology ought to look like; in fact, we can see that it often differs in no observable way from nonradical sociology.

Some radicals in sociology claim that there is no truly radical sociology because most sociologists, being liberals or worse, are on the take from the establishment and naturally do not wish to make analyses that will subvert their own material interests. These radicals further suggest that the organizations which distribute research funds and control publication are so dominated by liberals and conservatives that radical work cannot be supported or published. If we accept such statements as radical sociological work, the ease with which they too achieve publication and professional recognition suggests that they are not true (see Nicolaus 1969).

Those who conceive of themselves as radical sociologists find it hard to do identifiably radical research, while politically neutral sociologists do research useful for radical goals (in the sense that they discover causal relationships which can be used as guides for radical political action).

That demands explanation. There seem to be three chief reasons for this lack of connection between radical sociology and radical politics: (1) the conservative influence of conventional technical procedures, (2) common-sense standards of credibility of explanations, and (3) the influence of agency sponsorship. Each of these, in its own way, deters the sociologist's full exploration of the range of necessary conditions that ought to be considered as potential causes of the situation he studies.

Most commonly used research techniques require the investigator to have worked out his hypotheses fully before he begins gathering data. If we conceive research as testing deductions made from existing theories (wherever those theories come from), then the data one gathers must be suitable for making such tests. One restricts what he finds out to what will be relevant to those hypotheses. Experiments, surveys, and paper-and-pencil testing necessarily restrict the range of causes eventually considered, by the simple technical fact of confining inquiry to what the researcher has in mind when he plans his research. But in doing research, we often find that we have failed to take into account many variables and causes that, on the basis of early findings, we see we should take into account.

With respect to the possibility of a radical sociology, what we leave out may not be important for the allocation of blame. But if what has been neglected, or made impossible to locate, is necessary to effect change, such research becomes less useful for radical political purposes by virtue of that gap. Even committed political radicals find themselves constrained by the research techniques they are familiar with. These techniques often leave out some things they would think important if they knew about them. Some techniques, indeed, require sociologists to leave out things they *know* might be important. Thus, it is difficult, though not altogether impossible, to study certain kinds of power relationships and many kinds of historical changes by the use of survey research techniques. If that is what one knows how to do, then he is stuck with what he can discover by that technique.

Another barrier to a radical sociology lies in common-sense conceptions of credibility. Every theoretical stance, including those defined as radical, makes assumptions about the character of the world. In particular, the sociological view of the world usually assumes that some people are more believable than others, that their stories, insights, notions, and theories are more worthy of being taken seriously than those of others. One of the chief reasons conventional sociology fails to uncover some important causes of events and situations is that it accepts the common-sense notion that the people who run organizations and are highly placed in communities know more about those organizations and communities than others, and therefore ought to be taken more seriously. The immediate effect of assuming the veracity of highly placed people is to leave out of considera-

tion questions and problems that appear foolish from an elitist viewpoint (Becker 1967).

Conventional sociologists might, for instance, find it reasonable to ask why some schools are more effective in teaching their students than others. But it violates common sense to suggest, even though research might show it to be true, that schools actually prevent people from learning what they are supposed to learn. We have similar official versions and analyses of most social problems. When we study those problems, we find it hard to free ourselves from official analyses sufficiently to consider causes not credited in those versions. This is not to say that other causes are necessarily operative, but only that sociologists often fail to look at them because they seem unlikely or bizarre.

Radical politics has its own set of official explanations, its own set of preferred causes, and one can err as badly by taking these for granted as he can by taking conventional causes for granted. Of course, radically oriented research will seldom leave out of account what conventional sociologists include, if only because it wishes to demonstrate that those analyses are wrong. Therefore, research organized on radical lines will probably be more inclusive and therefore more useful.

Agency sponsorship tends to put conservative limits on the search for necessary conditions (Blumer 1967). Although research is most commonly funded and sponsored by the government or foundations politically suspect from some radical point of view, the trouble does not necessarily arise from the political character of the sponsors. Rather, it occurs because, whatever their political persuasion, when agencies purchase research they are concerned with answers to particular questions, questions which arise for them as operational difficulties. They do not wish to spend their money on meandering investigations of God knows what. Therefore, the agreement between researcher and agency typically specifies a limited area of research, the limits set by the agency's conception of what the problem is and where its causes lie. Ordinarily, the agency will not see its own operations as one of the causes of the problem, and thus those operations will not be included in the area the researcher agrees to study; by implication, he agrees not to study them (see Platt 1971).

This discussion of barriers to unconventional radical sociological analyses allows us to look critically at some common notions of what constitutes radical sociology. Most of the common definitions of a radical style in sociology bear some relation to making the kinds of analyses we have now identified as radical. In every case, however, the connection is contingent rather than necessary. We need to understand the circumstances under which the phenomenon in question actually leads to radical analysis and when it does not.

When one does research for a government agency, that agency will

want the questions to be studied in a way that makes it difficult to come up with unconventional and radical conclusions. But refusing to accept government funds does not guarantee a radical analysis, nor is all research paid for by the government by definition conservative. If a federally funded researcher has arranged conditions so that he has maximum freedom, he may very well produce radical findings. Having done so, he may find it difficult to get further research funds from the same or similar sources. The remedy for that is to travel light, to avoid acquiring the obligations and inclinations that make large-scale funds necessary.

Studying radical groups from a sympathetic point of view, though one need not be particularly sympathetic with them to do so, may be of great use. Those groups might be exceptions to sociological wisdom, based on more conventional cases, and might make us aware of causal connections sociologists had not seen before. Thus, the study of communal living groups might allow sociologists to see certain possibilities of social organization that are ordinarily masked if we examine only longer-lasting and more stable institutions.

The influence of the sociologist's life style on his work becomes especially important in an era of theatrical politics. Wearing a Viet Cong button does not make one a radical any more than living in a suburb makes one a conservative. Nevertheless, wearing buttons, beads, or otherwise looking "freaky" may cause the person to have experiences (with police, fellow sociologists, or others) which will force him or her to question assumptions that might otherwise have been left uninspected. In the same way, living in a middle-class suburb might insulate the sociologist from some experiences and so lead him to incorrect assumptions about some matters of fact.

Personal involvement in political radicalism or affiliation with an organization that champions radical programs and positions does not necessarily lead one to do radical sociology. Such a political commitment might dispose a sociologist to search for causes and possible modes of intervention other analyses had left out. On the other hand, a radical sociologist might do research for his political allies which was no different in its style from the research other sociologists do for General Electric or Standard Oil. Such research might produce no more profound analysis of causes and would thus be no more useful to the movement than market research and an investigation of how to keep the native labor force happy have been for industry.

A radical rhetoric or ideological posture does not inevitably result in politically useful sociological work. Ideologically "correct" analyses cannot substitute for cogent, empirically verified knowledge of the world as a basis for effective action. Ideological radicalism cannot provide a workable understanding of the relative roles of China and India in the devel-

opmental process of Asia. Ideological radicalism cannot tell us how long it takes to make the transition from rural to urban life. Ideological radicalism cannot prove the merits or demerits of one or another form of economic investment. When radicalism without sociology is employed as a surrogate for truth, it becomes fanaticism—a foolish effort to replace substance with style. But when these limits are understood and expressed, sociological radicalism can help us measure the distance between where people are and where they want to go—between the society and the utopia.

In a period of railing and ranting against the social sciences, it is perhaps time once again to raise the matter of priorities for our age. It is the purpose of a meaningful sociology to demonstrate how it is that society and its institutions are on trial, and how it is that society and its organizations are undergoing crisis. When we keep this in mind and remember that sociology is part of society, and that sociology in itself means very little apart from the larger social tasks, then perhaps the sense and style of radical sociology will be enhanced, adding flesh and blood to its current programatics and calling us back once again *first* to the criticism of society and only *second* to the criticism of other sociologists.

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The Politics of American Sociologists

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Sociology has recently been subject to a severe critique by Alvin Gouldner (1970), who has repeated and sought to document the charge that the theoretical orientations fostered by Talcott Parsons carry conservative implications and have been dominant within the field. Such arguments now fall on fertile soil. A large number, especially among the younger and more left-wing sociologists, echo the claim that the major trends in the field sustain a conservative view of society and are basically biased against radical social change. The caucus of left-wing sociologists, the Sociology Liberation Movement, has been perhaps the most aggressive and critical among those leftist academics within various professional associations (Brown 1970; Roach 1970; Nicolaus 1969).

At the same time, however, as Gouldner himself has noted, one observes "the prominent role of young sociologists in current student rebellions" (Gouldner 1970, p. 10). Sociology has provided more support for student militancy than any other discipline. Daniel Cohn-Bendit, who was himself a student of sociology at the University of Paris-Nanterre (one of the few institutions at the time in France which had a full-blown sociology department), asserts that "student agitation since 1960, abroad as in France, has been rife among sociologists far more than among other social scientists and philosophers. . . . The case was similar in the U.S.A., in France, in Germany, and also in Poland and Czechoslovakia" (Cohn-Bendit and Cohn-Bendit 1968, p. 47). On the other "side," David Riesman has lamented that "the field is becoming so politicized it's hard to bring sober people into it. Sociology is the soft underbelly [of the academy which is] the soft underbelly of society. It is interesting that all over the world student revolutionists have been led by sociologists; from Tokyo to the Free University of Berlin, sociologists have been the vanguard" (Riesman and Harris 1969, p. 63).

Long before the rise of the contemporary New Left, moreover, German sociology stood out as a leftist discipline amidst the general conservatism and even right-wing sentiment of professors in the Weimar period (Eschenburg 1965). There is, then, at least a superficial conflict between Gouldner's charge of ascendant conservative orientations in sociology and the picture of the field as the most activist and change oriented in academe. We shall here try to unravel the sources of this contradiction by examining

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the impressive body of survey data that explores the actual political views of sociologists.

ATTITUDES TOWARD NATIONAL POLITICS

In the 1950s and 1960s, a number of surveys of the views of social scientists—on civil liberties, party identification and voting behavior, and on politics generally—showed sociology to be among the most liberal fields in academe. The national survey of social scientists (1955) under the direction of Paul Lazarsfeld and Wagner Thielens found sociologists to be more disposed to vote for liberal and left candidates such as Truman, Wallace, and Thomas in 1948, and Stevenson in 1952 (Lazarsfeld and Thielens 1958). Along with social psychologists, sociologists were the most opposed to firing faculty for membership in the Communist party and were most likely to think of themselves as left of the rest of the faculty (table 1).

Another series of studies made between 1959 and 1964 found sociologists in the vanguard on several different measures of liberalism. Spaulding and Turner reported that sociologists (78%) identified as Democrats slightly more often than political scientists, historians, and psychologists, and much more than physical and biological scientists (for example, botanists 50%) and the applied fields (engineers, 27%) (Spaulding and Turner 1968). This support of Democrats reflects a generally liberal to left orientation. Using a 14-item index to measure the liberal-conservative dimension, only 12% of sociologists emerged as conservatives compared with 51% of botanists, 61% of geologists, and 66% of engineers. Similar findings which placed sociologists in the forefront of other disciplines were reported by Ladd in a study of academics who signed newspaper statements opposed to the Vietnam War and in questionnaire responses of a small sample of social scientists who signed such advertisements. The sociologists among them were most disposed to favor student activism (Ladd 1969, 1970).

This picture of the politics of sociologists, based on limited samples, may now be elaborated through an analysis of data from a comprehensive survey of 60,000 academics.¹ The questionnaire contained more than 300 items of information covering social background, professional activities and achievements, and opinions about issues and controversies ranging from those exclusively within the academy to matters of national and

¹ This is a massive survey of college faculty conducted in the spring of 1969 with the financial support of the Carnegie Commission and the United States Office of Education, Department of Health, Education, and Welfare. We wish to acknowledge our debt to Dr. Clark Kerr, chairman of the Carnegie Commission, Professor Martin Trow of the University of California, Berkeley, who directed the administration of the survey, and to their colleagues. The interpretations expressed in this publication are, of course, solely our responsibility.

TABLE 1

POLITICAL POSITIONS OF AMERICAN SOCIAL SCIENTISTS, BY DISCIPLINE
(LAZARSFELD-THIELENS DATA; AS PERCENTAGES OF *N*)*

| | All Fields (<i>N</i> = 2,451) | Sociology (<i>N</i> = 405) | Social Psychology (<i>N</i> = 141) | Political Science (<i>N</i> = 384) | History (<i>N</i> = 681) | Economics (<i>N</i> = 565) | Geography (<i>N</i> = 160) |
|--|-----------------------------------|--------------------------------|---|---|------------------------------|--------------------------------|--------------------------------|
| Are you more liberal or more conservative than most of the faculty here [respondent's university]? | | | | | | | |
| More liberal | 39 | 49 | 43 | 42 | 37 | 37 | 22 |
| Same | 39 | 36 | 38 | 33 | 39 | 41 | 50 |
| More conservative | 12 | 6 | 10 | 15 | 12 | 13 | 17 |
| Don't know | 9 | 8 | 9 | 9 | 11 | 9 | 11 |
| Should an admitted Communist teach- ing in a college be fired? | | | | | | | |
| Yes | 46 | 38 | 38 | 46 | 51 | 43 | 61 |
| No | 36 | 42 | 48 | 32 | 31 | 39 | 23 |
| Don't know | 18 | 19 | 14 | 22 | 18 | 17 | 16 |
| 1948 Vote† | | | | | | | |
| Dewey | 28 | 19 | 24 | 27 | 32 | 28 | 28 |
| Truman | 63 | 70 | 62 | 66 | 59 | 66 | 63 |
| Wallace | 4 | 6 | 9 | 2 | 3 | 4 | 4 |
| Thurmond | 1 | 1 | ... | 1 | 1 | ... | 1 |
| Other (mainly Socialist) | 4 | 5 | 5 | 4 | 4 | 2 | 4 |
| 1952 Vote† | | | | | | | |
| Eisenhower | 34 | 26 | 42 | 27 | 33 | 36 | 61 |
| Stevenson | 65 | 73 | 55 | 73 | 67 | 63 | 39 |
| Others | 1 | 1 | 3 | ... | ... | ... | ... |

* The data in this table were obtained from a secondary analysis of the data set made available to us by the Bureau of Applied Social Research of Columbia University.
† Nonvoters excluded from the computations.

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international affairs. Just 1.7% (1,036 persons) of the respondents are sociologists, which is about 20% of those teaching full time in sociology in American colleges and universities.²

In general, these data indicate that while liberal to left propensities are characteristic of all social scientists, there is a progression to the right from the social sciences to the humanities to the natural sciences, and an even stronger progression to the right by the applied fields with a close connection to economic enterprises—business administration, engineering, and agriculture. While they closely resemble their associates in the other social sciences on national questions, sociologists are almost invariably somewhat to the left. For example, only 6% describe their political views as conservative, as do 12% of all social scientists, 27% of the entire faculty, 41% in engineering, and 50% in agriculture.³ In the 1968 election, only 13% of the sociologists voted for Nixon, and in 1964 only 6% opted for Goldwater. Among all social scientists, support for these candidates was slightly higher, 19% in 1968 and 10% in 1964. On the other hand, Nixon received the votes of 41% in the natural sciences, 45% in medical schools, 56% in business administration, 58% in engineering, and 62% in agriculture.

When the survey was conducted in early 1969, about one-third of the sociologists supported the immediate withdrawal of American troops from Vietnam while another half believed the government should decrease its involvement and encourage the formation of a coalition government in South Vietnam (83% for both), compared with 26% and 49% for all social scientists. These figures should be contrasted with others who favored either immediate withdrawal or the encouragement of a coalition government: 73% in English, 67% in physics, 58% in chemistry, 55% in education, 44% in business, and 36% in agriculture.

The extent to which sociologists differ even from other social scientists in commitment to a liberal-left position is pointed up by the data in table 2. Three scales—"Liberalism-Conservatism," "Campus Activism," and "Black Support"—comprise pertinent items that cover an immense amount of ground, from Vietnam to the hiring of black faculty, yet it is striking that the various disciplines occupy the same relative position in each.⁴

² Sample and weighting procedures allow us to generalize from the survey's respondents to the entire full-time faculty in the United States. For a complete description of the sampling and weighting procedures and for a copy of the questionnaire with marginals, see Bayer (1970).

³ The question to which they responded was: "How would you characterize yourself politically at the present time?" The alternatives posed were "left," "liberal," "middle of the road," "moderately conservative," and "strongly conservative."

⁴ The dimensions used for analysis were derived from a factor analysis and orthogonal rotation. The basis for the scales is described briefly elsewhere (Ladd and Lipset 1971b, pp. 137-38). The texts of the questions in the scales, and the construction of

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TABLE 2

FACULTY POSITIONS ON CAMPUS ACTIVISM, BLACK SUPPORT, STUDENT ROLE,
AND LIBERALISM-CONSERVATISM SCALES BY FIELD (AS PERCENTAGES OF *N*)

| Field | Liberalism- Conservatism Scale— Percentage Very Liberal and Liberal | Campus Activism Scale—Percentage Strongly Supportive and Moderately Supportive* | Black Support Scale—Percentage Strongly Supportive and Moderately Supportive |
|-----------------------------------|---|---|--|
| Sociology (1,033) | 72 | 72 | 58 |
| Social work (510) | 71 | 60 | 62 |
| Political science (1,267) | 61 | 63 | 49 |
| Psychology (2,103) | 62 | 59 | 48 |
| Anthropology (421) | 64 | 55 | 41 |
| Economics (1,490) | 57 | 52 | 40 |
| All social sciences (7,122) | 63 | 61 | 48 |
| Humanities (9,546) | 55 | 52 | 42 |
| Law (611) | 51 | 46 | 38 |
| Fine arts (3,475) | 45 | 43 | 41 |
| All fields† (52,364) | 41 | 40 | 34 |
| Education (3,277) | 32 | 39 | 37 |
| Physical sciences (7,599) | 38 | 35 | 28 |
| Medicine (2,384) | 38 | 34 | 31 |
| Biological sciences (4,403) | 35 | 34 | 28 |
| Business (2,080) | 20 | 25 | 21 |
| Engineering (4,165) | 24 | 24 | 21 |
| Agriculture (1,348) | 13 | 16 | 19 |

NOTE.—*N* in parentheses.

* Includes the percentage of the field with scores in the range of the two most supportive (liberal) quintiles for the faculty as a whole.

† Some 7,664 respondents did not answer the question, "What is your principal teaching field?" and are excluded from the total. Included in the total are some fields not shown in this table.

RELEVANCE OF SURVEY ANALYSIS TO THE GOULDNER CRITICISM

Some radical critics of sociology, who are generally impatient with the use of survey data to deal with political questions, may argue that these data do not bear on the central theoretical emphases in the discipline. They contend that the dominant theme of sociology since World War II has been functional analysis (an approach inherently concerned with problems of system maintenance), that adherents of functional analysis have controlled the major positions in the field and led it away from social problems and involvement in social change. These criticisms have been most

each scale are available on request. We computed the raw scores for all 60,000 faculty members in the Carnegie sample on each of the scales—from most liberal to most conservative or from the most supportive of student activism and of the demands of blacks, to the most opposed—and then collapsed the raw scores into five approximately equal categories: that 20% of the faculty with the most liberal (supportive) responses, on down to the 20% most conservative (opposed). If the percentage in a field classified as very liberal exceeds 20%, then a larger proportion of this field is very liberal than of the whole professoriate.

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definitively set forth in Professor Gouldner's book, *The Coming Crisis of Western Sociology*.

Therefore, in seeking to evaluate the worth of surveys as a means of understanding "domain assumptions," it should be noted that the largest single such survey of American sociologists was conducted by Gouldner and his then doctoral student, J. T. Sprehe, in 1964. They collected data from 3,400 members of the American Sociological Association (ASA) on 89 attitude items plus assorted other questions dealing with politics and the profession. Gouldner (1970) has defended investigations such as his in terms we would second completely:

Some methodological purists might object that such questions cannot be answered, or are "meaningless," or are lacking in specificity. Basically, however, such an objection either rests on the assumption that sociologists are fundamentally different from other human beings and do not hold the same kind of vague and "unproven" beliefs that others do, or else it wishes to blur the issue, which is an empirical one, with the irrelevant notion that sociologists *should* not have such beliefs. But, if our approach needs any defense, it was one of the elemental findings of our research that sociologists seem to have no more difficulty than anyone else in answering such broad questions, and, like other men, they do indeed hold the kind of beliefs that I have characterized as domain assumptions. [P. 36]

Gouldner relies heavily on the responses to one of the 89 attitude items to justify his contention that the work of Talcott Parsons has been pre-dominant:

In the United States, where I believe Parsons' influence has reached its apogee, his work retains a considerable audience, and its standpoint still commands considerable respect. Thus, in the 1964 survey that Timothy Sprehe and I conducted among American sociologists . . . we asked these men to express their views on the following statement: "Functional analysis and theory still retain great value for contemporary sociology." Some eighty percent of the responding sociologists expressed agreement with it in varying degrees of intensity. We must thus center our discussions of the present state of Academic Sociology on Talcott Parsons' theory. [P. 168]

It is unfortunate that although the Gouldner-Sprehe survey is the largest ever conducted of sociologists, very little from it has ever been published. Apart from the one variable that Gouldner mentions, the only publication was a short article which appeared soon after the data were collected, reporting mainly on the marginals for most of the attitude items (Gouldner and Sprehe 1965). In Sprehe's dissertation (1967), he factor analysed the 89 attitude items. An examination of the marginals for various indices measuring these factors supports the conclusions of other surveys that, as a group, sociologists hold leftist positions. For example, he finds the "sample as a whole scored towards the radical side on the Index of Conservatism-Radicalism" (p. 321). Respondents were asked to list the three

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most pressing social problems facing the United States and whether "the solution would require basic change in American social structure and values." As of 1964, race relations was perceived as the most pressing issue, followed by unemployment, mental health, and urban problems. "Only 13.3% felt little change in basic structure or values would be necessary to solve these problems; 10% were at the midpoint, 76.7% scored on the side of 'Basic Change in Structure and Values'" (pp. 264-65).

Replies to statements about sociology suggest, moreover, that despite agreement by 82% that "functional analysis and theory still retain great value for contemporary sociology," the large majority of sociologists do not accept a Parsonian view of the world or of sociology, nor do they define functionalism in conservative terms.

From the data in table 3 it appears that most sociologists in 1964 found merit in functional analysis, in focusing on social problems, in the use of mathematics, and in humanistic approaches. Over three-quarters of them also looked for basic changes in the "structure and values" of society to solve major social problems. Contradicting what one might assume would

TABLE 3
OPINIONS OF AMERICAN SOCIOLOGISTS (GOULDNER-SPREHE STUDY, 1964)
(%)

| Item | Agree | Uncertain | Disagree |
|---|-------|-----------|----------|
| Functional analysis and theory still retain great value for contemporary sociology | 82.4 | 7.7 | 9.9 |
| Some of the most powerful theories in sociology have emerged from the study of social problems | 75.6 | 12.5 | 11.9 |
| Emphasis on methodology too often diverts sociologists from a study of society to the problem of how to study society | 61.0 | 10.4 | 28.6 |
| The coming generation of sociologists will need much more training in the use of higher mathematics | 80.0 | 7.5 | 12.5 |
| Sociology should be as much allied with the humanities as with the sciences | 58.0 | 9.9 | 32.1 |
| The problems of modern society are so complex that only planned change can be expected to solve them | 62.2 | 11.4 | 26.4 |
| By and large, social problems tend to correct themselves without planned intervention | 7.7 | 6.4 | 85.9 |
| Many modern social institutions are deeply unstable and tensionful | 61.0 | 14.8 | 24.2 |
| The sociologist, like any other intellectual, has the right and duty to criticize contemporary society | 91.1 | 4.4 | 4.5 |
| One part of the sociologist's role is to be a critic of contemporary society | 70.6 | 10.0 | 19.4 |

SOURCE.—Sprehe 1967, pp. 235, 236, 241, 247, 258, 259, 221.

be the beliefs of professionals committed to Gouldner's image of functionalism, a large majority thought that many modern social institutions "are deeply unstable and tensionful," and that a sociologist should be "a critic of contemporary society." If Parsonian sociology has indeed been as conservative (system-maintenance oriented) as Gouldner argues, how could it have been so influential in a profession so concerned with system instabilities and tensions and with the need for radical change?

One possible answer is that the dominant sociologists—those who controlled the prestigious departments, who secured the largest research funds, who led the ASA—were indeed adherents of Parsonian sociology, did hold conservative functionalist beliefs, and did oppose a political activist, social change orientation. Dominant minorities clearly wield much more social power than does majority opinion as recorded in opinion polls. And the Gouldner-Sprehe survey did make this assumption about sociology. Sprehe observed that "there [is] a group of persons who informally dominate any social system." He sought to identify "dominant groupings within sociology and to examine their ideological leanings" (p. 150). Respondents were classified as members of the "dominant group" by such criteria as whether they came from a prestigious department, were employed in a large secular university, were tenured, had significant sums of research money, published often in professional journals, held office in professional associations. Sprehe states that the study began with the hypothesis that "dominant" sociologists would show positive orientations toward an emphasis on "Scientific Method," "Value Freeness" in social research, "Professionalization," and "Self-Image." Conversely, the "dominants" would tend "to score low on Optimism and Radicalism." That is, the more prestigious and highly rewarded sociologists should be more conservative, less optimistic about the possibilities of social reform, more supportive of a scientific view of the discipline, of the idea that social science research can and should be value free, and for establishing formal professional criteria for membership in the discipline. The dominants also "were expected to score low on Societal Roles" (involvement in action groups) because "those espousing the ideology of dominant sociology [should] engage principally in the work of sociology itself and not personally concern themselves with political or social action" (pp. 151-54).

Unfortunately, the results of the analysis did not confirm these hypotheses. The one indicator of "dominance" which was most frequently correlated with conservative academic and political views was academic rank, but, as Sprehe noted, this variable is strongly associated with age. Repeatedly, the precise opposite of what the investigators predicted occurred: As some of Sprehe's conclusions make clear, the dominants held the more left-of-center positions:

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Respondents from smaller schools tended to hold the concept of applied sociology in disfavor while those from larger schools scored higher on Societal Role. . . . In general, it appeared that the more research funds a respondent claimed to be responsible for, the higher he scored on Societal Role. . . . Those who participated heavily in professional associations tended to score high on Societal Role. . . . The higher the score on Societal Role, the lower the predicted score on dominance measures. For the variables, Prestige School of Origin, Size of School, Research Funds Responsible for, and the Indexes of Periodical Publication and Professional Participation, the . . . hypothesis appears disconfirmed. For the first three variables named immediately above, the relationship is apparently opposite to that predicted. . . . In summary, as regards general, diffuse beliefs concerning the role of sociology in solving society's problems, the over-all relationships seem to be: the higher the score on dominance measures the . . . higher the factor score. [Pp. 301-3]

The factors of "Value Freeness" and "Pure Sociology" were related inconsistently or inconclusively to the indicators of dominance, but "the more research funds a respondent is responsible for, the more likely he was to score low on Value Freeness. The statistical relationship was the strongest for any considered of this factor" (Sprehe 1967, p. 305). The investigators had posited that professionalization, that is, desire to limit membership in the ASA and set up formal qualifications, would be correlated with indicators of dominance within the profession. Again, Sprehe says "the . . . hypothesis . . . was largely disconfirmed." There was, in fact, some indication that low-status and aspiring sociologists ("respondents from non-prestige schools" and "nontenured faculty") were "more in favor of professionalizing sociology," while curiously, "the greater the amount of research funds a respondent had, the more likely he was to score low on Professionalization" (p. 314). "The . . . hypothesis for the Index of Radicalism stipulated that the dominant sociologists would be low scorers." This was also "largely disconfirmed" (pp. 312-13). The only measure of dominance which correlated positively with radicalism was the age-related factor of academic rank. With respect to possession of research grants, "radicalism tended to increase . . . as the amount of research funds grew larger, except for the very highest category" (pp. 327-33).

Since increasing age was generally accompanied by a more conservative position on academic and political issues, controlling for age should reduce any association of the dominance indicators with a conservative position and enhance the relationships with a liberal, radical, or activist position. In the body of his dissertation, Sprehe did not deal with the age variable, but in a later brief chapter some age-controlled relationships were presented. For the most part, these relationships strengthened the associations between dominance and an activist, reformist view of sociology and society—particularly among sociologists over 40 (pp. 446-51).

These unpublished results of the Gouldner-Sprehe study present a

picture of sociology that is consistent with other surveys, including the recent Carnegie one. At the same time, their findings sharply contradict many of the assumptions about dominant trends within the discipline which Gouldner uses to justify his detailed polemical criticisms of Parsons.⁵

The 1964 survey was explicitly designed to locate the "domain assumptions" of sociologists. Yet all through *The Coming Crisis of Western Sociology* (1970) Gouldner continues to identify as the "domain assumptions" of sociology positions which, in 1964, he had found were not adhered to by the majority of sociologists and were particularly rejected by the most productive and most rewarded scholars.

It may be worth noting that a much smaller "survey" of "30 outstanding sociologists at . . . Columbia, Harvard, Boston, Brandeis, Chicago, University of Michigan in Ann Arbor, and University of California in Berkeley" who were interviewed in depth in 1963-64 by a Yugoslav sociologist, Mihailo Popovich, yielded results highly congruent with the Gouldner-Sprehe findings. Twenty-two of the 30 who presumably were all among Gouldner's dominants did not think that there is any "general theory which is dominating or prevailing in today's American sociology." When asked "which problems are among the most important in contemporary sociology," the largest number (10) mentioned "social change"; next in order of frequency (five) was "social problems of economic development." Only three mentioned "problems of social integration." Most strikingly, when these 30 "outstanding sociologists" were asked, before the recent wave of campus activism and revived left ideology, about the relationship of Marxism to other sociological theories, most found a considerable overlap in approach and concerns. As Popovich (1966) reported, "it is a significant fact that almost all of the interviewed sociologists think that there are some 'common points' between Marxist theory and non-Marxist sociological theories. These common points concern not only certain categories or principles, but also some problems. As is pointed out above, problems of social change and economic development are mentioned the most as *the* important issues of modern sociology. Are not they the problems with which Marxist sociologists mainly deal, at least on a theoretical level?" (p. 135).

⁵ Some readers may think that we are unfair in identifying Gouldner with a study whose primary product is an unpublished (though publicly available) manuscript written by J. T. Sprehe. We should note, therefore, that Gouldner refers to this study in the following terms in his book: "In a study of the American Sociological Association, Timothy Sprehe and I polled its 6,762 members"; "the national opinion survey of American sociologists conducted by Timothy Sprehe and myself"; "Thus in the 1964 survey that Timothy Sprehe and I conducted among American sociologists, . . . we asked . . . the following"; "The findings of the national survey of American sociologists that Timothy Sprehe and I conducted in 1964. In this survey we sought"; "As previously mentioned, the national survey of American sociologists conducted by Timothy Sprehe and myself asked them" (Gouldner 1970, pp. 24, 36, 168, 247, 377).

OTHER COMPONENTS OF THE GOULDNER CRITICISM

Since the Gouldner-Sprehe survey did not inquire directly about the influence of Talcott Parsons or of any other sociologist, Gouldner sought indirect measures of Parsons's impact to justify his criticism of the work of a man whom he considers "more Delphically obscure, more Germanically opaque, more confused and confusing by far than . . . any other sociologist considered here or, indeed . . . any whom I know" (p. 200). In fact, statistical data on the scholarly influence of Parsons and other leading functionalists do exist in studies of the frequency with which various individuals are cited in the literature of the field. All such surveys indicate that Parsons and Merton are invariably the two most cited modern sociologists (Oromaner 1969, 1970; Bain 1962). Though these indicators of intellectual influence are ignored by Gouldner, other supposed evidence of Parsons's organizational or political control are given, namely that some of his students have played "dominant roles as officers of the American Sociological Association and as editors of its journals" (Gouldner 1970, p. 168). In fact, an examination of the editors of the *American Sociological Review* and of the contests for president of the ASA since World War II suggests that Gouldner is mistaken. Only one editor, Neil Smelser, was a student of Parsons or any other exponent of functionalism. Most, in fact, were severe critics of the functionalist approach, as the following list indicates:

1946-48, Robert C. Angell
1948-51, Maurice R. Davie
1952-55, Robert E. L. Faris
1955-57, Leonard Broom
1958-60, Charles H. Page
1960-62, Harry Alpert
1963-65, Neil Smelser
1966-68, Norman B. Ryder
1969-present, Karl F. Schuessler

The results of the contests for the presidency of the association also challenge the view that the rank-and-file membership followed Parsons. The first two times he ran for the presidency he was defeated by Louis Wirth and by Franklin Frazier, both of whom represented a clear social-problems, nonfunctionalist viewpoint. Parsons defeated Thorstein Sellin, the criminologist, in his third effort. His close friend and Harvard colleague, Samuel Stouffer, was beaten on his first try by Robert Cooley Angell but was elected in 1953 over Florian Znaniecki. A direct confrontation in 1953 between a functionalist, Robert Merton, and an SSSP (Society for Study of Social Problems) proponent, Herbert Blumer, produced a victory for Blumer. Merton was elected the following year, followed in succession

by two other students of Parsons, Robin Williams and Kingsley Davis. These contests, from 1954 to 1957, were the high point for the functionalists of the Columbia-Harvard school. Then Paul Lazarsfeld, regarded by critics as an exponent of a value-free or "scientistic" approach, was twice defeated, by Howard Becker and Ellsworth Faris. Lazarsfeld won on his third try against the same Thorstein Sellin who had been beaten by Parsons. Another prominent functionalist student of Parsons, Wilbert Moore, also had two electoral defeats, from Everett Hughes and Pitirim Sorokin, before finally winning against Philip Hauser in 1964. However one interprets these results, they certainly do not add up to domination of the field by Parsons and the "pure sociology" approaches allegedly represented by Harvard and Columbia sociologists.

In spite of the fact that Parsons's students and collaborators have not played "dominant roles as officers of the American Sociological Association and as editors of its journals," "there can be no question that Parsons has had more impact on sociology than any other modern scholar. Yet the Gouldner-Sprehe survey and our own findings in the Carnegie study sharply challenge Gouldner's conclusions that a commitment to functionalism, and particularly to Parsons's version of it, has served to conservatize the discipline. It might even be reasonable, with the needed research, to question Gouldner's contention that as a result of lifelong antisocialist orientations, Parsons occupied himself with undermining Marxist and other radical thought. Unfortunately, Gouldner has done little or no serious research on Parsons's early academic career and seriously misinterprets some of his initial publications and scholarly activities.

Far from being an apolitical or conservative student, at Amherst College Parsons was a member of the Student League for Industrial Democracy (SLID), a direct ancestor of the Students for a Democratic Society (SDS)—a membership which various histories of the league and of its student affiliates have proudly noted over the years. Both SLID and SDS were formed as affiliates of the adult league. Parsons published his first article in what was the major left student magazine of the 1920s, *The New Student* (Cutler and Parsons 1923). In a joint article with another Amherst undergraduate, Addison Cutler (later a Communist intellectual who published frequently in the Marxist magazine, *Science and Society*), Parsons discussed the factors related to the firing of Alexander Meiklejohn, the liberal academic and reformist president of Amherst. He noted that "there was a very definite split in the faculty . . . [among] Old Guard, New Guard, middle ground men" (Cutler and Parsons 1923, pp. 6-7). Parsons subsequently went abroad to study at the London School of Economics (LSE) in part because of its image as a center of socialist scholarship.

It is interesting that although Gouldner does not refer to Parsons's undergraduate activities or his choice of the LSE for study, he does

analyze in some detail Parsons's first scholarly paper as part of his effort to show that Parsons was always fighting socialism. To demonstrate that Parsons's early interest in three "anticapitalist" thinkers (Marx, Sombart, and Weber) represented a defense of the established order, Gouldner (1970, pp. 178-84) says that "Parsons believed modern society could be gradually perfected *within* the framework of capitalism: that is 'on the basis which we now have'" (p. 183). It is clear from the original article of Parsons that this is not so. In the context of criticizing Sombart and agreeing with Marx, Parsons (1928) wrote: "There seems to be little reason to believe that it is not possible *on the basis which we now have* to build by a continuous process something more nearly approaching an ideal society. In any case the process of social change is certainly neither so radically discontinuous nor so radically determined by any 'principles' as Sombart would have us believe. *In the transition from capitalism to a different social system* surely many elements of the present would be built into the new order. This is precisely *what socialism wishes to do*, retaining all the technical progress of capitalism" (p. 653) (italics ours).

In his effort to identify the conservatism of the sociological interests of the young Parsons, Gouldner discusses his membership in the Pareto Circle, a seminar of faculty and graduate students which met regularly at Harvard from 1932 to 1934. Gouldner (1970, pp. 148-51) uses selective "guilt by association" to demonstrate that membership in the circle implied conservatism. By citing statements of L. J. Henderson, the chairman of the group, George Homans, and Crane Brinton, he presumably shows that each became interested in Pareto as an outgrowth of a conservative philosophy, and attributes their supposed motivations to Parsons as well.⁶ But while Henderson and Homans were political conservatives, other members of the circle were involved in liberal and even left-wing activities and associations. Brinton, for example, was an early member of the Harvard Teachers' Union, then under heavy attack as allegedly controlled by the Communists. Gouldner does not refer to the membership or politics of Henderson's senior colleague in the seminar, Charles P. Curtis, Jr., who subsequently wrote a book with Homans about Pareto. Although Curtis was a member of the Harvard Corporation (its governing board), he was quite surprisingly a "liberal and New Dealer," according to his coauthor. In his discussion of the circle, Gouldner notes that "also attending were R. K. Merton, Henry Murray, and Clyde Kluckhohn," but says nothing

⁶ Gouldner relies heavily for his information and quotations referring to the Pareto Circle on an article by Barbara S. Heyl (1968). This article is taken from a master's thesis written at Washington University which we have not read. It should be noted that Miss Heyl carefully differentiated the influence of Pareto on Henderson, Homans, and Brinton, from that on Parsons. She noted that Parsons "did not embrace the Paretan social system and equilibrium concepts as immediately or as completely as did the others" (p. 333).

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about their politics. Since all of them have been close collaborators of Parsons and belonged to the circle, an inquiry about their politics might have been fruitful.

The record seems clear that Robert Merton, as a young faculty member at Harvard, was deeply interested in assorted left-wing causes and ideas. One aspect of his Cambridge friendship pattern is reported in an autobiographical work by Granville Hicks, then a close friend of Merton's, a member of the English department, and deeply and publicly involved in the Communist party as its chief literary spokesman (Hicks 1965, pp. 170, 172, 174, 175). Merton and Hicks had been active in the Harvard Teachers' Union. Merton published one of his early papers on science in the Marxist journal, *Science and Society*, in 1939, and a subsequent eulogy of the magazine's longtime editor, Bernhard J. Stern, in 1957. As executive officer of the Columbia sociology department during the early 1950s, Merton played a major role in defending Stern against attacks stemming from Senator Joseph McCarthy. In 1952, the New York *Daily News* attacked Merton together with other Columbia sociologists, Robert MacIver and Paul F. Lazarsfeld, as "reds" and "pinks." Clyde Kluckhohn also had liberal to left sympathies, according to those who knew him. This may be seen from the fact that, among other things, he wrote two highly sympathetic reviews (1946, 1955) of books by Marxist scholars, Vernon Venable and Herbert Marcuse. His description of *Eros and Civilization* as a "stirring" and "significant" book brought Marcuse's work to general intellectual attention at a time (1955) when such praise of the work of Marxists in the mass media was rare. Henry Murray reports that as a young student of psychology he was totally "apolitical."

In citing some of the "leftist" links of some members of the Pareto Circle to counter Gouldner's attempt at "guilt by association," we are not trying to substitute a form of "absolution by association" that would be equally illogical and irrelevant. But the kind of quick imputation of political orientation by affiliation or friendships can be seen as meaningless by demonstrating how easy it is to find contradictory evidence of the kind on which the imputation is based. The conservative or leftist views of members of the Pareto Circle have no bearing on an attempt to characterize Talcott Parsons, nor do they help to explain why he joined George Homans and Robert Merton in the seminar.

It is worth noting in this connection that the principle of "guilt by association" was applied in the fifties to Samuel Stouffer, who was told when he appealed a denial of clearance by a federal agency that one of the negative facts on his record was his close personal association with Talcott Parsons. Lest this article become an exercise in correcting errors about Parsons's biography, we will leave things here in the hope that we have demonstrated that Gouldner's inferences from Parsons's career display

the same weakness as those he drew from survey data and the history of the leadership of the ASA. (Parenthetically, we would note that Gouldner was also in error [1970, p. 15] when he reported that C. Wright Mills "never became a full professor" as evidence that a radical outlook has blocked chances for academic rewards. As a matter of easily accessible fact, Mills became a full professor at Columbia in 1956.)

LEFT-OF-CENTER POLITICS OF THE INTELLECTUAL ELITE

In beginning their research with the assumption that the "dominants" would be more supportive of left views than the "rank and file" sociologists, Gouldner and Sprehe had apparently been unaware of earlier surveys which found that the most successful academics were seldom "conservative" in any accepted sense but were rather the most liberal or left-oriented faculty. The earliest studies of faculty religious beliefs conducted by James Leuba, a psychologist (1921, 1950) in 1913-14 and again in 1933 revealed that the more distinguished professors, both among natural and social scientists, were much more irreligious than their less eminent colleagues. Leuba sampled members of the ASA both times and found that academic members were more inclined to atheism or "liberal" religious beliefs than the nonacademic, and that the most creative sociologists (as judged by a panel) were the least religious. Only 19% of the "greater" sociologists reported a belief in God in 1913, by contrast to 29% of the "lesser" and 55% of the nonacademic members of the ASA (Leuba 1921, pp. 262-63). Although religious and political beliefs are clearly different, many investigations have shown that, among Americans, religious unbelief is associated with liberal to left political values.

The Lazarsfeld-Thielens study of social science opinion mentioned earlier found a clear relationship between scholarly productivity and propensity to vote Democratic, and to see themselves as further left in their views. Moreover, "the proportion of productive scholars rises as we move from the very conservative to the very . . . [liberal] respondents, with respect to opinions on academic freedom" (Lazarsfeld-Thielens 1958, pp. 17, 144-46). Secondary analysis of the data shows that the more productive sociologists are clearly to the left of those less involved with research and lower on indicators of achievement (table 4).

More recent studies support the same conclusion. Eitzen and Maranell (1968), in their national survey of party affiliation, found a comparable relationship (p. 150). Ladd (1969) showed that the signers of anti-Vietnam War petitions were quite disproportionately academics of higher rank and status. His findings for the faculty generally, have been confirmed for sociology in a study of the characteristics of the 1,300 who signed the

TABLE 4

POLITICAL POSITIONS OF SOCIOLOGISTS, BY ACADEMIC STANDING AND ACHIEVEMENTS
(LAZARSFELD-THIELENS STUDY; AS PERCENTAGES OF *N*)

| Achievement of the Sociologist | Disagrees That a College Professor Who Is an Admitted Communist Should Be Fired | Considers Himself More Liberal than Most Faculty at His University |
|--|---|--|
| Number of publications: | | |
| None (104) | 33 | 42 |
| Three or more (239) | 46 | 50 |
| Number of books published: | | |
| None (258) | 39 | 46 |
| Some (145) | 45 | 52 |
| Number of papers delivered at professional meetings: | | |
| None (109) | 36 | 41 |
| Three or more (185) | 44 | 48 |
| Index of personal academic status:* | | |
| Low (131) | 38 | 42 |
| Medium (198) | 41 | 47 |
| High (76) | 51 | 53 |

NOTE.—*N* in parentheses.

* The index is based upon highest degree held, whether dissertation was published, whether the respondent had held office in a professional society, and whether he had served as a consultant.

"Open Letter to President Johnson and Congress" in November 1967 opposing the Vietnam War: "The overrepresented signer is male, an ASA Fellow with a Ph.D., primarily engaged in research or teaching at an academic institution located in the Northeast. These characteristics hardly describe the younger, less professionally socialized, and more alienated member of the profession. Rather, they point to a signer who is well integrated into the profession and who signs from at least an objective position of security and strength" (Walum 1970, p. 163).

Our own 1969 Carnegie survey of the politics of academics, not surprisingly, reinforces the findings of previous investigations. The more scholarly and highly achieving faculty appear significantly more disposed to left-liberal views than the professoriate generally (Lipset and Ladd 1970, 1971a; Lipset 1972a). In addition, sociologists who had, in the 12 months preceding the 1969 survey, served as paid consultants to some federal agency or who had held federal grants were much more opposed to United States policies in Vietnam and more supportive of an immediate U.S. withdrawal than were the rank and file of the profession. Those receiving federal research grants gave more support in 1968 for left-wing third-party candidates than their colleagues who had not received grants and furnished much less backing for Richard Nixon's candidacy. The reason for such

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TABLE 5

POLITICAL POSITIONS OF SOCIOLOGISTS, BY ACADEMIC STANDING AND ACHIEVEMENTS
(1969 CARNEGIE SURVEY; AS PERCENTAGES OF *N*)

| | VERY LIBERAL AND LIBERAL, LIBERALISM- CONSERVATISM SCALE | FOR IM- MEDIATE U.S. WITHDRAWAL FROM VIETNAM (SPRING 1969) | 1968 VOTE | | |
|--|--|---|---------------------|----------|-------|
| | | | Left Candidates* | Humphrey | Nixon |
| Achievers (140) | 85 | 48 | 8 | 90 | 2 |
| Consultants (173) | 80 | 40 | 8 | 85 | 7 |
| All sociologists (1,036) | 72 | 32 | 7 | 79 | 14 |
| Research support: | | | | | |
| Received federal grants during last year (287) | 82 | 43 | 8 | 86 | 5 |
| No federal grants (722) | 70 | 30 | 6 | 77 | 16 |
| Teaching vs. research: | | | | | |
| Primarily committed to research (484) .. | 83 | 39 | 9 | 82 | 8 |
| Primarily committed to teaching (532) .. | 68 | 28 | 6 | 77 | 17 |

NOTE.—*N* in parentheses.

* Includes Dick Gregory, Eldridge Cleaver, and the established minor parties of the left.

"curious" findings, of course, is that the federal government appoints as consultants and awards grants to a disproportionate number of high achievers—that segment of the academic community most disposed to left views. Achievers in sociology (defined in table 5 as those with five or more scholarly publications in the preceding two years and holding positions at major universities) are predominantly to the left of the general membership of the discipline on all measures of opinion on important issues.

Findings of a relationship between academic achievement and liberal social and academic views are all the more impressive if we recall the high correlation between age and opinions. More so than in extramural society, younger academics are much more liberal politically than their elders (table 6). Since older sociologists are more likely to have achieved "positions of dominance," the older dominants should show up as even more liberal when compared with their age peers who have been less productive or less involved in research—as indeed is the case. Thus, according to the Carnegie data, among sociologists 50 years of age and older, of those who received a federal research grant during the year prior to the survey, 70% rate as liberal or very liberal by contrast to 48% of those who did not have a grant. Sixty-four percent of the older members who had five or more publications in the previous two years were liberal compared with 43% of

TABLE 6

POLITICAL POSITIONS OF SOCIOLOGISTS, BY AGE STRATA
(1969 CARNEGIE SURVEY; AS PERCENTAGES OF *N*)

| Scale | Over 50 Years of Age (<i>N</i> = 205) | Aged 40-49 (<i>N</i> = 294) | Aged 30-39 (<i>N</i> = 358) | Under 30 Years of Age (<i>N</i> = 179) |
|--|---|------------------------------------|------------------------------------|--|
| Liberalism-conservatism: | | | | |
| Very liberal and liberal | 50 | 58 | 80 | 85 |
| Conservative and very conservative | 28 | 13 | 9 | 12 |
| Percentage liberal minus percentage conservative | +22 | +45 | +71 | +73 |
| Campus activism: | | | | |
| Strongly supportive and moderately supportive | 54 | 70 | 77 | 86 |
| Moderately opposed and strongly opposed | 23 | 24 | 6 | 6 |
| Percentage supportive minus percentage opposed | +31 | +46 | +71 | +80 |
| Black support: | | | | |
| Strongly supportive and moderately supportive | 24 | 56 | 58 | 70 |
| Moderately opposed and strongly opposed | 35 | 21 | 26 | 16 |
| Percentage supportive minus percentage opposed | -11 | +35 | +32 | +54 |

those who had not published. Older recipients of federal grants were more likely (36%) than those who had no government funds (24%) to favor immediate U.S. withdrawal from Vietnam, as of spring 1969. Clearly the "dominants," the men who supposedly controlled the field, were to the left of those who had lesser publication accomplishments and research resources.⁷

The association between academic status, left views, and propensity to be used as consultants or to receive grants from the federal government has affected the pattern of participation in assorted government-funded projects which have been under severe criticism from the left. Thus the ill-fated notorious Project Camelot (financed by the Department of the

⁷ It should be noted that the pattern varies somewhat with respect to opinion on campus controversies, relationships discussed in detail for the social sciences elsewhere. Many who are liberal or leftist on national and international issues, and who reject the idea that social science can be neutral, are not supportive of student activism or the demands for intramural student power. The antiestablishment-disposed, research-oriented faculty may be troubled by activist attacks on the research complex. But whatever factors are involved, it remains true that the correlations between academic achievement and left views do not hold up for campus issues (Ladd and Lipset 1971a).

Army to study "internal conflict" or revolution) was headed by the late Rex Hopper, a serious student and partisan of revolutions in Latin America, a man who was a strong public admirer of C. Wright Mills (Hopper 1964). Many of the sociologists who served the project have been active in assorted activist causes, a fact implicitly attested to by C. Wright Mills's literary executor, who reports his concern about whether to deal with the subject because so many of those involved with Camelot "were former students of mine, while yet others were and remain colleagues and warm acquaintances" (Horowitz 1967, p. vi). (Parenthetically, it may be worth noting that three of the best-known radical spokesmen at Berkeley, Harvard, and M.I.T. have been long-term recipients of personal grants from the defense department. It is "bad form" to mention such facts about the far left, but seemingly proper for some on the left to invent such items about those they seek to discredit or intimidate.)

THE INTELLECTUAL AS SOCIAL CRITIC

The relative liberalism of the dominant in academe must be viewed as a manifestation of the general tendency of achieving intellectuals to support a politics of social criticism (Lipset and Dobson 1972). What factors, then, inherent in the social role of intellectuals result in their persistent position as critics of the larger society, in their fostering what Lionel Trilling (1965) has perceptively called the "adversary culture"?

Thomas Hobbes, writing in the *Behemoth* in the mid-17th century about the English revolution, noted that "the core of the rebellion as you have seen by this, and read of other rebellions, are the universities." Whitelaw Reid, American abolitionist leader, in an essay on "The Scholar in Politics" (1873) described behavior in a variety of Western countries that led him to conclude that "exceptional influence eliminated, the scholar is pretty sure to be opposed to the established. . . . Wise unrest will always be their [the scholars'] chief trait. We may set down . . . the very foremost function of the scholar in politics, *to oppose the established*" (pp. 613-14) (italics ours).

Intellectuals, as distinct from professionals, are concerned with the creation of knowledge, art, or literature. Status within the occupation accrues from creation, innovation, from being in the avant-garde. Inherent in the obligation to create, to innovate, is the tendency to reject the status quo, to oppose the existing or the old as philistine. Intellectuals are also more likely to be partisans of the ideal, of the theoretical, and thus to criticize reality from this standpoint. The need to express the inner logic of their discipline, of their art form also presses them to oppose the powers—the patrons—who seemingly are philistines, who prefer continuity rather than change.

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A similar argument was made over half a century ago by Thorstein Veblen (1934) in an attempt to account for "the intellectual pre-eminence of Jews":

The first requisite for constructive work in modern science and indeed for any work of inquiry that shall bring enduring results, is a skeptical frame of mind. The enterprising skeptic alone can be counted on to further the increase of knowledge in any substantial fashion. This will be found true both in the modern sciences and in the field of scholarship at large. . . . For [the intellectually gifted Jews] as for other men in the like case, the skepticism that goes to make him an effectual factor in the increase and diffusion of knowledge among men involved a loss of that peace of mind that is the birthright of the safe and sane quietist. He becomes a disturber of the intellectual peace. [Pp. 226-27]

In their effort to explain why faculty at high-quality schools had the most liberal and left views, Lazarsfeld and Thielens (1958) pointed out that such institutions "attract more distinguished social scientists," and they pointed out that creativity is associated with "unorthodox views" about society (pp. 161-63).

The pressure to reject the status quo, is, of course, compatible with a conservative or right-wing position as well as with a liberal or left-wing one. In some European countries, intellectual opposition to the status quo has often taken the form of right-wing extremist critiques of democracy because it fostered a mass society in which the vulgar taste of the populace destroyed creative culture and in which populist demagogues undermined national values. Although right-wing intellectual criticism remains vital, it is clear that since the 1920s, in the United States and increasingly in other Western countries, intellectual politics have become left-wing politics. The American value system, with its stress on egalitarianism and populism, fosters challenges to the polity for not fulfilling the ideas inherent in the American creed.

This stress on the critical antiestablishment role of the intellectuals may imply more support for reformist and radical social objectives than actually exists within the professoriate. Obviously, only a small minority of American intellectuals are radicals or revolutionists, as is even more true in the polity generally. In a country where 1% or less of the electorate call themselves "radicals" and where leftist parties secure but a handful of votes, we cannot expect that intellectuals, no matter how much farther left they are than other groups, will contain a dominant revolutionary segment.

Of course, most of the opinions voiced in our Carnegie survey and in the Gouldner-Sprehe study can be classified as liberal, not radical or revolutionary. Yet evidence definitely suggests that there is a much higher proportion of radicals among sociologists than among any other occupa-

tional group. In 1948, 11% of sociologists favored Henry Wallace and Norman Thomas, in contrast with 8% among social scientists generally, and only 2% among the American electorate. More strikingly, perhaps, in 1968, left-wing third-party candidates were on the ballot in considerably less than half the states, and went unmentioned in most discussions of the election. Yet 7% of sociologists reported voting for them, compared with 4% among other social scientists, 2% among professors generally, and well under half of 1% in the electorate. Wherever the choices offered by the larger American political system are extended, sociologists have disproportionately supported the most left-of-center alternatives. In 1968, a larger proportion of sociologists than of any other discipline preferred McCarthy (66%) to Humphrey for the Democratic nomination.

In other countries, where there are more radical alternatives, sociology is in the forefront of academic backing for them. This has generally been true in eastern Europe, where sociology has stood out as the discipline most identified with opposition to Marxist orthodoxy and regime politics.

The one survey of sociologists in a non-Communist country with strong radical movements, Japan, reinforces our conclusion (Suzuki 1970, p. 368). This study indicated that most sociologists voted for the Left Socialists (pro-Peking), with the Communists receiving the next highest support. Only one in 10 voted for the "bourgeois" Liberal Democrats, the majority party in the country, while less than 10% chose the Democratic Socialists (pro-Western). Yet these predominantly radical Japanese scholars, when asked to name non-Japanese sociologists worthy of considerable attention, listed Talcott Parsons more frequently than anyone else (24%), with Robert Merton in second place (19%) (Suzuki, p. 383).

Even more striking in the Japanese results is the fact that preference for radical politics and for the sociology of Talcott Parsons and Robert Merton was strongest among the youngest scholars. Sociologists under 30 years of age gave more backing to the Communists (35%) than did any other age group, and they did not supply a single vote for either the Liberal Democrats or the Democratic Socialists. Conversely, those aged 54 and over were the most conservative—35% Liberal Democratic—with not one Communist voter among them. Yet endorsement of the two leading American exponents of functionalism (31% for both Parsons and Merton) was most frequent among the sociologists under 30, none of whom mentioned Karl Marx. The others who received the remaining endorsements from this youngest cohort were Dahrendorf, Fromm, Homans, Lipset, and Weber. Support for Parsons and Merton generally declined with age, and was lowest in the oldest age group.

A comparable link between support for socialist politics and praise for functionalist sociology may be found in British data. The 1964 sample survey of the British academic profession, conducted by A. H. Halsey and

Martin Trow (1971), indicated a relationship between discipline categories and political orientation very similar to that in the United States. Two-thirds of the social scientists (66%) reported supporting the Labour party, while 70% defined their politics as "left." Other fields showed much less backing for Labour: arts, 47%; natural science, 36%; technology, 32%; and medicine, 26% (p. 430). The Halsey-Trow sample was too small to permit specification within disciplines, but our guess that a larger survey would also show British sociologists to be to the left of their colleagues is given weight by the findings of a survey of students at the LSE in 1967, during a student boycott and sit-in. This study found much higher percentages of sociology students, both undergraduate and graduate, giving "support" to and participating in the sit-in. The same pattern occurred with respect to political allegiance; sociologists were more preponderantly socialist, with higher percentages backing groups to the left of the Labour party than those in other social sciences (Blackstone et al. 1970, pp. 212-15, 277). Though American sociologists are involved in "liberal" politics and the British, like the Japanese, back "socialist" or "left" causes, English-speaking scholars on both sides of the Atlantic agree in citing Parsons, Merton, and Durkheim, major theorists of functionalism, most frequently in their literature (Oromaner 1970, p. 329). Clearly, the Japanese and British data indicate that there is no incompatibility between adherence to radical and socialist politics and positive attitudes toward American functionalism.

There are, of course, sharp differences in social and political outlooks among academics, but we suggest that they result, in part, from different levels of commitment to intellectual and hence critical functions. Most faculty are, in fact, primarily teachers, dedicated to the passing on of existing traditions, not to the enlargement or critical rejection of it. And, of course, many faculty, even those who are doing research, are not concerned with "basic" work, with the core of ideas centered in the so-called liberal arts faculties.

WHY SOCIOLOGY?

It is obviously necessary at last to ask why sociology has been the most liberal-left field in academe. What makes those who practice it, particularly the more successful and scholarly among them, more favorable to basic social change than those in other fields, even in the other social sciences? What factors produce the Gouldner-Sprehe finding that 77% of the sociologists in 1964 affirmed the need for "basic change in structure and values" to accomplish necessary social reforms?

First we should note that neither the greater liberalism of sociologists nor the distribution of political orientations among the various disciplines

The Politics of American Sociologists

TABLE 7

SOCIAL BACKGROUND OF FACULTY, BY FIELD

| | Father's Education (Percentage Having Attended College) | Father's Occupation (Percentage Manual) | Father's Occupation (Percentage High Status)* | Religious Background (Percentage Jewish) |
|---------------------------|--|--|--|---|
| Sociology | 34 | 25 | 18 | 13 |
| Social work | 34 | 26 | 23 | 16 |
| Political science | 48 | 22 | 26 | 13 |
| Psychology | 41 | 22 | 20 | 17 |
| All social sciences | 42 | 22 | 21 | 15 |
| Anthropology | 53 | 16 | 30 | 12 |
| Economics | 43 | 18 | 21 | 15 |
| Humanities | 43 | 21 | 24 | 8 |
| Law | 50 | 14 | 32 | 25 |
| Education | 30 | 32 | 14 | 6 |
| All fields | 40 | 23 | 22 | 9 |
| Medicine | 57 | 10 | 39 | 22 |
| Physical sciences | 41 | 25 | 21 | 8 |
| Biological sciences | 42 | 23 | 22 | 10 |
| Business | 32 | 27 | 13 | 8 |
| Engineering | 39 | 26 | 19 | 9 |
| Agriculture | 25 | 21 | 9 | 1 |

* Working with the Duncan occupational prestige scale, occupations were classified as high status, middle status, and low status.

can be attributed to differences in the social origins of their members. The academic fields do contain different mixes of social backgrounds, and at the extremes these are quite substantial. The percentage of those of Jewish parentage (table 7) ranges from 25 in law and 22 in medicine to 15 in the social sciences—the most Jewish of the liberal arts and sciences groups—down to less than 1% among the faculties of agriculture. Professors of law and medicine also come, on the whole, from families of much higher socioeconomic status than the faculty as a whole: fathers of nearly 60% of the medical school faculty, for example, attended college, and only 10% were blue-collar workers—compared with 23% in the whole professoriate and 60% in the country's male labor force in 1950. Sociology, contrary to some speculation, has a slightly smaller proportion of Jewish faculty members than the social sciences collectively, and indeed it is not much more Jewish than the faculty at large. Sociologists and their colleagues in social work come from families of lower socioeconomic status than other social scientists; the contrast with political science and anthropology is quite striking.

Interesting as these data on social origins are, they do not account for differences in political orientations. Whether a faculty member was brought up in a working-class family with parents who only attended grade school

or was the child of a college-trained professional shows little effect on his present political opinions. Class position of parents is not closely correlated with any of the scales or with any political-opinion variable included in the Carnegie questionnaire. This holds for all fields.

Religious background is another matter. Faculty members of Protestant and Catholic parentage do not, as groups, differ much in their politics, but Jewish faculty members are much more liberal-left (Lipset and Ladd 1971*b*). The liberalism of social sciences, however, is not a function of Jews "bringing up the average." Jews in the social sciences are very liberal, but so are the others. Indeed, the more liberal the field, the smaller the differences between Jews and non-Jews. In such conservative disciplines as business or engineering, professors of Jewish backgrounds are much more liberal than their non-Jewish colleagues: in engineering, for example, 54% are very liberal or liberal on the liberalism-conservatism scale, compared with just 20% of the faculty of Protestant and Catholic parentage; in business, 51% of the Jews but only 17% of the Gentiles are in the two most liberal quintiles. Those factors which have operated to make American Jews disproportionately liberal-left operate as well among Jewish academics, and, in conservative fields, Jews are thereby sharply distinguished. But in the liberal fields, Protestant and Catholic faculty are—contrasted with their religious peers in the general public—distinctly liberal, and the Jew-non-Jew differences are not large. Besides this, sociology is less Jewish than any of the social sciences except anthropology.

To understand the political commitments of sociologists, we should begin with the fact that academe as a profession has recruited heavily through the years from the more left-inclined segment of undergraduates (Rosenberg 1957; Davis 1965). For example, a study of the Berkeley undergraduates in 1959–61 found that those who "realistically considered" becoming a college professor were more likely to have a left political self-identification. A large majority of those describing themselves as "socialists" (62%) considered becoming professors, followed by liberal Democrats (34%), liberal Republicans (20%), conservative Democrats (14%), and conservative Republicans (15%). "Other findings which [independently] support this conclusion are that those who give liberal responses to questions concerning the Bill of Rights, labor unions, and minority groups are more likely than illiberal responders to have considered college teaching" (Currie et al. 1968, p. 541). Studies of student images of various occupations give professors "a high score on radicalism" and on "power in public affairs" (Beardslee and O'Dowd 1962; Knapp 1962).

If academe attracts more left-disposed students, then social science should have the most appeal to those who would combine an academic career with a concern for social problems. As Alain Touraine (1971) put it, "It is normal that those who have chosen to study society should be

most aware of social problems" (p. 312). The 1969 Carnegie surveys of undergraduate and graduate students found that a higher proportion in the social sciences were oriented to social reform.

And if social science intrinsically appeals to the more politically oriented and reform-minded among students, it may be anticipated that sociology should be even more attractive to students with left predispositions than the other social sciences, concerned as it is with topics which remain a focus for discontent—race, stratification, urbanism, power, crime, delinquency, etc. It differs from the two directly policy-relevant social sciences, political science and economics, in having less focus on government as a source of social change. Charles Page (1959) has explicitly suggested that the view of sociology as "an ameliorative enterprise . . . fairly widespread in academic faculties and among college students, draws many of the latter to classes in sociology" (p. 586).

The Carnegie faculty questionnaire permits a limited test of these assumptions since it asked about the politics of the respondents while they were seniors in college. The results (table 8) seem to generally confirm

TABLE 8
PERCEPTION OF FACULTY MEMBERS OF THEIR POLITICS
AS COLLEGE SENIORS, BY CURRENT DISCIPLINE

| Field | Left | Liberal | Middle of the Road | Moderately Conservative | Very Conservative |
|---------------------------|------|---------|--------------------|-------------------------|-------------------|
| Social work | 15 | 51 | 20 | 12 | 2 |
| Anthropology | 15 | 42 | 26 | 16 | 1 |
| Sociology | 12 | 48 | 21 | 16 | 3 |
| Political science | 10 | 50 | 22 | 16 | 3 |
| All social sciences | 10 | 46 | 23 | 18 | 3 |
| Economics | 9 | 44 | 24 | 19 | 4 |
| Psychology | 7 | 46 | 26 | 18 | 3 |
| Humanities | 6 | 41 | 25 | 22 | 5 |
| Law | 6 | 41 | 26 | 20 | 7 |
| Medicine | 5 | 35 | 26 | 28 | 6 |
| All fields | 5 | 34 | 29 | 27 | 6 |
| Biological sciences | 4 | 32 | 30 | 29 | 6 |
| Physical sciences | 4 | 31 | 30 | 30 | 6 |
| Education | 3 | 32 | 31 | 28 | 6 |
| Business | 2 | 26 | 30 | 34 | 9 |
| Engineering | 2 | 24 | 33 | 33 | 8 |
| Agriculture | 1 | 16 | 33 | 41 | 9 |

the "selective ideological recruitment" thesis. Fifty-six percent in the liberal social sciences remember their undergraduate politics as "left" or "liberal," compared with just 28% of the faculty in business, 26% in engineering, and 17% in agriculture. The differences, however, among the various social sciences are much too small to validate the hypothesis that

sociology has been more attractive as a career to reform or left-minded students than political science, for example. Clearly, sociologists as faculty members are further to the left of those in the other social sciences than they were as students. These impressions are reinforced by the results of the Carnegie surveys of undergraduates and graduate students, which show smaller differences between sociologists and other social scientists on the student than on the faculty level. If we compare the retrospective findings of table 8 with the current views of scholars in different disciplines (table 2), it becomes evident that postgraduate activities of sociologists have had a more radicalizing or less conservatizing effect on their political views than have the experiences of those in the other social sciences.

Much of the grand (and petty) tradition in sociology has fostered the "distrust of reason," through the effort to explain opinion and behavior as motivated by hidden private drives, by concealed self-interest and by the system needs of societies. Methodologically, the sociologist is cautioned against accepting rational manifest explanations for human activity (Bendix 1951, 1970). Robert Merton (1968) has effectively pointed out that functionalism does not differ from Marxism in this respect. As an example, he outlined the similarities between the functionalist and Marxist analyses of religion, that both see it "as a *social mechanism* for 'reinforcing the sentiments most essential to the institutional integration of the society'" (p. 98). Elsewhere, one of us (Lipset 1970) has analyzed the fact that the three major approaches to social stratification—those of Marx, Weber, and functionalism (Durkheim)—each assumed a form of alienation, of self-estrangement, as a consequence of inequality. Thus no "school" of sociology believes that social hierarchy can constitute a stable system accepted fully by the lowly. As Lipset noted, "Functionalist sociology . . . like the Marxist and Weberian forms of analysis . . . points to ways in which the demands of a stratification system press men to act against their own interest, and alienate them from autonomous choice. However, the focus in functionalism on means-ends relationships reveals the conflict-generating potential of stratification systems, in which goals are inherently scarce resources. Hence, functional analysis, like the other two, locates sources of consensus and cleavage in the hierarchical structures of society" (p. 184).

Sociology also has a "debunking" effect on belief in basic assumptions through its production of empirical data which invariably "disprove" the validity of collective self-images. Research which is relevant to social stratification conclusively indicates the existence of sharp inequities with respect not only to income, status, and power, but also to education, health, housing, treatment before the law, and many other values. Whether it is sociological research on social mobility and education in the Soviet Union

and Poland, on the skin color of people in differentially rewarded positions in Yugoslavia, on infant mortality or job possibilities in the United States, on the factors related to job satisfaction in many countries, the evidence all points to punitive character of social systems on the personalities and life chances of those segments of the population who are the offspring of the lowly valued. And given the legitimation of authority in most "advanced" societies, whether communist or capitalist, on the basis of populist and egalitarian values, the findings of sociology reinforce the position of left antiestablishment critics. Whether these results affect the views of many outside the discipline is debatable, but that they are known to most sociologists can hardly be doubted.

What the relevant factors are that press sociology to greater support for leftist views and social activism may not be decided here. It is clear, however, that sociology must be rated as most socially critical, or at the very least, "less conservative" in its dominant ideological orientations or "domain assumptions" in university life. This would seem to support Merton's (1968) thesis that "the fact that functional analysis can be seen by some as inherently conservative and by others as inherently radical suggests that it may be *inherently* neither one or the other" (p. 93).

Moreover, this conclusion throws considerable doubt on the effort to create a politically linked dichotomy between functional analysis and supposed radical sociology—in which the latter is differentiated by its prophetic image of potentialities of society, compared with the preoccupation with things as they are in functional systems analysis. Though Gouldner seeks to present Parsonian sociology as a conservative ahistorical approach to social analysis, a decade ago he saw it in terms which are not antagonistic to an emphasis on the concrete historical sources of maintenance and change: "Both Merton and Parsons agree that in any accounting for any social or cultural pattern an effort must be made to relate this to the context in which it occurs, so that it may not be understood in isolation but must be analyzed in relation to other patterns" (Gouldner 1959). He concluded his earlier comparison of functionalist and Marxist theory by insisting that the real distinction among sociological theories was not, as some critics of functionalism then argued, between system and factor theories (those which emphasize the primacy of certain factors such as the economic) but rather "between implicit [Marxist] and explicit [functionalist] system theories" (1959, p. 211). And he suggested that functionalists, unable to specify the causal weight of particular factors possibly "because they then lacked the mathematical tools for a rigorous resolution of the problem," might soon be able to deal with it given "mathematical and statistical developments."

Two East European Marxists, Helmut Steiner of the Academy of Sciences of the German Democratic Republic and Owsej I. Schkaratan of the

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Academy of Sciences of the USSR, presented a paper at the 7th World Congress of Sociology in Varna (1970) in which they also argued that with all its major deficiencies from their point of view, Parsons's "functional system theory is preferable to most of other mentioned [western sociological] concepts because it tries to understand the social structure as a social organism. That makes it possible to conceive the social structure not only as a complex of statistical variables but as a system of social interrelations. . . . The Marxist understanding of society as a social system is based on the idea that the most important social processes are determined by socio-economical factors. It must be emphasized that in most cases the forms of the socio-economic determinations are extremely interrelated—the more so as a great number of misunderstandings of the Marxist concept derive from neglecting this fact" (p. 3).

Gouldner's earlier position has recently been reiterated by the Marxist sociologist, Pradeep Bandyopadhyay (1971), who observes that "Marxian sociology is often just as concerned [as functionalism] with the analysis of system, structure and equilibrium" (p. 19). And though he has sharp disagreements with the Parsonian theory, Bandyopadhyay points out that the issue between the two approaches is not, as some radical sociologists argue, one of alternative paradigms of analysis. He criticizes many of the contemporary radicals for attacking those objectives which Marxism shares with functional analysis when they seek to show that since all thought has some ideological referent, scientific analysis is, in effect, impossible. Bandyopadhyay notes that this position is not a radical one, but one which has been argued against sociology by conservatives in order "to deprive radicals of their justification for social change" (p. 21).

Given the predisposition to the left of most sociologists, including inevitably most functionalists, much that has been written by functionalist sociologists supports a left ideological position. However one interprets the relevance of the empirical findings to the ideological concomitants of given theoretical approaches, the fact remains that data collected by scholars of different political persuasions over six decades indicate the following: (1) Those involved in intellectual pursuits, including academics, have been farther to the left than any other occupational group on religious and political issues. (2) Within academe, those who possess dominant characteristics (particularly when age is held constant), who have done more as scholars and have been more rewarded, are to the left of the nondominants; and those who emphasize research are to the left of those who focus on teaching. (3) Sociology has been the most critical and change oriented of all academic disciplines. And since the dominants within sociology are more critical of the status quo than others in the field, the leading scholars in sociology are as a group the most antiestablishment. Even the anarchist leader of the French student revolt of May 1968, Daniel Cohn-Bendit

(1968), though convinced that universities inherently serve the needs of capitalism, notes that "sociology professors like to pose as Leftists, in contrast to the heads of other departments who apparently still hanker after the good old times" (p. 39).

Perhaps the reason that there is so much literature in sociology attacking other sociologists for their alleged "conservatism" is that the left is more heavily represented in this field than in any other, and that within a "left discipline" (as within a radical political party) the question of who is the "most revolutionary" becomes salient. The extremity of such criticism may be seen in the manifesto put out by the sociology students of Nanterre, "Tuer les sociologues," in which they called for boycotts of "reactionary" sociology courses. They bitterly attacked American sociology for, among other things, reacting to riots in the ghetto by taking government funds to "study the movements of mobs and furnish recipes for repression." In their judgment, sociology in France and at Nanterre is equally bad since "all current sociology in France is imported from the US, with a few years' delay." The task is "to unmask the false arguments, throw light on the generally repressive meaning of a career in sociology, and to dispel illusions on this subject" (Cohn-Bendit et al. 1969, pp. 374-75, 378).

To appreciate fully the politics of this manifesto by sociology students who were among the principal leaders of student activism in Nanterre, it is necessary to recognize that, in general, there has been considerable sympathy for Marxist and dialectic approaches in French sociology.⁸ The major figure at the Sorbonne, and in French sociology until his death in 1965, was Georges Gurvitch who "was able to effect in France his own highly original synthesis of Marxism, phenomenology and empirical sociology" (Birnbaum 1971, p. 16). As René Lourau (1970) notes: "Among the principal representatives of French sociology are professors and research workers who have been members of the Communist party for periods of a few months to several years" (p. 228). The most important of these former Communists, Henri Lefebvre, was the first sociologist appointed at Nanterre. He was followed by Alain Touraine, also a radical sociologist. Though hostile to each other, both men have been strong critics of American empirical and functional sociology. They were instrumental

⁸ A national survey of French university students in the faculties of letters and the human sciences (May-June 1965), three years before the "events" of 1968 and in a period of relative quiescence in university politics, found that the sociology students were more active in syndicalist or political groupings and much more supportive of Marxism than were those in any other field. When asked to which school of thought they adhered, 35% of the sociology students replied, "Marxism," more than mentioned any other approach (Delsaut 1970, p. 53). Student views, of course, need have no relationship to the opinions of the faculty, but there is some indication that they did in the French case.

in appointing almost all the junior faculty ("assistants") still at Nanterre in 1968. Radical sociology dominated Nanterre even though the next two professors appointed there, François Bourricaud and Michel Crozier, could not be described in these political terms (Crouzet 1969). These men, however, were much less influential, since the assistants (lecturers) who dominated communications with the students were largely unsympathetic to them.

In Germany, where sociology students and assistants have also played a major role in university-based radical protest, the leading leftist sociologist of the postwar era, Jürgen Habermas—as a student and later a member of the Frankfurter school of Marxist sociology made famous by men like Horkheimer, Adorno, and Marcuse—has also been denounced as a conservative by younger sociologists. Adorno, the most creative of the senior members of the Frankfurt Institute, died in 1969 soon after his class was disrupted by students. His long-time friend, Carla Henius, has recently written that these events had so "hurt him and broken him that they probably caused his death." Habermas, admired by many American "critical sociologists," has given up teaching at Frankfurt for a full-time research appointment at the Max-Planck Institute near Munich (Lasky 1971, p. 64). Norman Birnbaum (1971), a self-described "revisionist Marxist" and disciple of C. Wright Mills, could not restrain his astonishment at such charges by German and French student radicals: "These were students taught *inter alla* by Alain Touraine and Henri Lefebvre: depicting them as agents of the oppression is as grotesque as the curious belief manifested by some German students that Jürgen Habermas is reactionary. . . . [This] suggests the discipline has been unable to assimilate the self-critique administered by radical professors like Habermas, Lefebvre, and Touraine" (p. 230).

The division within the Left which plagues sociology has, on the one hand, those protagonists who are primarily concerned with social action and who want the discipline to be its handmaiden, and on the other, those oriented toward traditional forms of scholarship. The latter include many who consider themselves as radicals. In an effort to defend himself from the radical attacks on *The Coming Crisis of Western Sociology* Gouldner (1971) has made it clear that the issue is largely one of a commitment to basic scholarship versus activism, rather than radicalism or other forms of politics. He states explicitly: "Sociology today does not need a Karl Marx or an Isaac Newton; it needs a V. I. Lenin," that is, a theorist concerned with political action (p. 96).

The activist, whether student or young faculty member—and our data indicate that age is by far the most important correlate of activist orientations within the American professoriate generally and sociology in particular—is inherently more of an advocate than a scholar. An activist

must seek to simplify problems if he is to help the movement. Scholarship, on the other hand, and functionalist theory in particular, seems to emphasize an opposite style. Stressing interrelationships and the fact that, in the absence of a key factor theory of change, harmful "unanticipated consequences" may result from "purposive social action," functional analysis implicitly argues for some caution in radical social change. The functionalist, in effect, tells the young (or old) activist that he should move—carefully. And his course of "action" is usually a request for more research, more consideration of the interrelationship of assorted factors.

The concentration on functional interrelationships and the consequent concern for "unanticipated consequences" does have political or ideological implications, then, to the extent that it stresses the complexities involved in social change. Still, as Irving Horowitz (1968) argues in a sophisticated analysis of functionalist ideology, men committed to sharply different political values may employ functional analysis in their scholarship. Thus, Tawney was able to conclude that "profits extracted from industry which serve the cause of conspicuous consumption; and in general, ownership divorced from production," were dysfunctional from a larger system point of view. Functional analysis supported his socialist beliefs (Horowitz 1968, p. 240). Or if we consider the two classic theorists of functionalism, Horowitz continues, "there is no more a 'functionalist imperative' for Malinowski's individualism than there is for Durkheim's socialism" (p. 243). The same can be said about the more recent controversy concerning the functions of systems of stratification between Kingsley Davis and Melvin Tumin. Davis sought to demonstrate that unequal rewards are necessary to motivate people to take on various "responsible" positions and to associate jobs with talent. Tumin countered with evidence that many highly rewarded positions do not in fact require scarce talent, prolonged training, or tension-breeding tasks, and that alternative systems of motivation can be envisaged. Yet both men saw themselves engaging in functional analysis. A recent methodological critique of functionalism by a Polish scholar, Piotr Sztompka (1971), lists the following scholars as functionalists: "R. K. Merton and T. Parsons, as well as some other writers like K. Davis, W. E. Moore, A. Gouldner, G. Sjöberg, M. Tumin, M. Levy" (p. 369).

To assert, as we do, that academic social science generally has an inherent built-in gradualist bias because of the scholarly dictum to consider all relevant hypotheses, factors, and possible evidence before coming to definitive conclusions is not to argue that political activists must draw policy conclusions from these inherent methodological considerations. It is simply to say again that scholarship and politics are different areas of human activity, even though some individuals are involved in both. A scholar is duty bound to report all evidence which challenges his basic assumptions and to stress the limitations of his results, their tentative and

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uncertain character. A politician, on the other hand, is an advocate and an organizer. He is expected to make the best possible case for his point of view, to ignore contradictory materials, to make up his mind on the basis of the limited information he can secure before the deadlines imposed on him by his role, and then to act in a self-assured fashion. The worst thing that can be said about a politician is that he is as indecisive as an intellectual, an image held of Adlai Stevenson, to his sorrow. Conversely, an academic will be subjected to criticism for publishing before all the evidence is in, or for oversimplifying what is inherently a complicated phenomenon (Lipset 1972*b*).

Once these distinctions are made, it should be clear that the two roles, scholarly analyst and political actor, must be separated. The scholar who seeks to serve directly political ends does both scholarship and politics a disservice, unless he keeps the two roles as distinct as possible. One reason that Max Weber was so insistent on the need to keep politics out of the classroom and research was that he was an active politician. Those dedicated to politics, particularly to reform or revolution, should, of course, seek relevant information or scholarly knowledge, but only as advice relevant to the attainment of precise ends. There are times when revolutions are necessary; any scholarly assessment of the probabilities that they will attain the goals they seek can only serve to undermine the commitment of the rebels to risk all. Our conclusion that social science is inherently gradualist is based on this logic: the implications of an analysis that can only claim to explain part of the variance, that admits any conclusion may be very wrong in a specific case, can only be to move slowly and carefully.

The case may be made that social scientists are more likely to contribute to the "solution" of many social problems if they separate themselves from policy-relevant matters to be free to look for more abstract levels of generalization. This point has been urged effectively by radical historian Christopher Lasch (1969), who finds the work of Erving Goffman on stigma and "spoiled identity" particularly useful in analyzing leadership behavior among blacks and other socially oppressed groups: "Goffman deliberately excludes the race problem from his analysis of 'spoiled identity,' on the grounds that established minorities do not provide the best objects for an analysis of the delicate mechanisms surrounding the management of stigma. . . . At the same time an understanding of face-to-face relationships drawn from quite a different perspective throws unexpected light on certain aspects of race relations—notably on the role of 'professionals' " (p. 21).

These sources of difference between the orientation of the committed scholar and those primarily concerned with political reform or revolution are real and should be regarded in a noninvidious fashion. As noted earlier, many students and young faculty enter sociology because they seek

ways of enhancing their political objectives. Hence, the interest of many of them is not that of the scholar but of the activist or politician. Such an interest is both valid and necessary. Insofar as it is also academic, it resembles that of the engineer or physician more than of the physicist or biochemist. Some individuals in science and social science successfully combine both the activist and the scholarly roles. A difficulty arises, however, when the activist in social science sees a concern with scholarship alone as reactionary, as necessarily serving the interests of the status quo.

Similar controversies, of course, have appeared in many other disciplines with comparable arguments, although the "activist" faction tends to be weaker. As in sociology, the debate is often conducted within the left between those who emphasize the obligation of the politically concerned academic to the canons of scholarship and those who would place activism first. In history, for example, the three major leaders of the "scholarly" faction which has been dubbed "rightist" by its opponents have been H. Stuart Hughes, who has a record of third-party involvement dating back to the Henry Wallace Progressive party, who ran as an "Independent" peace candidate for senator in Massachusetts in 1962, and who has been the head of the antiwar group SANE; C. Vann Woodward, a participant in the Socialist Scholars Conference; and Eugene Genovese, a self-described Marxist historian who was pressured to resign from Rutgers University in 1965 for his public advocacy of a victory for the NLF in Vietnam and his praise for Mao and Communist China. Genovese (1971) has written eloquently concerning the tensions faced by the leftist academic. He concludes, however, that he must concentrate on his academic work, that it cannot be done well by anyone who considers it a "substitute for the more exciting vocation of street fighting or organizing" (pp. 7-8).

To emphasize the validity and importance of the scholarly undertaking is not to suggest, of course, that value-free scholarship, in any absolute sense, is possible. Personal values, variations in life experiences, differences in education and theoretical orientation, strongly affect the kind of work men do and their results. Max Weber, long ago, pointed out that the concept of ethical neutrality was spurious, that those who adhered to it were precisely the ones who manifested "obstinate and deliberate partisanship." He stated unequivocally that all "knowledge of cultural reality, as may be seen, is always knowledge from particular points of view" (1949, p. 81).

Weber wrote both as a scholar and a political activist. As scholar, he also argued that verifiable knowledge was possible given the communism of science, the exposure of findings to the community at large. Any given scholar may come up with erroneous results stemming, in part, from the way in which his values have affected his work. But the commitment of scientists to objective methods of inquiry, the competition of ideas and

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concepts, will increase the possibility of finding analytic laws which hold up regardless of who does the investigation. "For scientific truth is precisely what is valid for all who seek the truth" (1949, p. 84).

A similar argument has recently been made by the Marxist sociologist, Pradeep Bandyopadhyay (1971), writing in the oldest continuous journal of Marxist scholarship in English, *Science and Society*. He strongly challenges the contention of contemporary university activists that a Marxist sociology of knowledge would deny the possibility of objective knowledge in the social sciences and cites various comments by Marx himself which are directly relevant (pp. 17, 18, 22).

The increase in efforts to inject into sociology tests of ideological purity has made the discipline perhaps the only one in which "professional" reviews of books can take the form not of evaluating the evidence for the validity of the hypotheses enunciated but solely of seeking to demonstrate that the author reflects a "conservative" bias; and in which a scholar like Talcott Parsons, who has supported a great variety of programs designed to foster social welfare policies, racial equality, equal opportunity, and peaceful and friendly relations with the Communist world, becomes a symbol of conservatism.

The effort to denigrate intellectual work by labeling it in terms which are deemed opprobrious by the audience to which they are addressed can only serve to prevent intellectual dialogue. A recent review of Gouldner's work states the problem vividly. "A central idea . . . is that when we pit an ideological tag on a theory by calling it repressive, prophetic, or what-not, we say something about the validity of the theory. This notion is alarming, for it would turn sociology into substandard moral philosophy with the resonating of sentiments replacing reason and observation as the basis for constructing and judging theories. Thus . . . Gouldner . . . [has] attacked more than one brand of sociological theory . . . [he has] attacked the rational underpinnings of the entire discipline, without which it cannot and should not be taken seriously as an intellectual enterprise" (Simpson 1971, p. 664). How are we to evaluate Gouldner's earlier work if we recognize, as Jackson Toby (1972) has pointed out, that in his textbook (Gouldner and Gouldner 1963), written presumably to introduce students to the best thought in sociology, Gouldner has "only respectful references to Parsons? Indeed, the 15 references to Parsons in his textbook dwarfed the three to Karl Marx and indicated intellectual debts on a variety of subjects in a straightforward fashion."

This point of view is not limited to defenders of the academic profession. A Marxist also finds that such efforts undermine the radical's effort to gain acceptance for his theory. "To judge theories in terms of the values they promote is to mistake good intentions for knowledge. . . . By using our values for the acceptable or rejection of theories, we do no more than

provide crutches for ourselves. . . . To criticize a sociologist for the values he holds when unable objectively to demonstrate the error of holding those values is to allow him to get off lightly" (Bandyopadhyay 1971, pp. 22–23, 26).

Efforts to judge scholars and theories by the presumed political consequences of their work produces curious amalgams. Thus, for some years the radical sociologists adopted Pitirim Sorokin, White Russian emigré bitterly denounced by Lenin and militant anti-Communist, as a hero, presumably because he was a severe critic of the sociological "establishment," and in his older years a strong peace advocate. Similarly, Alvin Gouldner sees conservative implications in the work of Parsons, but has much more praise for that of his colleague, George Homans, a self-proclaimed conservative who (as Gouldner acknowledges) has taken a variety of conservative positions on domestic, international, and university issues. Gouldner also prefers the sociology of Erving Goffman, as comprehensive an advocate of an apolitical, nonproblem, "pure sociology" as exists in the field today. We suspect that the unifying theme which makes sense of these positions is that Parsons, together with others identified with him—Merton, Lazarsfeld, and the late Samuel Stouffer—became identified as the "Sociology Establishment" from the late 1940s on, an establishment defined by scholarly achievement and influence.

Applying the class-interest theory of politics to academe, as reflected in the hypotheses of the Gouldner-Sprehe study, those who see themselves outside the "Establishment" attribute an inherent social and academic conservatism to those they identify as within it, and a more left-oriented posture to those outside it. But, in fact, whatever the politics of a given individual, statistically speaking the "dominants" within sociology have been and remain considerably to the left of the "nondominants," a finding that should be analyzed by the radical critics of the university.

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Professionalization of Sociology

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In the years 1965–70, there was a marked increase in writings on the “sociology of sociology” or, more particularly, on the “institutionalization” of sociologists. However, ever since sociology emerged as an academic discipline in the United States, issues of the proper organization and role of sociologists have been central themes for debate. As early as 1894, Ira W. Howerth wrote about the “Present Condition of Sociology in the United States.” Edward A. Tiryakian, in *The Phenomenon of Sociology* (1971), offers an extensive bibliography on these topics which indicates that already in the 1920s and 1930s there was an active sociology of knowledge approach to sociology.

These writings reflect the obvious fact that most sociologists in the United States (and throughout most of the world where academic and research freedom exists) pursue their vocation in a university setting. Research into sociology as an institution has been pursued by means of the standard categories of organizational analysis; for example, institutionalization focuses on division of labor, organizational control, internal communication, authority and power, interorganizational relations, and the impact of societal values and norms. Moreover, although the forms and mechanisms of professionalization are relatively undeveloped, sociologists are members of a profession. To speak of sociology as a profession is to focus on a relatively neglected aspect of the organization of the discipline, namely, its clients and the dilemmas of client relations (Hughes 1958).

As an academic profession, students are, of course, the immediate clients of sociologists. It is a professed goal that the academic sociologist will accept the responsibility to teach, although the audience for teaching may be variously defined to include other professional groups or extra-mural assemblies. Ideally, teaching is thought of not only as a professional responsibility per se but as an essential activity for improving one's research.

While the academic sociologist seeks to combine teaching with research, identifying the clients for university research is more difficult. The researcher relates, with his procedures and responsibilities, to his colleagues who help define the standards of performance rather than to clients. As researcher, the sociologist is concerned with the complex issues concerning the treatment, dignity, and privacy of the subjects of his investigations. But strictly speaking, the clients of the sociologist, as researcher, are relatively ambiguous. If one thinks of donors of financial support for research as a kind of client, for instance, the sociologist in a university setting has the

minimum responsibility to that client to publish his research findings. However, his primary professional purpose in publishing is to serve other sociologists and all others who are potentially interested in his findings. The ability of the sociologist as researcher to find other clients in various types of nonuniversities settings such as independent research bureaus, voluntary associations, governmental agencies, or business enterprises has been a central theme of analysis and debate (Evan 1962).

Since sociology began as an academic discipline, there has been a continuous effort to create practice roles of sociologists, often called "applied sociology." The objective has been to locate sociologists outside of the university to serve a variety of specific clients in the larger society: in consultation, social planning, the collection of data, and various forms of administration. In fact, sociologists have not created an extensive component that renders specific services to clients—either individual or organizations—in the same sense that medicine, law, or psychology have. Nor have they produced a compelling logic for their practice or an effective system of professional or social control of their activities.

The purpose of this paper is to sketch out the "natural history" of the efforts to create an applied sociology and to try to account for the relative lack of success despite extensive energies invested in efforts to create and develop specific clients for sociology. The underlying assumption is that the theoretical and empirical content of sociology as an intellectual discipline, more than the organization of sociologists, accounts for its inability to develop a practice specialization. As such, sociology needs to be thought of as a staff-type profession based on the fusion of research and teaching roles most effectively institutionalized in a university structure. The teacher-researcher format, rather than that of engineer or clinical psychologist, is the basis for analyzing the performance and social responsibility of sociologists.

In fact, the concentration of sociologists engaged in sociological efforts outside the university is smaller than that of either psychologists or economists. The absence of an effective "applied" element in sociology is not the result of indifference on the part of the profession; the search for an "applied" sociology has had a long tradition. One of the central elements in the natural history of sociology as a profession is the tradition of a liberal and "left of center" personal ideology among sociologists. This personal ideology, more than any other social characteristic, helps account for the persistent effort of many sociologists to "apply" their knowledge directly rather than to be content with the role of teacher-researcher.

The analysis does not discount the impact of sociological thinking on contemporary society and decision making. This impact results, for better or for worse, from its contribution to the general education of generation

after generation of undergraduates. Some sociologists would attribute an even greater impact from those books and monographs which have been widely read by informed publics.

The teacher-researcher format has been and will continue to apply to new institutional settings; for instance, sociologists have been appointed to the faculties of medicine, law, and education (Young 1955). Another important professional development for sociologists has been their participation in public commissions of inquiry on crucial and pressing social issues. But, as discussed later, these commissions function most effectively when the sociologists conform to the teacher-researcher format. In addition, even in the absence of a stable and professionalized cadre of applied sociologists, sociological researchers and social statisticians supply the indispensable descriptive social intelligence and data without which decision making in contemporary society could not function (Duncan 1969).

PROFESSIONAL PERSPECTIVES OF SOCIOLOGISTS

That the theoretical and substantive content of sociology inherently limits the development of an applied or practice specialization in sociology is a view not shared by wide segments of the sociological community. Indeed, most sociologists hope for the day when the discipline will develop its theories and research measures to permit the emergence of such a specialization. I have explored this issue elsewhere in terms of the distinction between an "enlightenment" conception of sociological knowledge and an "engineering" conception. The enlightenment conception accepts the above expectation while the engineering conception is oriented toward the ultimately developed nonacademic practice profession in the conventional sense of the term (Janowitz 1970).

The engineering model makes a strong distinction between basic and applied research. The sociological enterprise is defined as a group of highly trained, theoretically oriented specialists supported by methodologists and, as required, field workers (McRae 1970). The actual interaction with clients is not done by theoretical and analytically oriented basic researchers but by the more numerous applied sociologists who are trained in their particular tasks of collating research and in dealing with clients and practitioners.

The enlightenment model sees no strong distinction between basic and applied sociology (Shils 1949). Hauser's formulation is worthy of scrutiny because of the manner in which he reduces the distinction (1949): "In this sense, both pure and applied research are scientific endeavors. The essential difference between pure and applied research is to be found not in the point of view or methods of the investigator, not in the nature of the

phenomena under investigation, but rather in the manner in which the problem is selected, in the auspices of the research and in the immediate, as distinguished from the long run objectives of the research."

The sociologist concerned with testing hypotheses cannot unduly separate any phase of the collection and assessment of data. He must recognize that he is part of the social process and not outside of it. He strives for a high degree of generality but must recognize that sociological knowledge is not to be found in formal propositions alone but in concrete facts and empirical data which give meaning to the search for generalization. Of course, some sociologists are more skilled in logical and formal analysis, while others have stronger skills in quantitative or field work. But an excessive differentiation can only serve to thwart the growth of sociological knowledge.

Sociologists must always be alert to the limitations of their findings and the possibilities of alternative explanations. In fact, many reject the view that sociological knowledge can produce definitive answers on which professional practice can automatically be based. The goal of sociological knowledge is to clarify alternatives and to assist professionals and the public in institution building.

The dissemination and fusion of sociological knowledge cannot simply be devolved to some "secondary" group. Although some division of labor is involved, the researcher himself must be involved in the process. As such, he performs as a teacher in a classroom or seminar, leading the student or the public to understand the processes of the research effort in order to assist them in formulating their own conclusions rather than in the formulation of prescriptions (Znaniecki 1940).

The essential issue is not in the degree of quantification but a much deeper question of the limits and potentials of "scientific" rationality in a democratic society. James Coleman, in the wake of the attention generated by his study on the American school system, articulated the underlying premises of the enlightenment model. "Thus at some point, the researcher must hand over his results to the policy maker and more generally to the public. The more easily interpretable and intellectually digestible he can make them, the better. The task of political digestion itself, however, is not the task of the researcher, but of the whole political process. The research informs that process but it does no more. It cannot, even if its information is perfect, take the place of that process" (Coleman 1970). Students of the history of ideas will note the parallel to the formulations of John Dewey and his pragmatic philosophy, especially in *The Public and Its Problems*. Coleman is indeed bold in formulation since he holds that one criterion in the statistical analysis of his materials is the comprehensibility of the procedures.¹

¹ The notion of "clinical sociology" as offered by Alvin Gouldner in his earlier writing

As of 1970, there exists no adequate statistical information about sociologists' professional commitments to these alternative formulations about the role of sociology in social action. But I venture to estimate that most hold, in one form or another, a hope for a sociology with an engineering component.

SOCIOLOGY AS AN ACADEMIC PROFESSION

By the middle of the 19th century, the intellectual elements of the discipline had been formulated. Only 50 years later—between 1890 and 1900—sociology became an integral part of the academic scene. The first elements were intellectual—the sociological categories for describing society; the constituent components can be traced back to antiquity if one desires, but they were first given a “modern” formulation in the writings of the Scottish Moralists (David Hume, Adam Smith, Adam Ferguson, and Dugald Stewart [Schneider 1967]). In turn, the explicitly “contemporary” formulations can be identified in the writings of August Comte.²

Empirical investigations constituted the second element, which sought to describe the social structure and which took many forms on the continent of Europe, in Great Britain, and in the United States. The heritage of social research, for example, can be thought of as emerging in Great Britain in 1834 with the appearance of the Report of the Commission on the Poor Law and the founding of the Statistical Society of London (Philip Abrams 1968). By 1861 Henry Mayhew's noteworthy studies of London were being published. In the United States, long before the Civil War, empirical social surveys, rich in detail, were numerous. However, before 1890 few of these empirical research studies confronted the issues raised by the “theoretical” sociologists. Moreover, the field work was rarely cumulative.

To speak of the institutionalization of sociology as a scientific discipline meant the (a) mutual interpenetration of sociological concepts with empirical research, and the (b) organization of stable groups for the training of new cadres of sociologists who would be aware of past efforts and who could work on the basis of some degree of collegial control and standards of performance. This did not occur until after 1890, when departments of sociology were organized in the universities in the United States and a journal was founded—two hallmarks of the organization of sociology as an academic discipline.

The idea of a systematic sociology with intellectual theory and research

appears to be compatible with the enlightenment model (1956, 1957). The import of his later writings is difficult to evaluate since they leave unclear the role of sociological knowledge in social action. See especially Richard A. Peterson (1971).

² See R. A. Nisbet, *The Sociological Tradition* (1966), for an analysis of the contributions of Maistre and Bonald to the origins of modern sociology.

came into being at roughly the same time in Europe and the United States. However, it became an academic discipline earlier in the United States—at the University of Chicago, Columbia University, and for a time at Johns Hopkins—than it did in Europe. In his intellectual history of the end of the 19th century, Stuart Hughes (1958) attempts to account for the rise of the sociological enterprise in terms of the societal transformation and the emergence of the notion of individuality. In his view, sociology served as a frame of reference in the intellectual search for a normative order which expresses the idea of individuality and individual conscience. An alternative explanation is to be found in Robert Nisbet, *The Sociological Tradition* (1966), in which he focuses more on the processes of urbanization and industrialization. There are, of course, common elements in these formulations. In addition, Edward Shils's (1970) analysis of the more open structure of the American as compared with European universities helps account for the more rapid institutionalization of sociology in the United States.

The experiences of the Department of Sociology at the University of Chicago, founded in 1892 and by no means atypical, reveal the early institutionalization and professionalization of sociology as an academic discipline. From its origins, the department was intellectually committed to the enlightenment model, although there were recurrent vigorous impulses to develop an engineering component.

Sociology was organized as a research discipline whose findings President William Rainey Harper assumed would have beneficial social consequences. Four professors were authorized, not only to prevent domination by one point of view but in order to assemble the varied skills and interests that were required. The department was oriented toward training new cadres of sociologists, and research was a central aspect of their training. The first sociology journal was founded and department members were active in the affairs of the national association. In short, from its very beginning, sociology had a departmental basis; it was more than the effort of a single professor. Moreover, it could not be thought of as a collection of men who were only part of a learned society. They were academic professionals or, rather, they were striving to be professionals, since professionalization for them meant control over their research and academic freedom.

Indeed, from the very beginning, there was a strong emphasis on teaching. President Harper repeatedly proclaimed that the University of Chicago professors taught because that was the responsibility of a university. Faculty were recruited with the expectation that they would teach, and the norms of the institution meant that the highest prestige was accorded to the men who both engaged in important research and who were effective and stimulating teachers.

This model appealed to the sociologists. Faculty members engaged in

extensive teaching both on and off the campus (even on radio later) and placed great importance on the preparation of treatises for teaching, of which the most outstanding example was *Introduction to the Science of Sociology* (1924 [1969]) by Robert E. Park and Ernest W. Burgess. The work agenda of a department member was strikingly similar to that of the 1970s.

In 1910, field studies and participant observations became an integral part not only of graduate training in sociology but also an element in the undergraduate liberal education offered by the sociological faculty. Students were encouraged to "tell it as it was." Cohort after cohort prepared their autobiographies in order to reveal their cultural background, their aspiration and frustration, and these materials became the raw material of class discussions.³ Training in statistical analysis, as borrowed from economics and psychology, was quickly incorporated.

TRENDS IN ACADEMIC AFFILIATION

Throughout the 75 years of organized departments of sociology, the discipline has remained essentially in its academic base. Until 1920, the graduates with M.A.'s and Ph.D.'s conformed in general to the model of their professors. They aspired to combine a career of teaching and research in a university setting. An important minority went off to church-related colleges and liberal arts schools, but most found employment in the state universities which were developing departments of sociology. Full-time research agencies interested in sociologists were almost nonexistent. However, a minority, mainly with M.A.'s, went into public agencies; some engaged in research, but most did administrative or social work.

Despite repeated exploration of other settings for the employment of sociologists, there has been little change, especially in the period of marked growth since 1945. No evidence indicates that the rapid increase in sociologists has been accompanied by a diminution of academic sociologists. At this point, sociologists are defined as persons who have received training at the graduate level and maintain an active identification with the discipline.

Incomplete data about the employment of sociologists since 1940 underestimate their employment in academic settings. Hollis (1945) reported in 1940 that 79% of Ph.D.'s were employed in educational institutions—nearly all in universities and colleges. Matilda Riley's (1960) comparison of the membership of the American Sociological Association in 1950 and 1959 revealed that the percentage of active members was the same in both

³ Mountains of biographical and documentary materials accumulated by Ernest W. Burgess remain housed in the archives of the University of Chicago Library and serve as valuable sources of the social history of the period.

years, 79%. During this decade, when interest in developing the practice aspects of sociologists was intense, 6% of Ph.D.'s who were members of the ASA shifted from academic to nonacademic positions and 3% or 4% had shifted in the opposite direction.

Based on data collected in 1960, Sibley (1963) found that 78% were in educational settings. Data reported by the National Science Foundation in 1966 reported 75.4% in educational institutions (Hopper 1967) and for 1970, reported an even higher figure of 83%. My evaluation of the sources is that for those who have more than a master's degree (including all but the Ph.D. thesis) more than 90% are employed in education and academic settings. As is well known, most sociologists in nonacademic employment have only a master's degree. For many of them, sociology is only a form of general education, and they lose their sociological affiliation after some years of employment, especially in business, but also in governmental services, although their sociological background may be highly appropriate. However, Sibley did find that over half of the sociologists with a master's degree were employed in academic settings, while only 20% were in health, welfare, religion, or correctional work. Thus, even the M.A. sociologist is oriented toward the academic setting.

Because these surveys of employment are based on membership in the American Sociological Association or the National Science Foundation Register, they underrepresent or even exclude categories of sociologists who are teachers in smaller colleges and who are affiliated with regional association. Other factors explain the fact that the actual concentration in academic settings is higher than that represented in these semi-official surveys.

Many Ph.D.'s in nonacademic posts maintain no effective links with the discipline or its organized associations. While sociology was an important element in their education and career development, such persons cannot be truly designated as practicing sociologists.⁴

Sibley data on Ph.D.'s for the early 1960s presents a useful portrait of the tasks actually performed by sociologists employed outside the university and indicates the small concentration of practice-oriented assignments. Of the 22% employed in nonacademic settings, the largest group (three-fifths) was engaged in full-time research, often in government agencies, while one-quarter were engaged in consulting, planning, or ad-

⁴ It does appear as if the available data were collected so as to emphasize the extent to which sociologists are employed in nonacademic settings. Thus, for example, the National Science Foundation Register listed 49 sociologists as being employed by the armed forces; however, with the exception of a handful of instructors at the service academies and a few research grant administrators, these sociologists were ROTC officers completing their obligated tour of active duty before returning to civilian educational institutions. An important number of sociologists employed in the government are engaged in serving universities.

ministration. Stated in overall terms, of all Ph.D.'s, 13% were engaged in nonuniversity full-time research, while 7% were in other types of practice-oriented assignments. Sibley found the well-known feeling of "marginality" among nonacademic sociologists; in particular, they reported a strong doubt "about doing it again" if they could have a second chance. Moreover, he found little link between training, preparation, and employment in nonacademic settings; personally generated interests and skills predominated.

Sociologists stand, therefore, in marked contrast to the other social sciences (except anthropologists) which have a much higher proportion of their professionals employed outside the university and with higher salaries. For example, the National Science Register for 1968 reported that 88% of all sociology Ph.D.'s were affiliated with educational institutions compared with 76% for economists and 60% for psychologists.

PERSONAL IDEOLOGY AND PROFESSIONALISM

The long-term concentration on academic affiliation among sociologists has been matched by a parallel and persistent pattern of personal ideology which helps explain their interest in an applied sociology. Lipset and Ladd (pp. 67-104 of this volume) show that American sociologists since the 1930s and even before have been overwhelmingly "liberal," that is, "left of center" (the size of the very small group of radicals is difficult to assess). However, one essential notion of sociologists as professionals—either academically based or practice oriented—is that they are able to separate, in a socially responsible fashion, their personal ideology from their professional roles in dealing with their clients, their publics, and their peers. Clearly, this dimension represents the deepest and most profound issue in the application of the concept of professionalization to sociology and especially in the period of university activism since 1965 (Ben-David 1972). Many sociologists, since the initial organization of sociology as a discipline, have had strong personal ideologies which have pressed them to seek to make their knowledge relevant or effective for social change; yet as academics they have had to face or have been attracted to the norms of the teacher-researcher.

As early as 1913, James Leuba investigated the attitudes of a sample of the members of the American Sociological Society and found that the academic as compared with the nonacademic sociologists were more atheistic and had more "liberal" religious beliefs (1921). Moreover, those who were judged by a panel to be more creative were the least religious. In a repeated series of studies, the "liberal"-left of center and radical orientation of sociologists have been documented, although most of these studies made use of mechanistic and undifferentiated categories of political ideology.

Jerome Davis's (1940) survey of the membership of the Eastern Sociological Association in 1939 is particularly noteworthy because he showed that sociologists of this period were extensively active in their civic roles, particularly in race relations. This characterization appears to be more accurate than the claim of Sibley that after 1945 "the involvement of academically based Ph.D.'s in non-academic activities on a part-time or intermittent basis has been growing greatly" (1963, p. 45). The deep involvement in civic and political activities has characterized the biographies of sociologists such as W. I. Thomas, Robert E. Park, and Robert Lynd from the very inception of the field. No doubt there has been a broadening in scope from local and metropolitan arenas to a national perspective.

Thus, it was not a startling revelation that on the issues of the 1960s—the war in Vietnam, race relations, civil liberties—sociologists were strongly on the "left of center" end of the continuum. Perhaps it was striking that the data collected for the Carnegie Commission on Higher Education documented that, as an academic group, sociology professors had the highest concentration of "left of center" attitudes on these related issues. This and other available sources do not make it possible to identify the fraction of sociologists who could accurately be called political radicals in their personal ideology. Of course, sociology graduate students and young faculty members were conspicuous in their campus activities during the period of student unrest in the late 1960s.

One would not expect, in the American setting, that social origins—father's occupational background or social class—would be a powerful variable in accounting for the pattern of social and political orientations of sociologists (Lipset and Ladd, p. 000 of this issue). While the reported data from the large-scale study sponsored by the Carnegie Foundation, in the absence of additional statistical analysis, does not permit precise estimates of the contribution of particular variables, the social background variable cannot be neglected. Although it throws only very partial light on the differences within the social science disciplines, it does help explain differences between the faculty in the social sciences and in professional schools or the natural sciences. The latter are less "liberal" and are recruited from somewhat higher social backgrounds. Gross categorization of faculty members into Protestant, Catholic, and Jewish background is not very important. There is every reason to expect that a more comprehensive approach based on social structure and personality would be more fruitful since the processes of self-selection into sociology and the system of professional socialization strengthen and refashion attitudes of academics. Sociology tends to recruit those with a "left of center" orientation; their style of life and their work tend to enhance this outlook.

However, the central question at this point in the analysis of the forms of professionalism in sociology is the link between the personal, social, and

political ideology of sociologists and their performance as teachers and researchers. To what extent are sociologists able to institutionalize their various roles? Are sociologists able to separate personal ideology from scholarly and teaching performance? It makes very little difference that a small minority of radical sociologists, for example, do not believe that such a distinction can be made. The underlying question which is, of course, in good measure beyond the scope of this paper, is the extent to which sociologists as a corporate group are committed to such professional norms, and the extent to which individual sociologists are actually able to act on the basis of them.

One approach to these issues, although indirect, is to explore the relationship between personal ideology and commitment to either the enlightenment or the engineering model of sociological knowledge. If one separates out sociologists who label themselves as radical sociologists, personal ideology does not seem linked to commitment to either model. Elsewhere I have argued that social background variables such as social class or religion per se would be of secondary importance in accounting for professional orientations (Janowitz 1970). Again we are dealing with a broader pattern of social background, social personality, and professional socialization—in particular, experiences in graduate school—plus the reenforcement that comes from collegial contact and support.

Likewise, Lipset has explored the links between theoretical commitment of sociologists and their own personal ideology. His reanalysis of the survey conducted in the early 1960s by Gouldner presents evidence that there was little relationship between the personal political orientation of sociologists and their acceptance of the importance and relevance of Talcott Parsons's theoretical formulations (Sprehe 1967). Despite its limitations, Lipset's analysis demonstrates one element of professionalism among sociologists. However, the strains within sociology as an academic profession since the 1960s have been enormously increased.

Clearly the links between professional norms and ideology require detailed case-by-case analysis of actual performance. However, from the larger Carnegie Commission sample one can separate out 1,368 sociologists whose teaching appointments were primarily in departments of sociology. They were asked for responses to the statement, "A man's teaching and research inevitably reflects his political values." Although ambiguous, this question indirectly measures one set of professional norms. Clearly the 14.2% who strongly agreed reject or have strong doubts about either the feasibility or validity of separating professional performance from personal ideology. On the other hand, the 10% who strongly disagreed and the 39% who disagreed with reservations are, at least in principle, committed to this norm. It is difficult to evaluate the responses of those 37% who agreed with reservations. Seemingly, after a period of intense politiciza-

tion, there is still widespread acceptance of the professional assumptions that sociological performance is distinct from personal ideology, but there is an important minority who do not have or who have abandoned such a belief. Thus, research into the beliefs of sociologists is relevant, not because of the clarity and richness of explanation of the factors that account for professional and political ideologies, but because the personal ideology of sociologists helps explain their persistent interest in an "applied" sociology.

THE SEARCH FOR AN APPLIED SOCIOLOGY: FIRST PERIOD

Because of their liberalism, many academic sociologists have wished to "apply" their knowledge directly to the larger world or to train "applied sociologists," rather than be satisfied with the teacher role with its indirect and diffuse consequences. Some have seen "applied sociology" as inherent in their philosophy of science. But in each department, such sociologists have had colleagues with a strong skepticism about these efforts.

Thus, it is not strange that the professional issues of sociology have been framed in terms of "basic research" versus "applied research," although the distinction is of limited analytical relevance. In the 1930s and immediately after World War II sociologists drew upon psychoanalysis as a model either for developing a theory of human behavior or for the pursuit of a treatment methodology.⁵ Although this interest has declined among sociologists generally, much of the format and practice of the American Psychological Association was adopted by the American Sociological Association.⁶

In fact, the merits of an explicitly organized applied branch of sociology was debated from the establishment of sociology as an academic discipline in the university system. Sociologists almost universally believed that their discipline would have social consequences, if only to sensitize political leaders to the limits of planned intervention. However, no real consensus emerged from these debates. Manifestations of these differences have varied over the years. The earliest period, in the 1890s, to the outbreak of World War II was one of development; the second, the years 1945 to the middle or end of the 1960s, was one of stabilization; and since the latter half of the 1960s, we find counter trends not yet stabilized.

During the initial period, there were sustained efforts to establish an

⁵ "It is indeed one of the distinctions of psychoanalysis that research and treatment proceed hand in hand, but still the techniques required for the one begins, at a certain point to diverge from that of the other" (Freud 1949, p. 326).

⁶ William Goode (1960) has highlighted the cognitive, normative, and institutional differences between psychology and sociology as organized disciplines, and has argued most strongly against the model of the psychologist for sociology.

applied sociology and to develop a set of practice specialties. At the University of Chicago, some members of the faculty were deeply involved in these efforts, as were their graduates in other centers of sociological research and teaching. There is no evidence that a lack of employment opportunities in academic settings was a main cause or even an important factor in these efforts. Certainly, concern about employment was a major preoccupation during the Great Depression of the 1930s, and many sociologists entered government and public service during the New Deal as an immediate alternative, but the expanded role of government offered new opportunities to sociologists. In particular, a group of outstanding sociologists entered the Bureau of Census and related agencies to engage in demographic work and allied social statistics. They served mainly as research specialists and created a model which is directly applicable to the present concern with social indicators and the evaluation of governmental programs of social change.

It seems clear that among university sociologists the interest in applied sociology of particular sociologists represented their intellectual convictions, institutional aspiration, and personal ideology. At Chicago, Ernest W. Burgess and, later, Louis Wirth had the strongest commitment to the "engineering model." Burgess's collaborator, the personally and intellectually powerful Robert E. Park, was his sharpest critic. But he could not sway Burgess from his comprehensive commitments to institutional building for an applied sociology. For Burgess and Wirth, however, advisory or consultative activities had to be grounded in authoritative methodology that would be internally consistent and produce major consequences for action. There was a strong entrepreneurial overtone in these endeavors and, as a result, sociologists began to study a host of social problems and policy issues.

A highlight of the search for an applied sociology can be seen in the contrasting "natural histories" of the relationships between sociologists and two other professional groups, social work and criminology, prior to World War II. In varying degrees, particular sociologists sought to use their discipline as an alternative route into or as alternative bases for professional practice in these two areas. Although they were for a time more successful in criminology than in social work, these efforts essentially failed; new practice specialists within sociology did not emerge as they did in psychology. Instead strong intellectual, conceptual, and research links emerged which, after World War II, became institutionalized.

Professional schools of social work—dedicated to the notion of scientific philanthropy—were established at about the same time that sociology became an academic discipline. From its inception, sociology had therefore to contend with an established and ongoing profession which believed that it could and should make use of various academic disciplines—economics

and psychology as well as sociology. While, originally, sociology had intellectual associations with the concerns of social work, professional relations were diffuse and at points antagonistic. This remained the case up through the outbreak of World War II. After all, in popular imagery, sociology and social work were often seen as the same, and sociology may have derived its only relevance from its supposed fusion with social work.

However, the realities were much more complex. Many social workers viewed sociology as a source of information about social welfare problems and a limited number of sociologists taught in schools of social work, but the field of community organization which would have supplied one of the major links between sociology and social work was in effect moribund. There was little systematic effort to incorporate sociological conceptions into this specialization. Because sociologists were developing their concerns with the local community and their ideas about the potentials for community organization, the main intellectual interchange between them and selected social workers focused on the settlement house, although even this brought few changes. Thus, sociologists with an "applied" perspective sought to bypass social workers and directly involve themselves in local community programs. Sociologists and their students had had continuous contact with settlement houses and community organizations. Service in such agencies was conceived as a *rite de passage* for sociologists that would transcend the limitations of social work and develop the appropriate critical attitude—even mild contempt toward social work.

The New Deal only served to heighten sociologists' search for alternative routes to "institutional building" in the urban community (Janowitz 1970). The massive reforms of the New Deal were based on economic and welfare measures that were national in scope and categorical in character. While the importance of these measures was fully recognized, most sociologists were also sensitive to their inherent limitations—they failed to create new forms of social organization required to improve the institutions and character of the local community. They were aware that important experiments in community development, including new forms of administration and citizen participation, were underway in rural areas. Comparable experiments in urban areas were very limited during the New Deal period. Thus, some sociologists sought to apply their notions of social change to the reconstruction of the slum and the social control of delinquency. For example, in the 1930s under the intellectual influence of Burgess, the Chicago Area Project embodied the essential conceptual elements of the community action programs of the 1960s. The goals were to break away from conventional definitions of education and social work in order to develop comprehensive institution building which would permit extensive local participation and generate new elements of social control. These experiments involved Joseph Lohman, Leonard Cottrell, Clifford

Shaw, and Henry McKay, and subsequently the network included a group who have been central in recent theorizing and conceptualization about community organization: Lloyd Ohlin, James Short, and Sol Kobrin.

The Chicago Area Project and comparable endeavors attracted considerable attention but had no impact on social work until these efforts and the concepts were "rediscovered" in the early years of the John F. Kennedy administration. Although a number of sociologists have been associated with the more recent experimental and demonstration community projects, few have devoted their careers to such enterprises. Sociologists have served as idea men and research personnel, while the professional cadres were recruited from social work and related fields.

By contrast, sociology in the 1920s and 1930s did train numerous criminologists, and for a period criminology was thought to be a practice-oriented specialty—largely because schools of criminology were slow to be organized in the United States. Sociology contributed to criminology its holistic approach and its emphasis on the social context. The sociologist's perspective received public attention because it pointed to the limits of existing techniques of social control of crime and delinquency and in particular to the limited effectiveness of the prison. The prison has served as the locus for fundamental and thoroughgoing research, and the literature on the prison is still one of the major accomplishments of the sociological effort in analyzing a social system, although it does not have high prestige in the field.⁷

During the 1920s and 1930s sociologists, including those at first-rank universities, sought to train practicing criminologists by offering both masters and doctoral degrees for this specialty. After 1945 this approach was deemphasized, but sociologists did contribute to the development of a more independent profession of criminology. Graduates with higher degrees tended to accept research and staff posts, while others took operational and administrative posts—perhaps less because of specific training than because of individual skills or desires.

Moreover, professionalism in criminology implied participation in a collegial group, and those with a sociological background could, by participation in the American Sociological Association, link themselves to their sociological colleagues through their research interests. While many criminologists continued to affiliate with the ASA and in time moved toward academic or quasi-academic careers, those who made a major commit-

⁷ The traditions included powerful monographic literature from the original work of Donald Clemmer on *The Prison Community* (1940), Gresham Sykes, *The Society of Captives* (1958), to the work of David Street et al., *Organization for Treatment* (1966), plus penetrating theoretical analyses such as Lloyd Ohlin's (1960). This tradition was powerfully enriched and extended by Erving Goffman in his writings on the "total institution" (1962).

ment to criminological practice soon shifted their professional association to groups more closely linked to practice roles.

Sociologists offering training in criminology were aware of the necessity to equip their students with more than a general orientation. They sought to develop an authoritative body of knowledge which could guide practice procedures. In particular, they believed strongly that quantification was essential, partly emulating psychology's development of intelligence testing. As early as the 1920s, sociology developed actuarial prediction studies which assumed that background social factors could be statistically aggregated to predict more accurately a wide range of social behavior for specific individuals. Social background and life experiences could account for success and failure in marriage, in higher education, or on parole.

The decision-making process of parole boards supplied an experimental setting for the utilization of such a technique. Parole boards, typically consisting of lay persons, had to make arbitrary decisions about early release and were willing to accept advice by sociologists as to who would succeed on parole. Although sociologists were trained and appointed to prison systems and did research to update and perfect the statistical techniques and to select the most appropriate variables, the procedures of parole prediction never became as sufficiently institutionalized as did psychological testing or personnel classification procedures. The very limited success of the prediction system, however, was more likely linked to the poor quality of supervision of parolees and the realities of their life chances than to the limitation of the prediction instruments.⁸

THE SEARCH FOR AN APPLIED SOCIOLOGY: SECOND PERIOD

With the close of World War II, the search for an applied sociology centered on the utilization of the sample survey. Of course, social surveys had been in use since the middle of the 19th century in the United States, Great Britain, and France. Some of the early surveys were based on a high degree of intellectual and methodological sophistication. In particular, those sponsored by the Russell Sage Foundation before World War I supplied an important stimulus for local reform.

But the institutionalization of the sample survey after 1930 stemmed from two developments which enhanced the scientific aspects of social research and supplied a new basis for a powerful but again abortive move-

⁸ Sociometric technique was another methodological device for research which had a potential for professional practice. Sociologists have displayed a strong interest in "sociometry." Moreno and his disciples offered the procedure not only as a research tool but a device for selection and reorganization of institutional life. Sociometric technique to the extent that it has become institutionalized employs a rating and evaluation procedure of personnel and is associated with the work of a personnel psychologist rather than a sociologist.

ment to develop an applied specialization in sociology. First, a social psychological perspective was incorporated in both the collection and analysis of survey data. Along with concrete "social facts," the social survey included the respondent's attitudes and feelings, and thereby attracted new clients who believed that these motivational findings were important. Samuel Stouffer, one of the major specialists in attitude surveys, had been a student of L. L. Thurstone, who laid the intellectual basis for the social psychological dimension of the sample survey in his classic article (1928), "The Measurement of Opinion." By means of psychophysical techniques, he demonstrated rationale for attitude measurement. The development of the cross-sorter for handling survey data should not be overlooked.

During World War I, psychologists came in contact with government officials and businessmen in connection with the mass intelligence testing undertaken for the War Department. After World War I, psychologists organized corporations to undertake attitude surveys, morale studies, market research, mass media audience studies, and advertising pretesting. Soon specific agencies organized mainly for profit, especially after it was discovered that the results of such opinion polls as the Gallup Poll, could be widely disseminated because newspapers became important consumers of these studies.

In the late 1930s, three social scientists emerge who raised standards and related survey research to academic departments: Rensis Likert, Clyde Hart, and Paul Lazarsfeld. In 1946, with his administrative assistant Angus Campbell, Likert developed a unit at the University of Michigan which emerged as the largest academic related survey research agency; Hart became director of the National Opinion Research Center when it moved to the University of Chicago in 1947; and Lazarsfeld organized the Bureau of Applied Social Research at Columbia University in 1940.

After 1945, Lazarsfeld, with Bernard Berelson and Hans Ziesel, formulated a new type of sociologist, an applied survey research specialist whose generalized techniques could be utilized by a wide variety of clients. They proposed advanced training centers and institutes of applied social research with advanced degrees including the Ph.D. The model was that of the operations research engineer; such graduate professionals would be employed in nonacademic settings. After 25 years, survey research units have become integral parts of the American university system, but they have not succeeded in creating a well-defined specialization of applied sociologists, although they do contribute a limited number who are employed outside the university.

While most survey research units were formed on the model of consulting psychology, those that have become attached to an academic setting have pressed to publish the results of their studies and to conform more

and more to university research standards. The availability of government and foundation grants enabled selected survey units to move in this direction, and away from specific client relations. However, in part because of the persistent problems of fund raising and in part because of the explicit commitment of key figures to the idea of an applied social research, these units have maintained strong elements for "servicing clients." At a minimum, they have institutionalized feedback sessions and have developed the format of the special client's report which is organized with a view of optimum usage by the clients, but which, in effect, usually guarantees a short intellectual life for the document. The Likert group at the University of Michigan has in fact organized a separate unit designated as the Center for the Utilization of Scientific Knowledge.

Thus, on balance, the academic professors rather than the applied social scientists dominate these survey groups especially with the passage from prominence of the original charismatic leaders. The result is that the second generation of leading figures is concerned with academic tenure and a code of operations which converges with the teacher-researcher. Survey research training has become part of graduate training, and an important fraction of theses are based on their data. But the graduates are in fact oriented toward traditional academic employment rather than "applied settings." In one period, students of the Bureau of Social Research entered commercial and advertising settings in New York City, but the demand for such personnel has been limited and, with new research technologies for market and media research, even this pattern of recruitment and placement declined.

The "natural history" of survey research centers, both the major and the smaller ones, in the United States before 1970 meant that an essential strength of the survey method was neglected—namely, trend studies. Partly because of their funding problems, these survey research centers did not emphasize the long-term collection of sociological and social psychological trend data. In fact, until 1970, the commercial agencies had more and better trend data than did the big three academic based centers.

In the late 1960s and early 1970s, however, there was a rebirth of interest in trends and social indicators deriving from the thinking of William F. Ogburn (1922) and other earlier sociologists (Sheldon and Moore 1968). Aided by federal funds, the major survey units are emerging as national laboratories for the collection and storage of basic social indicator data. Well-grounded data of this sort can overcome the short-time perspective of recent sociological writing and facilitate the emergence of an "applied sociology" that will appeal to various public and private policy makers. Improved data collection is not only indispensable for social research but supplies essential information in the enlightenment model.

Professional organization of survey research specialists, interestingly

enough, has not taken place within the sociological fraternity, but instead in the American Association for Public Opinion Research (AAPOR) and its international counterpart, the World Association for Public Opinion Research (WAPOR). These groups, which are loose amalgams of persons with varied backgrounds, the overwhelming bulk of whom are not sociologists, have been preoccupied with professional standards but have not yet had any major effect. The success of nonacademic survey enterprises—commercial or nonprofit—seems to depend on the administrative skill of the organizer. Moreover, while interest in client-sponsored survey research has persisted and even grown during the 1960s, it has encountered competition from other methodologies such as operational research analysis and analysis of organizational records. As a result, survey research is likely to persist as a multidisciplinary effort with an important core located in the university setting and many satellite activities operating as commercial enterprises. In this case also no practice-oriented specialty has emerged.

SOCIOLOGISTS AND THE PUBLIC COMMISSION

The “natural history” of sociology has included participation of sociologists in a series of public commissions of inquiry into basic social issues which may well have had a greater impact on U.S. society than the work of “practice”-oriented sociologists. Before World War II, there were three conspicuous commissions which had essential common features; the Chicago Commission on Race Relations, President Hoover’s Research Committee on Recent Social Trends, and the Carnegie Corporation’s sponsorship of Gunnar Myrdal’s investigation of the American Negro which can be considered a quasi-public commission (1944). After 1945, the scope and frequency of sociologists’ participation in public commissions increased and their forms were more varied. Three can serve for comparison: the National Advisory Commission on Civil Disorders (the Kerner Commission), the National Commission on the Causes and Prevention of Violence (the Eisenhower Commission), and the Task Force on Pornography and Obscenity.

The three prewar commissions produced significant research results for the development of sociology and, at the same time, important contributions to public understanding, perhaps largely because they conformed to the enlightenment model. Problems were analyzed in fundamental sociological terms; and the analysis which resulted became standard exemplifications of sociological scholarship. The participating sociologists accepted their dual responsibility of engaging in comprehensive research and at the same time producing a document which served a variety of publics, essentially an extension of the teacher-researcher role. Their findings received widespread attention in the mass media not because of the specific recom-

mendations they made—important though they may have been—but for the deeper public understanding they generated. One can be intrigued by the hypothesis that the high scholarly quality of these early commissions, rather than any special policy orientation, resulted in their significant impact, although there sometimes was prolonged delay in their full impact.

The earliest of these commissions, the Chicago Commission on Race Relations, was established in 1919 by the governor of Illinois at the request of several citizen delegations in the aftermath of the Chicago race riots. The director of research and the person responsible for the collection of the research materials and contents of the published volume was Charles S. Johnson, a Negro and a sociologist, later president of Fisk University. Johnson was a graduate student at Chicago, where he worked with Park who became actively associated with the work of the commission. Three members of Park's seminar on collective behavior were also on the staff of the Commission. The final report, entitled *The Negro in Chicago* (1922), carried the strong imprint of Park's concepts and categories.

The staff of the commission issued a report on the "natural history" of the riot, including an analysis of the underlying causes and the role of the various agencies of social control in dealing with the outbreak, which resulted in an unsurpassed sociological case study using a theoretical framework of collective behavior and social control.

President Herbert Hoover's Research Committee on Recent Social Trends constitutes a similar commitment of sociologists to the enlightenment model. Hoover called for a comprehensive report on social change in the United States which would assist both government agencies and private groups to plan "intelligently." The survey was actively supported by Wesley C. Mitchell, the economist, and Charles E. Merriam, political scientist, who were active in the founding of the Social Science Research Council and who became chairman and vice-chairman of the committee (Karl 1968). William F. Ogburn, of the Chicago department, and Howard Odum, of the North Carolina department, were director of research and assistant director of research. The effort, enormous by the standards of that time, was financed by the Rockefeller Foundation by a grant of \$500,000.

The major publication of the commission, *Recent Social Trends* (1933), and the supporting monographs constituted a major landmark in the development of social research in the United States. The intellectual direction of the investigation reflected Ogburn's concern with the development and assembling of statistical indicators of basic social trends. He assumed that the collection of these data were essential both to test basic theories of social organization and social change and of prime importance to groups concerned with planning. The reports of the commission were extensively

circulated, and Ogburn's conception of "cultural lag" (1922) entered the rhetoric of public discourse and supplied a stimulus to debates about social planning and institution building. Unfortunately, the depression sidetracked interest in favor of programs that responded to the immediate problems (Karl 1969).

The full consequences of the Research Committee on Recent Social Trends were themselves illustrations of cultural lag. The notion of indicators of social change had been formulated and the extensive research and documentation of the commission temporarily intensified interest in the subject, but after 1945, interest in self-contained and formal theorizing in sociology led to a sharp decline of interest in indicators. In the late 1960s, there was a revival of interest. While the methodology became more sophisticated, social indicators continue to be based on Ogburn's writings; they continue to be both tools for testing theoretical models and data to guide social policy.

Gunnar Myrdal's undertaking which led to the publication of *An American Dilemma* (1944) and the associated monographs also met the common criteria of the first two commissions: a major piece of social research and a report with a broad impact on public affairs. Myrdal was chosen director because the Carnegie officers wished to bring in a foreign scholar from a country without a history of imperialism. The product of Myrdal's large staff, *An American Dilemma*, was widely discussed in the mass media and led to wide debate on the position of the Negro in American society. For more than two decades it was used as a textbook in the social sciences in American universities and colleges. Myrdal was an economist with strong sociological interests, but he included on his staff numerous outstanding sociologists. Much emphasis has been placed on Myrdal's analysis of values and social research, but, important though his analysis may have been, it hardly constituted the central contribution of the effort. As Edward A. Shils points out, Myrdal's study occurred at a time when sociologists were turning away from the study of race and ethnic relations (Shils 1970), and the report undoubtedly stemmed the decline in such investigations that might otherwise have occurred due to the waning influence of the "Chicago School" in the 1950s. The findings served not only as a massive source of documentation but as the basis for conceptual and theoretical formulations such as rank order of discrimination, patterns of interracial marriage, and style of Negro leadership.

In assessing these early commissions, the key sociologists performed with a high degree of professional responsibility, not only as collectors of empirical data, but as writers of clear reports set in a sociological framework which served to enhance the ensuing public debate of policy alternatives.

There were no comparable public commissions in scope and stature involving sociologists in the years 1945-60. While this decline was partly

due to the impact of Joseph McCarthy, it was also a period in which sociologists were beginning to operate on their own as individual consultants with governmental agencies and private enterprise. Also in those years, sociologists were engaging in other professional efforts such as those by the "social psychologists" to adjudicate their relations with psychologists. However, these efforts to solve the question of certification availed little to either group.

Again in the 1960s, there was a proliferation of public commissions and national studies, but a full evaluation of the activities after 1945 still requires more historical distance. One is often struck, however, by the absence of the kind of integrated sociological perspective which characterized the early commissions. Perhaps the generous funding which brought with it increased bureaucratization deterred the kind of intellectual leadership which could produce a more unified approach. In any case, sociologists in recent commissions seem to have had more diffuse and less central influence. Thus, the basic report of the Kerner Commission cannot be characterized as a profound or penetrating document which will have an enduring intellectual existence. It lacks both careful documentation and a pervasive perspective for analyzing the position of the black community in the United States. The focal term "institutional racism" appears to be more a label than a basis of analysis, and it has strong elements of a limited tautology. This commission did sponsor the collection of important data which have subsequently entered the literature by the efforts of individual investigators, but pressure of a deadline prevented these materials from being carefully assessed and integrated.

The Milton Eisenhower Commission (the National Commission of the Causes and Prevention of Violence) represented a very different approach. From the outset, it was decided that there would be a strong scholarly component and its research was codirected by two outstanding and vigorous sociologists, James F. Short, Jr., and Marvin Wolfgang. They organized a strong interdisciplinary effort and in fact organized one part of their staff as a "Task Force on Historical Perspectives." The scope and objective of this commission plus the talent that was assembled resembled the three earlier commissions. Especially important is the fact that the two-volume collection of monographic studies entitled *Violence in America: Historical and Comparative Perspectives*, which contains considerable material of lasting scholarly worth (Graham and Gurr 1969) resembled the publications of the earlier commissions.

In the 1930s, Quincy Wright and Harold Lasswell attempted to introduce the naturalistic study of violence into the curriculum and research agenda of political science departments. To their credit, the efforts of Short and Wolfgang on the Eisenhower Commission helped achieve this

early objective. From an intellectual point of view, however, the final report failed either to integrate effectively the massive findings or to demonstrate effectively the links between the research and policy recommendations.

Otto Larsen, a specialist on mass communications, and Marvin Wolfgang represented the sociological component on the Task Force on Pornography and Obscenity. They undoubtedly represented the majority viewpoint among sociologists—that all censorship should be removed. While the work of this task force was a historical milestone in the formulation of perspectives toward public policy, it is problematic whether its content and research will command lasting attention for its sociological contribution.

Of the variety of national studies conducted during the 1960s, the Coleman report commissioned by the United States Office of Education was the most conspicuous and influential. While basically a research document, it had many overtones of a public commission of inquiry. The effort achieved a new threshold in sociological research because of the massive collection of data and the imposition of systematic modes of analysis. One consequence of the "inquiry" was to intensify the question of privacy in the collection of data. The findings—that a heterogeneous student body optimizes classroom learning—may have been less significant than the stimulus the report provided to new forms of educational experimentation. As a research document, this report focused on specific findings and on a search for statistically meaningful relations. However, as a commission of public inquiry some of its shortcomings became rapidly apparent. It could not and did not probe the alternative arrangements for handling the policy problems it encountered. The document became the major argument for school busing in order to achieve racial and social balance, but it did not examine either the full range of approaches to school busing or alternative systems for achieving greater student heterogeneity or a wider range of social and class contacts. Moreover, it failed to identify teaching procedures, formats of school organization, or patterns of educational leadership which, although statistically infrequent, were markedly more productive and could therefore result in more effective mass education (Janowitz 1971). In the end, its public commission characteristics were submerged, and it remains one of the outstanding pieces of contemporary social research.

No doubt there is an economy of public commissions; the relatively high frequency of such bodies during the 1960s probably implies a much lower number for the decade of the 1970s. Moreover, the accumulation of massive amounts of data and monograph research may call for a changing pattern for public commissions. The task is now to assess and integrate the available documentation. The Surgeon-General's Scientific Advisory Committee on Television and Social Behavior is a step in this direction (U.S.

Public Health Service 1972). A panel of experts was appointed to review the existing research on the impact of television, and especially of violent contact, on young people.

Despite the fact that the final report of this group produced immediate controversy, its contribution cannot be brushed aside. Certainly the selection of panel members is open to question; those research specialists who were concerned with broad systematic consequences rather than specific and micro effects, were eliminated from the original list of nominations. Also, the report did not present majority-minority positions which would have sharpened the issues. Moreover, it failed to identify sufficiently those aspects of the problem which must be dealt with on the basis of political or moral principle. Nevertheless, this commission did produce a public document which, for the first time, acknowledged with some degree of scholarly authoritativeness the negative impact of commercial television.

THE REDEFINITION OF PROFESSIONAL BOUNDARIES

The search for an applied sociology reasserted itself with renewed energy in 1971. First, the theme of "relevance" which had been so prominent during the years of student activism left its impact of university faculties. In the absence of disruptive student pressures, many sociologists felt obliged to judge their work in terms of this criterion, and, for them, the term "applied sociology" had renewed meaning. Others advocated that relevance could be found for students in the format of fewer years of academic work, easier entrance and exit procedures from college life, or a mixture of work-study programs. They argued that in-service training could better be administrated in occupational, professional, and administrative settings off campus.

Second, pressure for a redefinition of the boundaries of the profession was the result of the reduction of federal support for graduate training plus the projected decline in the demand for Ph.D.'s in sociology. Concern about employment prospects in teaching led to renewed discussion of careers in applied sociology.

Estimating the supply and demand for sociologists is a hazardous task, although it is clear that the demands in the 1970s will be markedly lower than in the 1960s. The trends in the growth in the number of sociologists in the years 1967-71 are most revealing in that they demonstrate a heavy decline in the rate of growth even before the reduction in support for graduate students. The rate of growth of the total membership in the ASA for the years 1966-67 reached over 13% and since has steadily and sharply declined until it was below 5% for 1971.⁹

⁹ Closer examination indicates that the greatest growth was in foreign members; for

However, the contemporary concern for an applied sociology rests mainly on factors internal to sociology. It reflects an old and persisting intellectual commitment. It expresses ideological interests and professional concerns about the social role of the man of knowledge which repeatedly come to the forefront. Three self-study reports on the organization of social science and sociology launched in the mid-1960s supply the background for the contemporary debate.

These three reports need to be assessed in terms of the widespread feeling among sociologists that they do not have the prestige, influence, and impact that their knowledge warrants. These self-study groups addressed themselves to this underlying issue as much as to an evaluation of the quality and scholarly character of contemporary sociology—perhaps a more important and antecedent question.

The most comprehensive of these efforts was the so-called BASS report—the Behavioral and Social Sciences Survey Committee—appointed in 1966 by the National Academy's Committee on Science and Public Policy (Social Science Research Council (1969). Beyond a sweeping overview of recent and projected substantive and methodological developments, the committee recommended the recruitment and training of applied behavioral scientists explicitly within the engineering model. The limited intellectual impact of this undertaking can be measured from the almost complete absence of the report from the bibliographies of graduate school instruction and from its failure to generate extensive debate in the professional journals.¹⁰

The other two reports with a narrower but sharper focus on the utilization of social science research were organized by two officials of the Russell Sage Foundation, which has been deeply concerned with the role of sociology in professional practice. A committee, under Donald Young, was appointed in 1965 by the National Academy of Sciences National Research Council to "review the organization and operation of behavioral sciences programs in the federal government" (1968). Orville Brim, Jr., was chairman of the Special Commission on the Social Sciences of the National Sciences Foundation Board (1969). The pluralism and affluence of the United States is reflected in the organization of three distinct but related study groups. However, these two groups displayed greater recognition than the BASS report to the differences between the social role of

1971 almost one-third of the net increase of 657 was foreign. Thus the rate of increase dropped to about 3% for 1971 among Americans; and, in the future, increases among foreign members can be expected to drop as well. While a growing number of sociologists are not members of the ASA, these data indicate a discounting of future demand even before steps were taken to reduce the supply. However, it is, of course, very difficult to judge whether the steps taken in 1970 and 1971 to reduce student enrollment will be effective for any length of time.

¹⁰ For an exception, see Albert Biderman's review, "Self Portrayal" (1970).

the natural and the social sciences and did not espouse simplified programs for training an "applied" behavioral science profession.

On the basis of the inherent logic of sociological inquiry and knowledge, plus the results of past efforts and experiences, there is little reason to assume that sociology will soon develop a strong practice-oriented specialization. A great deal of valuable energy can be lost in such an enterprise which would weaken the intellectual vitality of the profession. On the contrary, there is every reason to assume that sociology will remain basically a staff-oriented profession rooted in academic and education settings. Even more pointed, the bulk of new cohorts of sociologists will remain oriented toward a career in the academic setting. In fact, the key question in the training of new cadres is their appropriate involvement and exposure in nonteaching experiences as part of their training.

Such assumptions do not rule out marked changes and redefinitions in the boundaries of the sociologist as a professional group. Nor does it rule out the likelihood of new roles compatible with a staff orientation or the clarification of old ones. Four problematic areas can be identified.

First, the most probable area of innovation will focus on the central clients—the students of sociology. The underlying question remains: who shall be the students of teaching sociologists. The American Sociological Association has invested considerable effort and government funds in the preparation of teaching materials for high school students, on the belief that these young people should be exposed to sociological materials. There is little evidence to project a marked growth enrolled for sociological instruction at the high school level. In this connection, it is noteworthy that a variety of sociologists of different theoretical and methodological persuasions—James Coleman, Albert J. Reiss, Jr., and David Riesman, for example—have expressed strong reservations about sociology in the high schools. New directions will likely extend sociological instruction to more mature students, students in professional schools, adults in the midst of their occupational careers, and students entering or reentering post high school institutions after work and community experience. Old directions will continue to mean that sociology will provide one form of liberal or general education for students entering a variety of occupations, especially those in the public sector.

Second, it appears likely that there will be some growth in nonacademic research centers which may have a limited number of new posts for sociologists. In particular, a major development in the sociological enterprise both for the needs of the discipline and for public and social policy will be the expansion of large-scale data collection centers and laboratories concerned with trend analysis or social indicators as the term is currently being used (Horowitz 1968). The period ahead parallels the introduction of sociologically trained demographers into the Bureau of the Census and

other governmental agencies in the 1930s. For data collection and analysis of social indicators, the sociologist will share responsibilities with the social statistician.

Third, sociologists will continue to be drawn into various consultative services. However, the role of either full-time or permanent consultant for a sociologist remains unstable and difficult to institutionalize (Wilensky 1956). Instead, sociologists are likely to serve as consultants for short-term and specific assignments. Sociologists from academic and educational settings will be the main source of such personnel; therefore, their services require an ethic which will not distort their teacher-researcher role. Interestingly enough, discontinuous and limited consultation can insure higher standards, and the responsible sociologist must publish the findings he accumulates from such experiences, with due regard to the rights of the individuals involved. The role of the sociological consultant in the enlightenment model can be seen as an extension of the teacher-researcher.

Roland L. Warren in *Social Research Consultation* (1963) has incisively explicated the professional responsibility of the sociologist both to his clients and to social science, and has underlined the central implication of the enlightenment model that consultation is not "hackwork" by lesser minds, but involves conceptual analysis and the creative role analogous to the outstanding teacher. "For if the research consultant is to honor his obligation to the client organization, as well as to behavior science and research methodology, he must not only be aware of the need for conceptual analysis of the social process in which he and the client system are involved, but must develop the social interaction skills which this dynamic conception of the research consultation demands" (Warren 1963, p. 141).

In the consultative role, the sociological equivalent of the public defender has not developed; a central issue is to make available consultative services to groups not familiar with such resources or unable to engage such talents. Minority organizations, labor unions, cooperative and community agencies do not have adequate access to existing social research facilities. New independent agencies, and essentially nonprofit ones, have been gradually developing to meet such needs. Thus, for example, under the leadership of Kenneth Clark and Hylan Lewis, a Center for Applied Metropolitan Research has been established in New York City to focus on social issues for the black community.¹¹

¹¹ One is struck by the fact that various community leaders are aware of the need for professional standards. At a 1971 conference on community organization, a militant black community leader requested a well-known white sociologist to undertake a piece of research which would evaluate the effectiveness of an experimental education program in which he was deeply involved. When the sociologist raised the question of research access, the black community leader indicated that he felt that such problems could be overcome by *tact* and patience, but he insisted that the result of the study

Fourth, it remains to be seen whether the professional associations of sociologists will develop and modernize the contributions made by earlier public commissions. The American Sociological Association in 1968 established a Committee on Social Policy which was charged with this type of objective but failed to formulate effective procedures. However, there is real promise in the format of a professional equivalent of the British Royal Commission, which would seek to assess the policy implications of social research. The objective—majority and minority reports where agreement cannot be readily achieved—is not to collect new data but to synthesize existing findings and draw attention to alternative policy implications. Such a mechanism would in effect be implementing the enlightenment “model” since it would not be limited to a particular client but would be related to a series of broad publics and would not seek to displace the role of elected officials or institutional leaders.

Albert Biderman has argued that “as they [the social sciences] have become larger and more consequential, they have increasingly become objects of public attention: the social sciences themselves have become social issues” (Biderman 1970, p. 1064). Such a formulation tends to underemphasize the extent to which, in the past, sociology as an academic and staff profession has been subject to internal and external controversy.

Contemporary controversy about the social and professional roles of sociologists reflect the social and political tensions of the larger society. No doubt, new elements in the debate include the sheer growth and increased visibility of sociologists. Before World War II, most sociologists felt that the maintenance of academic freedom was their crucial and essential professional problem. Of this period, Everett Hughes has written, sociologists realized that their sociological theories were critical of the existing mores (Hughes 1970). Sociology and the teaching of sociology have supplied life space for students who were from particularistic subcultures and who were seeking entrance into the larger cosmopolitan world. He spoke of teaching as a kind of field work; the intellectual endeavor and the intellectual experiences were liberating for teacher and student.

In the contemporary scene, conflicting notions of the consequences of social knowledge and extra-university obligations have emerged as of equal or greater importance. Social and political movements have arisen within the university which called into question the traditional format of the sociologist as teacher-researcher. In the well-known extreme version, the

would have to be submitted to a professional journal for publication. As an explanation, he indicated that “if I go out and hire a commercial research organization—white or black—to work on a black community problem, they will very likely give me the answers they think I want to hear. Only if I get your professional self-interest involved will I know that the results are worthwhile; and if you are going to publish them, I know that they will be good and that you will not run out when the heat gets turned off.”

intellectual effort of the university is seen in opposition to revolutionary change. While such an outlook commands very little support, it is the view held by campus activists—both faculty and student—that if it is to survive, the university must be a direct and immediate agency of social and political change. It is not enough, they say, for the university to be an indirect source of change through teaching and research. Such a viewpoint draws on the traditions of the land grant universities and their orientation toward public service. The specific recommendations are numerous and any assessment of their implications is a complex task, but for the purposes at hand it needs to be emphasized that there is often a persistent element of mistrust of reason in such formulations and recommendations.

Reinhard Bendix has argued that the form and content of modern sociology, because it has engendered a mistrust of reason, has contributed to the undermining of its own enterprise and of the university in general. No doubt, sociology and the university can be faulted for many major errors, one being the endless pressure for expansion. Sociology may also have neglected to place sufficient emphasis on key topics such as the social and psychological preconditions for a democratic society. But I believe that the reverse of Bendix's argument may be even more correct. The impact of sociology, it can be argued, has been to contain and reduce the mistrust and revolt against reason. Without a vigorous sociological inquiry in the enlightenment model, the essentials for a free society are weakened. If sociologists perfect their discipline, and understand that they are part of a staff-oriented profession—academic and nonacademic—and if they assume their responsibilities to their various publics, they will make their maximum impact and contribution. We are not dealing with a Hegelian dialectic in which rational inquiry produces its own seeds of self-destruction but a pragmatic thrust in which human motives and voluntaristic commitments fashion and are fashioned by rational pursuits.

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On Gouldner's *Crisis of Western Sociology*¹

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The Coming Crisis of Western Sociology (1970), by Alvin W. Gouldner, is a highly provocative and stimulating work about the development of sociological theory and may prove to have an influence on the future development of that subject. After analyzing a few of the dominant social theories from the beginning of the 19th century to the present, Gouldner concludes that sociology is presently in the early stages of crisis.

He contends that every social theory has an "infrastructure" consisting of a number of "domain assumptions" learned by the person before he became a sociologist, which, although not a part of the theory, leave an indelible imprint upon it (p. 46). These domain assumptions concern conceptions of the nature of man and society, such as that men are rational or that social problems will correct themselves without planned intervention. They may reflect value judgments as well as purely testable assertions. The infrastructure affects not only the way a theorist constructs a theory and what it states but also its chances for acceptance by some audience, whose own domain assumptions it may or may not "resonate" (pp. 39-40). Gouldner goes on to contend that when dissonance occurs between the infrastructure of a hitherto accepted theory and a rising structure of antithetical assumptions, the science in question is ripe for crisis (p. 34). And, according to Gouldner, this is what is now happening to sociology.

Since *The Coming Crisis* treats of ideas central to the discipline, sociologists need to take the work seriously, scrutinize it closely, and evaluate it carefully. Despite its importance for sociology and the provocativeness of its conclusions, however, the work contains serious errors about the development of social theory that can leave misleading impressions about social theory and its future direction. To correct these impressions, I hope, with the use of documentation drawn largely from the original works of the theorists Gouldner discusses, to disclose some of these errors. Second, I want to dispute Gouldner's conclusions about an impending crisis in sociology. The second of these objectives is related to the first insofar as Gouldner bases his conclusions about the crisis on his analysis of the development of social theory.

¹ I am grateful to Professor Eleanor Godfrey for her helpful review of the style and substance of this manuscript.

SOCIOLOGICAL POSITIVISM AS A COUNTERBALANCE
TO UTILITARIANISM

In his model² of the development of western sociology, Gouldner contends that the social theory of the first period, the period of the sociological positivism of Saint-Simon and Auguste Comte, was constructed to a large extent as a counterbalance to the cultural utilitarianism of the middle class. Gouldner distinguishes cultural utilitarianism as a technical philosophy, and concludes that it was cultural utilitarianism that stimulated the theories of Saint-Simon and Comte rather than the philosophical utilitarianism of Jeremy Bentham and John Stuart Mill. The principal characteristic of cultural utilitarianism was the importance of utility as a standard of evaluation, according to which men were to judge others by what they achieved rather than who they were. With the rise of the bourgeoisie, achievement supplanted ascription as a dominant value, for the middle class successfully opposed the value of utility to the ascribed values of the aristocrats (pp. 61-65).

The rise of utilitarianism, however, brought forth some dire consequences. The evaluation of actions according to how effectively they achieved certain goals introduced relativity into men's thinking, weakening traditional standards (p. 66). If one action failed to achieve a goal, no matter how much it conformed to tradition, the thing to do was to substitute another action that would achieve the objective. Thus, moral judgments were subordinated to cognitive ones; the focus of action shifted to the achievement of useful objectives, and purely technical requirements became uppermost (pp. 66-67). As an efficacious means to attain ends, power became divorced from morality, for the attainment of an end was what mattered and not the morality of the end or the means by which it was attained (pp. 84-85). In sum, in its preoccupation with success and failure the utilitarian thinking of that period possessed an inbuilt disposition toward anomie, Gouldner says.

He goes on to say, moreover, that the unique historical context of this utilitarian-produced anomie was Restorationist France. The collapse of the Old Regime after the French Revolution and the decay of the church's moral authority both contributed to the need for new beliefs. Men had to know what kind of society France should be, but the Restorationist elites and the middle class held divergent ideas. Thus, the need to remake France and the need to counteract the anomic pathology of the cultural utilitarianism of the middle class presented social theory with the

² The discussion of Gouldner's work must be selective, for it is not possible to deal with all of even his major arguments. Of his major points I have chosen those that are debatable and particularly those that are most crucial to his case for a crisis in sociology.

task of remapping the social world and, through reducing ambiguity and anxiety, making it whole once again (pp. 83–84).

Saint-Simon and Comte heartily responded to this twofold challenge with their theories of sociological positivism. Positivistic sociology, particularly in Comte's version, presented the middle class with a map-making technique based on scientific method, on the one hand, and a religion of humanity, on the other. The religion of humanity congenially resonated with individuals' needs for new moral beliefs, and the map-making system resonated with their commitment to the scientific method that was winning allegiance. Thus, the origin of social theory was informed by certain domain assumptions and unfulfilled sentiments of the middle class.³

Utilitarianism and the problems it posed reappeared in the future development of social theory. As Gouldner sees it, the importance of utility was reaffirmed in the functionalist theory of classical sociology (pp. 124, 133), and the attempt to remap the social world in order to unify it was later evident in the social theory of Talcott Parsons (pp. 199–209). Thus, the domain assumptions of the theory of the first period of Western sociology continued to influence sociology as it evolved.

Since the middle-class orientation of social theory in the founding period is a central point made by Gouldner, a closer look at Comte's intentions and his audience may prove revealing. In a letter to the czar of Russia, Comte stated: "In France my attitude as a philosopher is necessarily anomalous, *because the unfitness of the upper class obliges me habitually to appeal to the lower*; a course which gives a revolutionary colour to the best counsels [*italics mine*]. In Eastern Europe alone can enlightened theories now find chiefs disposed to appreciate and able to utilize them. This contrast is a natural result of Western anarchy" (Comte 1876, p. xxiv). That Comte was concerned with appealing as much to the working class as to the middle is also the opinion of W. M. Simon,⁴ who contends that in Comte's view the workingmen and women were ripe for a new philosophical appeal because their minds had not been contaminated by metaphysics (1963, p. 11).⁵ In the preface to *The Catechism of Positive*

³ Gouldner admits that Saint-Simon and Comte wrote within a political context that also included the aristocrats, but on balance their principal target was the middle class (pp. 91, 106, 111): "After the sprawling genius of Saint Simon, Western Sociology underwent a kind of 'binary fission' into two sociologies. . . . One was Comte's program for a 'pure' sociology . . . the university sociology of the middle class, that achieved its fullest institutional development in the United States. The other was the sociology of Karl Marx, the party sociology of intellectuals oriented toward the proletariat" (p. 111).

⁴ Simon, an intellectual historian, has researched the history of positivism subsequent to Comte's death.

⁵ Simon compiled an occupational breakdown of a positivist group of about 200 in

Religion, Comte designated as positivism's "true social audience" women and "proletaries," who were the only groups able to appreciate the need for a new philosophy. Hence, it was necessary to acquaint the public with philosophical terms (1883, pp. 9-10).

The fact is that Comte did press his case for a reconstructed society to a wider audience than the middle class. His writings reveal his trust in the working class's ability to play a crucial role in the new society—a role that could not be fulfilled by the more privileged members of society, who, contaminated by false philosophies, had already demonstrated their incompetence in official positions. There is little doubt that Comte intended to "resonate" his theories in the minds of middle-class individuals, but this was not his only audience; to argue for the middle class as a selective focus of his work is to mistake the part for the whole of his orientation.⁶

We must also be dubious of Gouldner's attempt to connect sociological positivism with the unique historical context of Restorationist France alone. According to Gouldner's formulations, the construction of a theory is affected not only by its infrastructure but also by its chances for acceptance; the theorist and his receptive audience share domain assumptions (pp. 39-40). In the light of these formulations it is instructive to note positivism's diffusion to England. There Richard Congreve became the founder of a formally organized Positivist Church in 1870. It was precisely the religious element of Comte's philosophy that fascinated Congreve. Simon quotes Congreve as saying, "It is a new religion I am engaged in putting forward and . . . I claim as much as the ministers of the existing faiths" (1963, p. 49).

Although there was a school of orthodox positivists in France after Comte's death, a brand of positivism that rejected the organized religion of humanity also appeared there around the leadership of Emile Littré. Congreve's church in England and Littré's positivism in France are cases which argue against Gouldner's explanation of sociological positivism. Although the social structure of England was *not* shattered by a revolution as occurred in France, Comte's religion diffused to England through Congreve's church. On the other hand, the purely secular elements of Comte's philosophy persisted in France under Littré despite the presumed need for a new religion after the destruction of the old order by the French Revolution.

Another of Gouldner's conjectures about sociological positivism deserves comment. He conjectures that society's failure to accept and institutionalize

France and found that 25% were manual workers. Although certainly not definitive, these figures indicate that workers were receptive to Comte's positivism (Simon 1963, pp. 69-70).

⁶ A distinction must be made between the class position of the theorist and the class whose domain assumptions the theory resonates. Gouldner is concerned with the latter.

Comte's religion of humanity was one of the factors that led to the adoption of value neutrality by social theory. According to Gouldner, value neutrality was the "anomic adaptation" by the positivists to their own political failure and their political impotence (p. 102). Frustrated in the political arena, the Comtean faction of positivism relinquished the grand design of the religion of humanity to become exclusively preoccupied with the methodology of map making instead of the map itself. As a result, value neutrality and the idea of sociology as a science became institutionalized in the academic sociology of the classical period.

This conclusion is questionable. It is not true that positivism relinquished the map-making function of religion when it failed to win the latter's acceptance, for the religion of humanity was retained by some positivists in France down to World War I (Simon 1963, pp. 68-69). In addition, the notion of value neutrality was not institutionalized in the sociology of the classical period insofar as Emile Durkheim subscribed to a science of ethics (Durkheim 1964a, chap. 3). Durkheim's well-known attempt to establish scientific rules to distinguish normal from pathological elements in society was inconsistent with value neutrality and consistent with Comte's philosophy.

THE CLASSICAL PERIOD

The reaction of social theory to middle-class utilitarianism continues into the classical period, according to Gouldner's reconstruction. Although the two theorists of this period were Emile Durkheim and Max Weber, Gouldner's analysis is directed primarily to the former. The centrality of utility continues into classical sociology and lays the foundation of Durkheim's theory of functionalism.

Functional theory required that the sociologist explain a social fact with reference to its contributions to the maintenance of other institutions—in other words, with reference to its utility. It asserted that if an institution survives, the latter must be contributing to social welfare in some way. One obvious set of social facts contributing to society's maintenance is moral values that restrain men's appetites, regulate occupational roles, and urge the poor to be content. In Durkheim's version of functional theory moral values reduce anomie. The search for functions replaced traditional legitimations of institutions with the legitimation of usefulness. The domain assumption that for an institution to be legitimate it had to be useful resonated congenially with the sentiments of the bourgeoisie (pp. 119-24). At the same time, the assumption joined utility with morality within the theory itself.⁷

⁷ Gouldner recognizes that Weber, the other major classical sociologist, took a different view of the role of moral values and of functionalism from the views of Durkheim.

Furthermore, functional sociology of this period was absent in Britain, but the theory appeared in British anthropology instead. The explanation of functionalism's absence in British sociology, states Gouldner, is to be found in the peculiar development of the class struggle in Britain. Because the middle class feared the French Revolution and the working-class movement, it accommodated itself to the aristocracy rather than opposing it as in France (p. 125). With the middle class and the aristocracy fused into one composite ruling elite, middle-class utilitarianism did not come to dominate British culture; hence, a functional sociology of industrial systems did not find a congenial structure of sentiments with which to resonate. However, a functional anthropology, developed by Malinowski and Radcliffe-Brown, was attuned to Great Britain's colonial concerns (Gouldner 1970, pp. 131-34).

There is little doubt of the functionalist perspective in Durkheim's social theory (1964a, pp. 95-97), nor is there any doubt of the centrality of moral values (1964b, pp. 129-30; 1951, pp. 209-16, 321-25). Gouldner is correct. Nevertheless, there is another side to Durkheim's treatment of values, which Gouldner, in his enthusiasm to relate the stress on moral values to cultural utilitarianism, fails to consider. In *Suicide* Durkheim observed that too great a consensus on moral values leads to negative consequences, one of which is high rates of altruistic suicide (1951, pp. 220-21). An overintegrated society has a stultifying effect on the personality, and there is "feeble individuation itself"—an observation that softens somewhat his stress on moral consensus.⁸ In short, Durkheim chose a middle ground between the extremes of moral laxity and moral overregulation.

Furthermore, Gouldner's interpretation of the role of utility in functional theory becomes questionable when we consider Durkheim's analysis of religion. In this classic study, Durkheim defined religion as those beliefs and rites relative to sacred things which unite into a moral community those who adhere to them (1961, p. 62). He drew a sharp distinction between sacred and profane things, the latter distinguishable from the former not by degree but by kind. Sacred things inspire the kind of respect that precludes considerations of utility: "We say that an object . . . inspires respect when the representation expressing it in the mind is gifted with such a force that it automatically causes or inhibits actions, *without regard for any considerations relative to their useful or injurious effects*. When we obey somebody because of the moral authority . . . we are not determined by the advantages or inconveniences of the attitude which is prescribed or

⁸ Durkheim wrote: "In the order of existence, no good is measureless. . . . So with social phenomena. If, as we have seen, excessive individuation leads to suicide, insufficient individuation has the same effects. When man has become detached from society, he encounters less resistance to suicide in himself, and he does so likewise when social integration is too strong" (1951, p. 217).

recommended to 'us' (Durkheim 1961, pp. 237-38). Sacred things are also symbols of society and its rules of morality (1961, pp. 236, 244-45).

This religious focus of Durkheim's functional theory contrasts dramatically with Gouldner's interpretation of functionalism as legitimating social arrangements strictly in terms of their usefulness. The purely nonutilitarian aspect of Durkheim's theory is further revealed in the importance he attached to religious ritual. By failing to call attention to these aspects, Gouldner exaggerates the utilitarian emphasis in Durkheim's work.

Although it is true, as Gouldner observes, that classical sociology was weakly developed in Great Britain, the contention that a functional sociology was absent is not true. Herbert Spencer not only designated as a task of sociology to interpret the development, structure, and functions of social aggregates as produced by individuals' actions (Rumney 1965, p. 24), but he also put this methodological precept into practice. He noted the similarities between an organism and a society and proceeded to show how the differentiations of various structures fulfill functions in evolving societies and how such differentiations are paralleled in organisms. In line with his functional focus, Spencer explained the origin of institutions not by men's intentions but by society's structural and functional exigencies (Spencer 1967, pp. 135-42).

Once again a cross-cultural comparison makes one hesitate to accept Gouldner's explanation of social theory as a reaction to utilitarianism. Strong anti-utilitarian elements appear in Durkheim's classical sociology, and strong functional elements appear in social theory in Great Britain, whose class structure, according to Gouldner, militated against utilitarianism.

However, Gouldner points out that there was another ingredient in the context of classical sociology besides utilitarianism-Marxism. After Saint-Simon's death, sociology bifurcated into the Comtean positivism and classical sociology of the West and the Marxist sociology of Eastern Europe. Marxism became a prime target of the classical theorists because it threatened the middle class, Gouldner states (p. 116).

Classical sociology's anti-Marxist polemic was evident in Durkheim's approach to the problem of solidarity in industrial societies. Gouldner contends (pp. 248-51) that Durkheim recognized a defect in the institution of private property. The trouble was that this institution frequently mismatches the talents of individuals with the requirements of occupations,⁹ and this "forced" division of labor undermines solidarity. Nevertheless, Durkheim retreated from a more extended analysis of property and turned his attention instead to the lack of consensus on moral beliefs as the cause of industrial societies' troubles. This direction of his thought

⁹ An example of such a mismatch is an individual who inherits a factory but who lacks the capability to run it (Durkheim 1962, pp. 15-16).

eventuated in his celebrated notion of anomie. Gouldner attributes great importance to Durkheim's retreat from his initial critique of private property. The reason for the turnabout, Gouldner argues, was that to continue this critique would have placed Durkheim in the uncomfortable position of being on the side of the socialists from whom he was striving to differentiate academic sociology.¹⁰

Durkheim's *Socialism* reveals the dubiousness of this explanation. In his discussion of Saint-Simon's proposals for a reorganized society, it becomes clear that what distressed him about the latter's proposals was not Saint-Simon's policies on property¹¹ but rather his vision of a society devoted solely to the encouragement of economic appetites. Durkheim believed that strivings to fulfill such appetites completely are futile, since appetites can easily be multiplied more rapidly than an economy can provide for them. What was lacking in Saint-Simon's vision was a mechanism to restrain men's appetites and adjust them to the possibility of fulfillment (Durkheim 1962, pp. 235-47).

THE ATTACK ON MARXISM AND THE DEFENSE OF CAPITALISM

Gouldner's title for the contemporary period of historical reconstruction is Parsonsian Structural-Functionalism. The title makes it obvious that, in his view, Talcott Parsons is its dominant theorist. The principal elements in the sociohistorical context to which Parsonsian theory responded were World War I, the Soviet Revolution, the rise of fascist movements, and the world depression (p. 144). These events made the international middle class anxious, and Gouldner argues that it became Parsons's task to create a theory that would restore their confidence in the traditional social order and, more specifically, in capitalism (pp. 145-48). If this explanation of Parsonsian structural-functionalism proves correct, Gouldner will have gone a long way in demonstrating the conservative infrastructure of the contemporary period's most influential theory.

Parsons's attempt to defend capitalism and provide an alternative to the Marxian challenge to capitalism assumed the form of his voluntaristic theory of action, which he later elaborated into his comprehensive systems theory. Gouldner singles out two facets of the voluntaristic and systems theories as contributing to the defense of capitalism and the subversion of

¹⁰ Gouldner puts the matter as follows: "If Durkheim had followed up his own lead on the forced division of labor . . . it would have been difficult to tell the difference between Durkheim and Jaurès. If modern Functionalism had pursued Durkheim's critique of the forced division of labor, it too would have to move toward *some* form of socialism" (p. 250).

¹¹ Saint-Simon did not propose to dispossess wealthy "idlers" of their property but merely to exclude them from the exercise of political power (Durkheim 1962, p. 175).

Marxism's appeal. They are the antideterministic tendency of the voluntaristic theory and the emphasis on interdependence of his systems theory.

Gouldner infers (pp. 180–83) that the early Parsons was disturbed that both Werner Sombart and Max Weber agreed with Marx that capitalism is a deterministic system—one that coerced businessmen into seeking profit by exploiting others. This determinism placed capitalism in a bad light; Parsons created an antideterministic theory to restore capitalism's vitality, Gouldner says. The antideterminism of the voluntaristic theory is manifest in the assumption "that men's efforts always make a difference in what happens"; men's actions are shaped by their desires, volitions, choices, and strivings. Furthermore, the choices and strivings of men are oriented to moral values. In the final analysis it is conformity to value systems that makes a difference in society; capitalistic man, therefore, can once again control his destiny (Gouldner 1970, pp. 185–86).

However, Parsons imposed an important qualification on the idea that men's strivings enable them to control their fates. As Gouldner interprets Parsons, men's actions make a difference in what happens, but seldom do their actions actually achieve their goals (pp. 189–90), for there are always unintended consequences of actions. Men are free to strive but not necessarily free to achieve what they strive for. This conclusion, states Gouldner, provided an apt lesson for men in a depression, for it encouraged them to strive to make the capitalist system work. At the same time it admonished New Deal reformers not to go too far because outcomes are not predictable (p. 194). The system was not to be disrupted, for ameliorating changes may be fraught with unpredictable and perilous consequences.

Before proceeding further with Gouldner's analysis of the infrastructure of Parsons's theory, let me reply to his observations about this antideterminism. The only proof he offers for his interpretation is a simple quotation from Parsons: "An end, then, in the analytical sense must be defined as the difference between the anticipated future state of affairs and that which it could have been predicted would ensue from the initial situation *without the agency of the actor having intervened*" (p. 197, n. 17). Gouldner's inference that Parsons implied from this that men rarely achieve their goal is a mistake. In *The Structure of Social Action* Parsons defines an end as the difference between what would have occurred had the actor failed to act and what would occur as a result of his having acted.¹² The difference in outcome may be both foreseen and desired by the actor, and the formulation is consistent with the possibility of an actor's having achieved his

¹² "An end, for these purposes, is a future state of affairs to which action is oriented by virtue of the fact that it is deemed desirable by the actor(s) but which differs in important respects from the state which they would expect to supervene by merely allowing the predictable trends of the situation to take their course without active intervention" (Parsons 1949, p. 75).

end. A footnote on the same page clarified why Parsons defined an end this way: "to include the maintenance of an existing state of affairs as an end, as well as the bringing into being of a state differing from the initial situation" (1949, p. 75, n. 1). Thus, if an actor predicts that a given state of affairs will be maintained without his acting to intervene, and if this state is his end, by refusing to act he will attain his goal. Parsons's definition here does not imply that men seldom achieve their goals but just the opposite.

Furthermore, in the same work, Parsons set forth an "analytical law," which states: "In any concrete system of action a process of change so far as it is at all explicable in terms . . . of the intrinsic means-ends relationship can proceed only . . . toward the realization of the rational norms conceived as binding on the actors in the system. That is, more briefly, such a process of action can proceed only in the direction of . . . rationality" (1949, p. 751). Parsons means by this, following Weber, that there is a trend toward the realization of men's ends in those action systems where there is a conscious attempt to be rational (1949, p. 752). These formulations render questionable Gouldner's interpretation that men do not often achieve their goals.

Gouldner advances another reason for his conclusion of an anti-Marxian infrastructure. Whereas the Marxian model is a single-factor one in which the economy generates a diversity of effects, the Parsonsian systems theory is a multifactor one in which every variable affects every other. This is the familiar notion of interdependence (Gouldner 1970, pp. 228-29; 346-47). Conceiving of society as an interdependence of parts avoids the difficult problem of causal priority and the weighting of the factors. Change is made difficult because the reformer has no direction as to what powerful "levers" of the society he may grasp to change it (pp. 226-29, 346-47).¹³ Here again, Gouldner states, the theory discourages a restructuring of capitalism.

In replying to Gouldner, I note that Parsons posits a hierarchical order of control. At the highest level is the cultural system, which controls the social system, which in turn controls the personality system, which finally controls the behavioral organism (Parsons 1961, p. 38). Paralleling this hierarchy of cybernetic control among these systems is the hierarchy of control of functions *within* the social system. The hierarchy of functions from highest to lowest in accordance with their relevance to the problems confronted by the system is: pattern maintenance, integration, goal attainment, and adaptation (Parsons 1961, p. 38). It is therefore not true that Parsons's model consists of an unqualified interdependence without causal

¹³ It should be noted, however, that Gouldner qualifies his observation that Parsons's theory fails to state causes, for shared moral values are central (p. 230).

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priority, for these are parallel hierarchies which rank the controlling elements. Nor is this ranking peripheral to the theory, for Parsons advances it as the third of three "essential axes of theoretical analysis," the others being the structural-functional and the dynamic axes. The theory, therefore, *does* yield clues to the levers of change, namely, the values and norms of the pattern-maintenance function. In addition, Parsons takes the position that a multideterminism is compatible with planned change (1960, pp. 156-57).

There is little doubt that causal determinism is built into Parsons's theory. The voluntaristic theory was created as a synthesis of the causal-functional elements of the positivistic theory of action, on the one hand, and the symbolic-meaningful elements of the idealistic theory on the other (Parsons 1949, pp. 81-82, 486, 698-719). The causal elements from positivism are the conditions of action and the technically adequate means to attain the end of the action. If one knows the conditions, means, ends, and norms, he can understand the action and predict its course. In this sense the action is determined.

Insofar as Gouldner's case for a conservative infrastructure underlying Parsonsian structural-functionalism rests on the pillars of antideterminism and system interdependence with causal priority, the preceding arguments undermine his case. There is little evidence that the voluntaristic and systems theories were developed as an anti-Marxist, pro-capitalist attempt, and Gouldner's evidence cannot withstand criticism.

THE SACRIFICE OF INDIVIDUAL AUTONOMY TO THE SOCIAL ORDER

Gouldner continues his analysis of Parsonsian theory by observing that the latter creates an image of society as a unity by relating every part of the system to the whole (pp. 211-12). With a focus on the whole rather than the parts, Gouldner interprets Parsons as saying that individuals pursue ends originating not within themselves but within the system. Men are hollowed-out shells into which the substance of society is poured, with a consequent reduction of friction between the individual and society. Society is a delicately balanced, self-regulating mechanism. Adaptive mechanisms that maintain its boundaries come into play spontaneously and automatically, seemingly apart from the intentions of individuals (pp. 346, 231-32). As problems arise, adjustive mechanisms restore equilibrium spontaneously. One such mechanism is socialization, which "fills" the individual with the experiences of the social system, Gouldner tells us Parsons says (pp. 218-19). Through the socialization process the individual is carefully trained to exercise restraint over his desires for material gratifications because the system's stability depends upon the curbing of appetites by moral values (pp. 237-41, 430-32). Material gratifications are replaced

by the gratification of conformity to moral values. The latter are grounded in religion, which plays a central role in Parsons's theory.¹⁴

This description of system interdependence implies a deterministic cast to Parsons's theory, which contradicts Gouldner's previous attempt to reveal an antideterminism within it. Apart from the inconsistency, however, his interpretation of the relationship between the individual and the social system does violence to Parsons's formulations.

In *The Social System*, Parsons distinguishes three systemic foci of any system of action. Three systems, the social, personality, and cultural, are always interacting. He describes the interrelationships among the three as interdependence, interpenetration, and irreducibility. This means that each system affects the way in which the other two function; all three share some elements in common; yet no one system can be fully understood simply from a knowledge of the other two. The mutual irreducibility of the systems in their relations to one another most strongly belies Gouldner's analysis, for it accords a sphere of autonomy to each of the three. It follows that an individual's actions are influenced, but not totally determined, by the social system, despite the fact that Parsons has not developed the systematics of personality as fully as one might like. Thus, he has avoided the pitfalls of a cultural, a social, and a psychological determinism (Parsons 1951, p. 6).

In addition to the partial autonomy of each system, each must meet its own functional imperatives. The personality, for example, optimizes gratification and utilizes the mechanisms of allocation and integration to achieve gratification (Parsons and Shils 1951, pp. 120-23). It would be a mistake to equate these functional prerequisites, states Parsons, with those of the social system (1951, p. 18). To be sure, mechanisms of socialization are accorded an important place in Parsons's theory, but it is also true that persons are by no means uniform products, for the socializing processes are quite diverse.

In his attempt to reinterpret Parsons's view of the social system as a whole and therefore to conclude that personalities lack autonomy, Gouldner has failed to take account of the total theory of action. By calling attention to only one of action's foci, namely, the social system, he has created a misleading impression of a purely social determinism.

Gouldner emphasizes, in addition to the holism of Parsonsian theory, the centrality of moral values within it. As he interprets Parsons, compliance with morality guarantees order and supplies the conformist with gratification (pp. 249-50, 428-29). Individuals are taught to conform and to enjoy their conforming acts. The theory stresses morality so much that it

¹⁴ Here Gouldner again detects a continuity in social theory. The preoccupation with religion appeared in Comte's and Saint Simon's positivism and in Durkheim's and Weber's classical sociology.

neglects the gratifications from material things and the technology that produces the latter, Gouldner says. Durkheim's view that values *limit* material desires prevails in Parsons. Moreover, Parsons is said to be generally unaware that action occurs within contexts of scarcity. "Parsons' Ego and Alter do not seem to live in a world of scarcity; scarcity seems to have no effect on their behavior or on their relationship" (p. 237). In short, Parsons's theory supposedly lacks concern with men's material desires—a lack consistent with its subordination of the individual to the system.

Once again a closer scrutiny of the theory urges qualification of Gouldner's interpretation. Although there is little doubt of the central contribution of values to order, Parsons continually calls attention to the costs of conformity to some values. An outstanding example is the universalistic achievement value of the occupational sector. Also difficult to adhere to is affective neutrality, which requires an actor to renounce the immediate pleasure that might be forthcoming from his situation (Parsons 1951, pp. 267–68, 225–26). The costs that some values inflict on the individual are a source of deviance. Moreover, Parsons is skeptical about how any degree of conformity to a value can by itself yield gratification: "There is a sense in which as we have seen, all normative patterning involves an element of affective neutrality, in that as was noted, conformity with a normative pattern cannot in itself be a source of direct and immediate gratification" (1951, p. 267).

Furthermore, his theory does allow room for strictly material gratifications. There is a tendency, he says, for the personality to optimize its gratifications and minimize deprivations, and undoubtedly some needs are genetic (1951, pp. 7, 9, 27). It is obvious that genetically determined needs are satisfied not by moral conformity but by material objects and processes. The theory also takes account of technology. In outlining the principal characteristics of industrial societies, Parsons delineates the sociological counterparts to the economic factors of production. The parallels to land are technological knowledge and commitment to cultural values (1960, p. 134), and in his discussion of the factors of production, Parsons considers technology as equal in importance to such values as achievement.

Finally, the charge that Parsons's actors interact in a situation where there is no scarcity overlooks his well-known critique of the "Hobbesian problem of order" in *The Structure of Social Action*. According to Hobbes, men in a state of nature utilize their reason in the service of their passions. Because men's passions are basically similar, they want the same things. But because these things are scarce, men inevitably conflict. Since each has the right of self-preservation, each is justified in using force and fraud to attain his ends against others, and the outcome is incessant warfare (Parsons 1949, pp. 89–90). Parsons was dissatisfied with Hobbes's

solution to scarcity-produced warfare, namely, a powerful government enforcing peace by its superior power.

THE POVERTY OF THE PARSONSIAN THEORY OF SOCIAL CHANGE

One of Gouldner's major theses is that system wholeness, the individual's lack of autonomy, the system's self-regulation, and the centrality of values preclude an adequate explanation of change within a structural-functional framework. Gouldner quotes Parsons himself as deeply pessimistic over the prospect of an adequate theory of change. His pessimism, says Gouldner, stems from the substance and structure of his theory, which distinguishes mechanisms that maintain equilibrium. Conformity to values works towards stability; factors such as conflict that upset stability are considered to be not quite as "real" as the equilibrating mechanisms. Although the theory does take conflicts, tensions, and strains into account, Gouldner goes on to say, they are relegated to the status of "fortuitous illnesses" rather than necessary conditions. The system is endowed with immortality (p. 354).

This conclusion that the theory accords greater reality to stability than to conflict is incorrect. Parsons's formulation of "the institutional integration of action elements," which depicts absolute harmony among interacting individuals and a mutually gratifying conformity to the normative order, is an ideal type from which empirical departures are the rule. The continuation of stability is a theoretical assumption, not an empirical generalization, and harmony is only a point of reference for the analysis of empirical events. Furthermore, in existing systems the normative patterns need *not* be maintained; their boundaries may dissolve into their environments or their patterns be transmuted into others (Parsons 1951, pp. 43, 481, 482). The equilibrium of normative patterns can also be a moving one. It follows that theory must provide explanations of change as well as stability.

Gouldner is correct when he reports Parsons as pessimistic about the prospects for a general, well-integrated theory of change. But this pessimism is traceable not to his preoccupation with an ideologically dictated stability, but rather to his recognition that no theory yields a set of laws describing the system's operations. Laws connecting the values of one variable with those of other variables of the system is the aim of theory. With such knowledge, the theorist could predict one change from another. In the absence of such laws, the closest approximation is a theory that outlines the structural categories of a system. Parsons's pessimism, therefore, stems from the absence of a knowledge of laws of process.¹⁵

¹⁵ In documenting Parsons's pessimism Gouldner refers to the following quotation

Another criticism Gouldner levels at Parsons's discussion of change is his failure to accord technology the place it truly deserves. Once again Gouldner comes down hard on Parsons's work for its neglect of material gratifications in favor of moral rewards. In an article about evolutionary universals in human history, Gouldner says, Parsons lists cultural legitimation, money, and democratic associations but omits science and technology. The latter are relegated to the status of "prerequisites" of evolution rather than accorded the rank of evolutionary universals. Gouldner attributes this glaring omission to Parsons's objective of proving the superiority of America over the Soviet bloc of nations. The United States institutionalizes some evolutionary universals such as money and markets, universalistic legal codes, and democratic associations, which are not fully developed within totalitarian systems. On the other hand, totalitarian societies do possess science and technology and in these areas would compare favorably with the United States. By choosing the first factors as evolutionary universals, Parsons can demonstrate the greater adaptiveness and superiority of the United States over these other systems (Gouldner 1970, pp. 366-67). Thus, Gouldner states, the pro-capitalist, anti-Marxist infrastructure of the voluntaristic theory of the early Parsons continues to underlie his recent work.

This conjecture by Gouldner that Parsons is here attempting to demonstrate American superiority is simply out of place. The following quotation proves that Parsons *does* include technology as a universal: "These four features of even the simplest system—"religion," communication with language, social organization through kinship, and technology—may be regarded as an integrated set of evolutionary universals at even the earliest human level. No known human society has existed without *all* four in relatively definite relations to each other" (1964, p. 342).

In sum, a look at various aspects of Parsons's functional theory casts a different perspective on its orientation to change than the one Gouldner sets forth. Systemic interdependence and equilibrating mechanisms do not preclude processes of change. The theory is not dualistic in its treatment of stability and change. Material factors and technology are factors of importance within its framework.

from *The Social System*: "We do not have a complete theory of the processes of change in social systems, *we do have a canon of approach to the problems of constructing such a theory*. When such a theory is available the millennium for social science will have arrived. This will not come in our time and most probably never. *But progress toward it is much more likely to be assured and rapid if we know what we want and need*" (1951, pp. 534-35; italics mine). However, Gouldner (p. 354) conveniently omits the italicized parts in this quote, which soften Parsons's pessimism about the prospects of a theory of change.

IS THERE A CRISIS?

The preceding account of social theory and its deficiencies lays the foundation for Gouldner's principal conclusion that academic sociology is now in the early stages of a crisis.¹⁶ By crisis he means a rapid change with much upheaval occasioned by the fundamental character of the change. "A crisis implies that taxing changes are proceeding at a relatively rapid rate; that they entail relatively sharp conflict, great tensions, and heightened costs for the system undergoing them; and, finally, . . . the system may soon find itself in a significantly different state than it had recently been" (pp. 341-42). What is causing this crisis?

The most important source of this crisis is the inability of functional sociology in its current state of theoretical development to fulfill the demands made upon it by the welfare state.¹⁷ The welfare state demands solutions to current social problems. Hence, academic sociology is caught in a contradiction between its structure and infrastructure, on the one hand, and the expectations laid upon it, on the other. The proofs Gouldner offers for this conclusion are found in his discussions of social theory's historical development. Since the acceptance or rejection of the idea of the imminence of a crisis depends upon his interpretations of the development of social theory, a brief recapitulation of these is in order. Reduced to a few statements, Gouldner's principal points are as follows:

First, the sociological positivists and the functionalists created theories of society as a system with spontaneous rather than planned regulatory mechanisms. These spontaneous mechanisms automatically alleviate social problems and restore order, so planned intervention is superfluous. Second, positivist, classical, and functional theories attribute little importance to the state. This tendency was presumably related to the value-neutrality posture, itself an outcome of the political failure to reorganize society in the early periods. Third, functionalists fail to weight the variables of the social system, making it impossible to exert leverage to change society in planned directions. Fourth, functionalists, with their emphasis on moral values, falsely attribute problems to the collapse of morality and suggest

¹⁶ Actually Western sociology which includes Marxism as well as academic sociology, is in crisis. My critique omits Gouldner's analysis of Marxism, which commands but a fraction of his attention.

¹⁷ There is another cluster of conditions contributing to the crisis, namely, the sentiments of the New Left, which are not resonated by functional sociology. The New Left has not yet formulated an opposing theory, but Gouldner contends that young sociologists are not accepting Parsonsian structural-functionalism (pp. 5, 399-402). I shall not comment extensively on these aspects of the crisis, mainly because their contribution to the crisis does not grow out of Gouldner's reconstruction of the history of social theory. Furthermore, there is no way of knowing what proportion of young sociologists will not accept functionalism in the future.

the inadequate solution of education rather than the mobilization of populations. Fifth, this same emphasis causes sociologists to neglect the redistribution of material gratifications and the contribution of technology to solving social problems. Indeed, Durkheim positively distrusted such gratifications. Sixth, the voluntaristic theory extols individual striving rather than social planning. Finally, the functional theory of change, which depicts problems as "aberrations," is inadequate to suggest needed changes (pp. 352-68).¹⁸

My critique of Gouldner's reconstruction is at obvious variance with these statements. At the risk of being repetitive, let me briefly restate some opposing contentions. Both Saint-Simon and Comte, far from being content with society's spontaneous adjustive mechanisms, devoted their lives to achieving a conscious, deliberate social reconstruction. The action theory of Parsons does not describe men as hollowed-out vessels for social forces, but rather as personality systems with their own partially autonomous system requirements. Functional theory does not preclude technology and material gratifications, substituting moral gratifications for them. The voluntaristic theory is not an antideterministic theory that urges men to strive but not to expect to achieve, but rather one that offers the possibility of a rational analysis of the means-ends relationship. System interdependence is not inconsistent with causal priorities. Functional theory does not depict conflicts and changes as unreal; the works of both Durkheim and Parsons show an authentic interest in social change.

Most of these contentions are adequately documented in the preceding commentary on Gouldner's reconstruction. Insofar as his predictions about a forthcoming crisis, now in its beginning stages, rest upon questionable interpretations and explanations of the history of social theory, it is prudent to assume a skeptical stance with respect to such predictions. He has not proved that contemporary social theory is deficient to meet the welfare state's demands; contrary to his view, it does allow for such factors as technology, autonomy, and change. It is also not obvious that the welfare state, if indeed it is the wave of the future, will be dependent on academic sociology. Neither has he marshalled sufficient evidence to substantiate the existence of a crisis. He can also be criticized for not spelling out more detailed criteria of crises.

The significance of Gouldner's study, however, transcends the problem of whether there is a crisis in sociology, for it focuses our attention on the approach that has come to be identified as the sociology of knowledge.

¹⁸ Gouldner notes that the later Parsons *has* accepted the principle of governmental intervention, and Gouldner even detects a convergence of Parsons's theory with Marxism. Nevertheless, academic sociology's main thrust is still out of harmony with the welfare state.

His concepts of domain assumption and infrastructure are ingenious ones that link the substance and form of theories with events and structures in the sociohistorical contexts of the theorist and his readers. They mediate the impact of life situations on thinking, even scientific thinking, and thereby help to clarify and account for the products of thinking. The concept of infrastructure generates Gouldner's basic hypothesis that social theories rest on extrascientific as well as purely scientific notions; for anyone to understand a social theory fully, he must therefore take extrascientific considerations into account (Gouldner 1970, pp. 29-35).¹⁹

To those sympathetic to the sociology of knowledge, these notions are appealing, for they are consistent with this approach and plausible. Nevertheless, a problem arises, as it does in Gouldner's study, when the notion of a theory's infrastructure is built into specific hypotheses. For if a theorist's domain assumptions are not known to him insofar as they are outcomes of his prescientific thinking, the task of verifying their existence in his theory becomes empirically difficult. What is the empirical status of the domain assumptions that a sociologist of knowledge detects in a social theory if the theorist himself denies their existence as part of his thinking? The next link in the sequences of scientific analysis is even more dubious. The analyst must now draw a causal connection between the domain assumptions of a theory's infrastructure and some condition within the theorist's sociohistorical context that presumably stimulated those assumptions. Usually the number of plausible events within the theorist's context is so legion that the selection of those presumed to bear a causal relationship to his theory is vulnerable to the threat of arbitrariness.

These general questions concerning the sociology of knowledge are not answered here, but they must be faced when any theorist undertakes the exacting task of bringing this perspective to bear on specific items of knowledge. Formulating the sociology of knowledge in abstract terms is one thing, but utilizing propositions derived from it as explanations of identifiable thought products is another. Apart from Gouldner's ambitious attempt to reconstruct the evolution of social theory, another merit of his work is his attempt to relate the sociology of knowledge to sociology.

¹⁹ Gouldner makes it clear, however, that domain assumptions are neither necessarily required in theory construction nor inevitable, but that sociologists have not been able to escape them. Neither does Gouldner deny that a theory should be judged according to its cognitive validity. He states: "The ideological implications and social consequences of an intellectual system do not determine its validity, for theory does indeed have a measure of autonomy" (p. 13). While recognizing the need to judge a theory with respect to its cognitive validity, Gouldner directs his principal efforts in this work toward understanding the infrastructure of social theories.

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Political Judgments and the Perception of Social Relationships: An Analysis of Some Applied Social Research in Late 19th-Century Germany¹

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INTRODUCTION

This paper is about two interrelated questions in the history of sociology and of social thought, and about a more general issue concerning the nature of certain concepts which sociologists of knowledge and intellectual historians often use.

First, how could the perception of social relationships, a prerequisite to the development of sociology, arise in market capitalist societies among people who were neither classical conservatives nor Marxists? There is a strong tendency in capitalist societies, and in the liberal ideologies which have been associated historically with their formation and growth (as compared with other societies and other ideologies), to see social systems as aggregates of individual persons instead of seeing systems of social relationships.² For example, in the vocabulary of feudal hierarchies, every word—such as “lord,” “liege,” “vassal,” “villein,” and “serf”—denotes a position in a system of social relationships. But in the vocabulary of industrial hierarchies—with terms such as “worker,” “foreman,” “superintendent,” “chairman of the board,” “industrialist,” and “manufacturer”—some denote positions in social systems while others do not.³ It is

¹ This paper is a revision of a paper presented at the annual meeting of the American Sociological Association, Denver, September 1, 1971. I am grateful to many colleagues, including John Brewer, Jeffrey Butler, William Martin, J. Ronald Milavsky, Louis Mink, Hubert O’Gorman, Ellen Kay Trimberger, and Harrison White for comments on earlier drafts. I regret that I have not been able to use many of the valuable suggestions, or to meet all of the objections, which these readers have offered.

² This dichotomy, perception of aggregates versus perception of social relationships, is, of course, somewhat forced. Perhaps there is a continuum, with the perception of social categories somewhere in the middle, between aggregates on one side and social structures on the other. What is more, the identification in this paper of the perception of social relationship with the central subject matter of sociology is discussable, at least, if not arguable, although it seems that no classical theorist in sociology limited his thought to social categories and that all (in various ways) dealt with social relationships. Though further refinements in these points seem desirable, this paper does not deal with them.

³ This point, as concerns the vocabulary of industrial hierarchies, was suggested by

possible to think of a worker as a person who works, independently of his relationship to a foreman. It is not possible to think of a vassal independently of his relationship to a lord.

Or compare the estate (*staendisch*) images of society in feudal thought—"There be in this world thre maner of men, clerkes, knythis, and comonyalte" (in the words of an English medieval sermon)⁴—with the individualist, aggregative images in liberal doctrine and in liberal social science. In Adam Smith's hypothetical tribe, for example, one person "makes bows and arrows . . . with more readiness and dexterity than any other." That person, because there is a "propensity in human nature . . . to truck, barter, and exchange one thing for another," and for certain other reasons inherent in human nature, exchanges bows and arrows for cattle or venison. He finds that he obtains more cattle and venison in this way "than if he himself went to the field to catch them." Hence, "from a regard to his own interest . . . the making of bows and arrows grows to be his chief business." That is, social structures—in this case, the division of labor in a system of market transactions—emerge out of the bumping together of individual persons, each of whom is pursuing individual goals and acting upon individual motivations or "propensities." The basic building blocks of a society are its individual members, not clerics, knights, and commoners, or proletariat and bourgeoisie, or even families. They are individual persons (Smith 1910, bk. 1, chap. 1, pp. 12–14).⁵

Now, the development of sociology in the 19th century depended upon (among other things) the existence of people who could resist, or who did

various passages in Gouldner (1955). For example (p. 24): "The workers defined their main role obligation to be that of 'working' or 'producing.' Their 'obedience' obligations, however were not equally stressed. . . . [and] . . . workers would have been amazed if, instead of being awakened in the morning with the call, 'Time to go to work, dear,' their wives called out, 'Get up, dear, it's time to go to *obey*.'"

⁴ Quoted in Thrupp (1962, p. 288). As quoted here, the spelling has been slightly simplified and modernized.

⁵ For another example see Hobhouse (1964, pp. 67–68). He espoused what he called (p. 67) "the organic view of society." But the units in the organism are individual persons, not hereditary estates, social classes, families, or social groupings of any sort. The organic view "means that, while the life of society is nothing but the life of individuals as they act one upon another, the life of the individual in turn would be something utterly different if he could be separated from society." Or again (p. 68), "Society consists wholly of persons. . . . The British nation is a unity with a life of its own. But the unity is constituted by certain ties which bind all British subjects, which ties are in the last resort feelings and ideas, sentiments of patriotism, of kinship, a common pride, and a thousand more subtle sentiments that bind together men who speak a common language, have behind them a common history, and understand one another as they can understand no one else." This book was first published in 1911 and illustrates both the transition from *laissez faire* liberalism to social welfare liberalism and the absence, in this transition, of any fundamental change in the liberal, aggregative images of society, in which the basic units are individual persons who have subjective "propensities," or motives, or (in Hobhouse) feelings, ideas, and sentiments.

not share, the strong individualist bent—individualist in cognitive orientation—of classical liberalism, and of market capitalism. Such people included, of course, both Marxists and classical conservatives.⁶ But, with a few exceptions, most sociologists during the second half of the 19th century (and ever since) were neither Marxists nor classical conservatives. They were centrist reformists of one or another type.⁷ How did such people come to see—to the extent they did—social relationships instead of aggregates of individual persons?

Content analyses of 102 research reports (using that term loosely) published in Germany during the 1880s and early 1890s by the Verein fuer Sozialpolitik (Association for Social Policy) suggest who such people might have been. Briefly, and subject to qualifications which appear below, the more involved authors—those who made explicit evaluations, who took sides in social conflicts, and who made policy recommendations—and those who looked to the German state to implement their policy recommendations, were relatively more likely to perceive social relationships. The more detached authors, writing on identical topics, for the same audience, and following the same observation protocols, were more likely to see aggregates of people instead.

These findings are consistent with our analysis of another series of

⁶ On the extent to which sociology has roots in European conservative thought of the early 19th century, see Nisbet (1952, pp. 167–75). On one Marxian contribution to the development of sociology, consider this cryptic, suggestive remark by J. D. Y. Peel in *Herbert Spencer: The Evolution of a Sociologist* (1971, p. 58). Peel writes: “Spencer, like Marx, predicted on the basis of current developments, a withering away of the state. This premiss of the radical view of society (which was an absolute prerequisite for the emergence of sociology) was put succinctly by Thomas Paine: ‘Society and government are different in themselves and have different origins. Society is produced by our wants and government by our wickedness. Society is in every state a blessing; government even in its best state a necessary evil.’” (Peel’s quote is from Paine’s *Common Sense*.) On Marx’s distinction between “state” and “civil society,” see Arthur (1970, esp. pp. 5–14); and McGovern (1970, pp. 430–66). It was probably more difficult, and more significant intellectually and ideologically, to arrive at this distinction in the German language than in English. For *Staat*, in German, often means not “state,” in the more narrow English sense, but rather “politically organized society,” as for example in the terms *Staendestaat* and *Klassenstaat*.

⁷ To cite only American examples, see the following. On Cooley, see the various passages (such as pp. 50–52, 199–212) on his political views in Jandy (1942). Jandy sums up these views (p. 201) by stating: “To classify Cooley as an intellectual liberal is to state an evident fact.” On Ross, see chap. 4, entitled “Edward Alsworth Ross: The Natural Man And The Community of Constraint,” in Wilson (1968). See esp. pp. 104 ff. and, on Ross’s support for Theodore Roosevelt and opposition to Bolshevism, pp. 111–12. On Albion W. Small, see, among many other writings, his novel, *Between Eras: From Capitalism to Democracy* (1912). On Giddings, see chaps. 24 and 25, entitled “Progress” and “Democracy,” respectively, of Giddings (1906)—for example, pp. 298–99 on “The Policies of Liberalism” (“World Intercourse,” “Free Thought,” and “Legality”) and pp. 312–34 on “False Notions of Democracy” (democracy is not class rule by the poor or by wage earners but is government of, by, and for all of “the people”).

reports which the Verein sponsored and published, not included in the data reported here. In the early 1890s, Max Weber and five other authors participated in a series of studies of agricultural laborers in the various regions of Germany (*Schriften des Vereins fuer Sozialpolitik* 1892b).⁸ Weber, as much as or more than the five others, made explicit evaluations, took sides in social conflicts, and made policy recommendations. And he had a much stronger perception of social structure than any of the other five authors did.⁹

Such findings, however, pose the second of our two historical questions. Most early sociologists, like the young Max Weber, did not conceive of sociology as a value-free science. Durkheim, for example, distinguished clearly between studying moral phenomena to understand them and studying them in order to make moral judgments (1926).¹⁰ But he went on to assert not only that knowledge must precede judgment but that knowledge is the basis for judgment. "The science of moral opinion provides us with the means for judging moral opinion and, if need be, for correcting it" (1951, p. 86).¹¹ And Albion Small argued at length not only that ethics presupposes sociology and that a generally accepted sociology is a precondition of ethical consensus, but also that "the ultimate problem on the side of pure science is: What is worth doing?" (1903, p. 119; emphasis in original omitted). These examples are consistent with the data reported here. Among our authors, evaluative involvement was associated with the perception of social relationships: most sociologists who worked at the same time did not conceive of sociology as a value-free science.

How did it happen, then, that in the 20th century the notion of "value-free science," in a variety of meanings which this paper does not go into, became associated with sociology more than it did with any of the other social sciences, with economics, say, or with history or political science?

⁸ The other five authors were Karl Kaerger, H. Losch, Kuno Frankenstein, Friedrich Grossman, and Otto Auhagen. For a summary of Weber's contribution to these studies, see Bendix (1960, pp. 14-23).

⁹ For a comparison of Weber's work on agricultural laborers with the work of the five other authors, see Dibble (1968).

¹⁰ For example (Durkheim 1926, pp. xxxvii-xxviii), "Ce livre est avant tout un effort pour traiter les faits de la vie morale d'après la méthode des sciences positives. . . . Les faits moraux sont des phénomènes comme les autres; ils consistent en des règles d'action qui se reconnaissent à certains caractères distinctifs: il doit donc être possible de les observer, de les décrire, de les classer et de chercher les lois qui les expliquent. C'est ce que nous allons faire pour certains d'entre eux. . . . Ainsi entendue, cette science n'est en opposition avec aucune espèce de philosophie, car elle se place sur un tout autre terrain."

¹¹ *Détermination du Fait Moral* was first presented to the Société française de philosophie in 1906 and first published in the *Bulletin* of the society. The point made in the quotation is developed and argued at some length (1951, pp. 85-89). Durkheim had asserted the same point years before, in the preface to the first edition of *The Division of Labor* (see 1926, pp. xxxix-xl).

The data reported below suggest what part of the answer might be. Briefly, the association between a "high" tendency to make explicit evaluations and a "high" tendency to perceive social relations holds only among the authors who were not professional officials. Among the officials, a different dynamic prevailed. Officials were more likely than nonofficials to look to the state for the implementation of their policy recommendations; and those who looked to the state were more likely to perceive social relationships. Evaluative involvement, then, made for the perception of social relationships; being in power, or being oriented to power, was the functional equivalent, in this respect, of evaluative involvement.

Third, a more general, nonhistorical point: some of the findings reported here suggest the necessity to be wary of certain rather global concepts—concepts such as "style of thought" or "climate of opinion"—which sociologists of knowledge and intellectual historians sometimes use. They suggest the necessity to break down such concepts into their component parts.

One of the central variables in the analysis presented here is the implicitness or explicitness of the evaluations in the 102 research reports in question—the extent to which the authors are explicitly evaluative, as against the extent to which they make evaluations as if they were making descriptive statements. As seen here, explicitness of evaluations is associated with the perception of social relations: implicit or pseudoneutral evaluations are associated with the perception of aggregates. But these two variables have, in part, different determinants or correlates. They have to be understood on somewhat different terms, suggesting, more generally, that (a) the evaluative style component and (b) the purely cognitive component in concepts such as "style of thought" may refer to quite different phenomena which have different determinants.¹²

METHODS AND SOURCES OF DATA

Turning to the methods of investigation and to the sources of the data presented below, it is necessary to note that the Verein fuer Sozialpolitik was a combination intellectual forum, research organization, and pressure group for social legislation. It was dominated largely by "academic socialists" (*Kathedersozialisten*) who had founded the organization in 1872.¹³ Economists of the historical school who opposed laissez faire, who supported social legislation—in part because they viewed social legislation as a means of reconciling conflicting classes and resolving class struggles—and who

¹² Talcott Parsons makes this same point, not with respect to particular concepts, but with respect to the global concept of "knowledge," as commonly used in the term "sociology of knowledge" (Parsons 1970, p. 282–306).

¹³ For a history of the Verein, see Boese (1939).

supported state action for social welfare were prominent among the founders. In their view, detailed quantitative studies of social conditions would show the need for social reform and catch the real richness of social reality, as against the abstractions of laissez faire theory.¹⁴

Hence, the Verein organized and published numerous studies on such topics as housing conditions of urban workers, sources of credit for peasants, emigration, local government, and the condition of agricultural laborers. They sometimes sponsored survey research, as in the mammoth survey of the conditions of agricultural workers—based on questionnaires sent to landlords all over Germany—in which Weber participated. More commonly, however, the Verein used informants, people who were presumably knowledgeable about their own localities.

For a given project on a given topic, the Verein sent questionnaires, or observation protocols, to clergymen, landowners, professional bureaucrats in the state administration, private officials such as officers of peasant associations, and local notables of various sorts. These people wrote about the housing conditions, or the state of the peasantry, or whatever, in their own localities, following (more or less) the observation protocols which the Verein had provided. These reports were then published in the proceedings (*Schriften*) of the Verein. Hence, it is possible to compare the ways in which different people, writing for the same audience and following the same observation protocol or list of questions, wrote about the same topic.

The data of this paper are from five projects—on “Peasant Conditions,” “Usury in the Countryside,” “The Housing Crisis,” “Local Government,” and “Emigration,” which included a total of 102 reports by almost as many different authors (*Schriften des Vereins fuer Sozialpolitik* 1883, 1886a, 1886b, 1890, 1892a). These five were chosen after other projects were excluded for various reasons. Given the purposes of the study, it was not possible to use projects in which there was only one author or only a small number of authors, or projects in which all or most of the participants were of a single social type—all professional bureaucrats or all professors. And it was not possible to use projects in which the authors could not follow the observation protocols without making value judgments and policy recommendations. The purpose of the study was to compare different social types in a number of respects, including their tendencies to make unsolicited evaluations and unsolicited policy recommendations. I also excluded the occasional non-German authors, writing about conditions outside Germany, and all studies published after 1920. However, the protocols for one of these five, the one on local government, did end with a request for policy recommendation. Therefore, when that variable, making or not

¹⁴ On the German historical school of economics in the 19th century, see Eisermann (1956).

making policy recommendations, appears below, the fourteen reports in this series are excluded, and the number of cases drops from 102 to 88.

I coded the 102 reports in the five projects for a number of variables, including the seven variables which are used in this paper.¹⁵ It is necessary to explain in some detail the coding of three of these variables: (1) the tendency to perceive social relationships, (2) the tendency to make evaluations, and (3) the making of explicit as against implicit evaluations.

I read each report as a series of topics. Most of the topics were defined by questions in the observation protocols, and the majority of authors in a given series dealt with most of the topics which appeared in any one report in that series. For example, some of the topics in the series on "Peasant Conditions" were "size of land holdings and distribution of ownership" (in all 39 reports), "indebtedness" (in 35 out of 39 reports), "inheritance law and practices" (in 37 out of 39 reports), "sources of credit" (33 out of 39), "trade in land" (31 out of 39), and so forth. For each topic, I asked whether the author addressed himself to social relationships. That is, in connection with a given topic, did the author write as if he were making some observation of individual persons which he could not make in principle, unless he were also observing others with whom the object of his observation interacted. For example, if an author says that large landowners use more advanced agricultural techniques than small landowners, then he is not addressing himself to social relationships. He is writing only of social categories, of occupants of different statuses, and not about the interaction between them. If however, he goes on to say that the small landowners in his district have begun to emulate the large landowners, and that the large owners encouraged this emulation and are thus exerting a wholesome influence on the small landowners, then he is coded as having discussed social relationships in connection with the topic "agricultural techniques." For each topic, there is a score of zero or one. The score for the entire report is the number of topics in connection with

¹⁵ I did all of the coding myself. Hence, there are no checks on intercoder reliability. I devised the coding procedure after first reading a number of reports and ranking them, judgmentally and impressionistically, relative to one another, with respect to three variables for which there are quantitative scores. The impressionistic rankings and the rankings by numerical scores came out the same. The less precise and the more precise procedures gave the same results. I, of course, did not look at the name or titles of any author until I had completed the coding of his report. But this check was not always effective, since the authors sometimes identified themselves by the nature of their work, for example, in the text of their reports. In my view, perhaps the most solid basis for believing that the coding did not consist primarily of the projection of previously formulated hypotheses onto the raw data is the number of unexpected contingent associations between variables in these data, of which some are reported here. It is easy to distort coding so as to make it conform to simple, two-variable hypotheses: It is more difficult to distort coding so as to make it conform to more complex hypotheses, and such distortion seems most unlikely when, as in this case, the complexities were not anticipated.

which the author mentioned social relations, divided by the total number of topics. Observations of purely legal relations or of purely economic transactions were not coded as being observations of social relationships. If an author speaks simply of buyer and seller or of debtor and creditor, he was not coded as having looked at social relations. But if he goes on to say that the relationship is marked by friendship, trust, and mutual aid, or by hostility and suspicion, between debtor and creditor, then he was classified as having looked at social relationships.

Similarly, for each topic, I asked if an author made evaluations. Now, when an author makes an evaluation as if he were making a descriptive statement, as if he were describing some attribute of objective reality, and if he does so without stating any criteria of evaluation, then he was classified as having made an implicit evaluation. For example, when an author writes that the distribution of landownership in his district is "satisfactory," or when he says that existing sources of credit are "insufficient," or when he writes that the labor supply is "adequate," then he is making an implicit evaluation. Such statements purport to be objective descriptions of phenomena but are made on the basis of unstated criteria of evaluation.

In contrast, (a) when an author erects explicit criteria of evaluation, or (b) when he uses evaluative words with denotations or connotations which are so clearly subjective that he is not purporting to describe objective phenomena, or (c) when he uses evaluative words which are strongly emotive, then he is regarded as making an explicit evaluation. Some examples are: (a) a prosperous middle-class peasantry is the strongest support for throne and altar, which all right-thinking people want to defend, and the existing distribution of landownership is clearly inadequate since it prevents the growth of such a class; (b) the lot of the slum dwellers is a sorrowful spectacle, and the author personally approves of measures to improve their lot; or (c)—a very infrequent type—usurers are blood-suckers and are cancers on the body politic.

Numerical scores for an entire article on the variable, tendency to make evaluations (of either type), were calculated as they were for the variable tendency to perceive social relations. For the third variable, tendency to make explicit evaluations, a somewhat different procedure was used. Instead of the ratio of all topics explicitly evaluated to all topics in the article, we used the ratio of all topics explicitly evaluated to all topics evaluated in one way or the other. This measure is, in effect, a measure of the propensities of the various authors to be explicit or implicit in their evaluations, controlling for their propensities to be evaluative in the first place.

The raw scores on these three variables had quite different distributions in different projects. For example, the overall tendency to perceive social

relations in the reports on workers' housing was weaker, quite naturally, than the overall perception of social relations in the reports on usury in the countryside. Hence, it was necessary to standardize the scores across projects. I used two different procedures. I classified raw scores as "high," "medium," or "low" relative to other raw scores in the same project by trichotomizing the range of scores (resulting in highly skewed distributions of standardized scores), and by trichotomizing the distribution of raw scores, such that the number of reports falling into any one of the standardized categories was equal to or almost equal to the number of reports falling into each of the other two. In the data presented below, standardized scores are based on trichotomizations of ranges, but the same substantive results appear when the other procedure is used.

The other four variables which appear in this paper do not require such extensive discussion. They are: (4) whether an author is a professional official,¹⁶ with information from a wide variety of biographical sources;¹⁷ (5) whether an author expressly takes sides with some social class or other social grouping, and against some other class or grouping—workers against landlords, say, or peasants against merchants; (6) whether an author makes policy recommendations; and (7) if an author does make policy recommendations, whether he looks to private means (such as coöperatives, for example) or to state action, or to a combination of private means and state action, for the implementation of his recommendations.

These, then, are the seven variables which appear in this paper—perceiving social relationships; making evaluations; making explicit evaluations, official or nonofficial; taking sides in social conflict; making or not making policy recommendations; and calling for private or for state action to implement recommendations.

The sections which follow deal, first, with certain differences between the professional state officials and the other authors; second, with the correlates of explicitness of evaluations; and, third, with the correlates of the perception of social relations. On this third point, however, it will be necessary to go simultaneously into a fourth matter—that is, into some of the ways in which the differences between officials and others, the correlates of explicitness, and the correlates of the perception of social relations all tie together.

¹⁶ Some of their titles; either at the time they wrote reports for the Verein's projects or at some other point in their careers, were *Regierungsassessor*, *Regierungsrat*, *Oberregierungsrat*, *Landrat*, and *Ministerialrat*.

¹⁷ A few of the more than 50 sources consulted were *Allgemeine deutsche Biographie*, *Badische Biographien*, *Biographisches Jahrbuch und deutscher Nekrolog*, *Familien-geschichtliche Bibliographie*, *Parlamentarisches Handbuch fuer den deutschen Reichstag und den preussischen Landtag*, *Deutscher Gelehrtenkalender*, *Wer ist's*, *Wuerttembergischer Nekrolog*, and *Das akademische Deutschland*. I am grateful to Ruth Heydebrandt for taking on the onerous job of tracking down the biographical information.

TABLE 1
OFFICIALS TENDED TO MAKE LESS EXPLICIT EVALUATIONS
THAN NONOFFICIALS

| Explicitness of Evaluations | Officials | Nonofficials |
|--------------------------------|--------------|--------------|
| High | 12% | 25% |
| Medium | 35% | 34% |
| Low | 53% | 41% |
| Total | 100% (34) | 100% (68) |

SOME DIFFERENCES BETWEEN OFFICIALS AND OTHERS

Turning first to some of the differences between officials and others, the professional officials were somewhat less likely than the other authors to be explicit in their evaluations. As seen in table 1, they were somewhat more likely than the nonofficials to make evaluations as if they were making descriptive statements, to use words such as "favorable" or "unfavorable," without specifying favorable for what. Professors, clergymen, attorneys, or landowners were somewhat more likely to regard the same phenomena in the light of their own feelings, or in terms of explicit criteria of evaluation.

This difference between bureaucrats and the other authors is, of course, consistent with our received images of what bureaucrats are like. Max Weber decried the bureaucratization of the political system of Bismarckian Germany, and the absence of genuine parliamentary government, because (among other grounds) bureaucrats, in his view, are not trained to set political goals as parliamentary politicians are. Bureaucrats know how to pursue political goals which others establish but not how to define them (1958, pp. 294-431). If professional officials tend to take goals for granted and to regard them as unproblematic, then they should be less likely than the nonofficials to make value criteria explicit, and more likely to use a pseudo-neutral language. Or perhaps the bureaucrats tend (slightly) to be more implicit in their evaluations than the nonbureaucrats for reasons which have nothing to do with bureaucracy. Perhaps we have here a difference between people who are in and people who are out of power. People who are in power can assimilate more things in a cognitive orientation, while people who are out of power may be more constrained to make explicit value challenges.

As seen in table 2, the professional officials were also somewhat more likely than the other authors to look to the state, instead of looking to private means such as cooperatives or self-help movements, for the im-

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TABLE 2

OFFICIALS WERE NEITHER MORE LIKELY NOR LESS LIKELY THAN
NONOFFICIALS TO MAKE POLICY RECOMMENDATIONS, AND WERE
MORE LIKELY THAN OTHERS TO LOOK TO THE STATE*

| Means Chosen to Implement Policy Recommendations | Officials | Nonofficials |
|---|--------------|--------------|
| State action | 30% | 12% |
| Mixed state and private action | 10% | 21% |
| Private action | 35% | 43% |
| No policy recommendations | 25% | 25% |
| Total | 100% (20) | 101% (68) |

* Excluding the series on local government, in which policy recommendations were solicited.

plementation of their policy recommendations. In this respect, also, the results seem consistent with the received images of bureaucrats.

In two other respects, however, the results are not consistent with the received images. According to data not reported here, the percentage of officials who took sides in social conflicts did not differ from the percentage among the other authors. If anything, the officials were slightly more likely to take sides. And as seen in table 2, the officials were neither more nor less likely than the other authors to make unsolicited policy recommendations. Such results seem inconsistent with the image of bureaucrats who go about their assigned tasks, as defined by higher authority, impersonally and without fear or favor.

These results may be an artifact of our "sample." Perhaps officials who felt strongly about the topics under investigation were more likely to agree to participate in the Verein's projects, while no such selective factor operated among the clergymen, landowners, and other nonofficials. But the entire history of officialdom in Bismarckian Germany, not simply our findings on 102 research reports, was inconsistent with our received images of bureaucracy.

To a rather large extent, the state bureaucracy in Bismarckian Germany (including the military) was an institutional base for *Junker* power, to which the *Junkers* turned after capitalist economic development had severely weakened their traditional economic base in agriculture. For their former positions in the social relations of production, distribution and exchange, they substituted positions in the institutions of political administration and rule. The civil service was not, and did not claim to be, politically neutral. For example, professional civil servants held elected office under party labels. And political tests insured their political reliability. The

oath of Prussian civil servants included the provision that they represent the interests of the government at the polls. And, beginning in the 1880s, only those officials who were also reserve officers were eligible for the highest offices in the civilian administration.¹⁸

This was the kind of civil service which Max Weber knew and which he deplored in some of his political writings. But in his more scholarly writings he presented a very different image—just as he decried the German bourgeoisie's aping of the aristocracy (Bendix 1960, pp. 36–48)—and then presented a very different, ideal image of the bourgeois in *The Protestant Ethic*. And it is this very different image of bureaucracy, in Weber's scholarly writings, which we American sociologists have inherited. But Weber knew that this image was not historically real.

Given this kind of civil service, then, it seems quite possible that the absence of any difference between bureaucrats and other authors with respect to taking sides in social conflicts and making policy recommendations is not an artifact of our "sample," and that similar results would obtain if it were possible to use more representative sampling procedures. State officials, in short, were somewhat less explicit in their evaluations than nonbureaucrats, were neither more nor less likely to make policy recommendations, were more likely to look to the state for remedial action, and were, if anything, a bit more likely to take sides in social conflicts.

SOME CORRELATES OF EXPLICIT EVALUATIONS

Turning to some other correlates of explicitness of evaluations, taking sides in social conflicts is associated with explicit evaluations—as seems to make sense, intuitively. Those who commit themselves to peasants against merchants, say, or to urban workers against landlords, are less likely to use a pseudo-neutral language and more likely to make explicit evaluations. And as seen in table 3, taking sides and being a nonofficial are associated additively with explicitness. Further, taking sides is associated with making policy recommendations; and these two variables, in turn, are associated additively with explicitness.

As seen in table 4, explicit evaluations, in turn, go along with a relatively greater tendency to perceive social relations instead of aggregates of persons. In fact, according to data not reported here, the greater the tendency to make evaluations of any kind, explicit or implicit, the greater the tendency to perceive social relationships. In contrast with the later asso-

¹⁸ To state that the military and the civilian administration were a substitute base for *Junker* power is only part of the story. For a view which emphasizes the extent to which aristocrats in and out of the bureaucracy coalesced with nonaristocrats in the bureaucracy, in agriculture, and in commerce and industry to form a new upper class, see Gillis (1971, esp. chap. 9, "Bureaucracy and Society"). This paragraph is based also on the following sources: Dibble (1971); Kehr (1929, pp. 253–72); and Muncy (1944).

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TABLE 3

BEING A NONOFFICIAL AND TAKING SIDES IN SOCIAL CONFLICTS ARE ADDITIVELY
RELATED TO EXPLICITNESS OF EVALUATIONS

| Explicitness of Evaluations | Officials Who Took Sides in Social Conflicts | Officials Who Did Not Take Sides in Social Conflicts | Nonofficials Who Took Sides in Social Conflicts | Nonofficials Who Did Not Take Sides in Social Conflicts |
|--------------------------------|--|---|--|--|
| High | 20% | 5% | 44% | 12% |
| Medium | 47% | 26% | 37% | 32% |
| Low | 33% | 68% | 19% | 56% |
| Total | 100% (15) | 99% (19) | 100% (27) | 100% (41) |

ciation of sociology with the notion of value-free science, among our authors, making evaluations and making them explicit went along with the perception of the central subject matter of sociology—the perception of social relations instead of aggregates.

It is possible to interpret this association in various ways. One possibility is that a greater tendency to make evaluations, and a greater tendency to make explicit evaluations, goes along with a heightened awareness of social structural obstacles to the realization of one's goals. That is, perhaps those authors who were less evaluative or more pseudo-neutral were more likely to think in technical, nonsocial terms. But whatever the explanation, the association between explicit evaluations and the perception of social relations holds up under various controls (not reported here) and is one of the clearest findings in these data.

THE PERCEPTION OF SOCIAL RELATIONSHIPS

Now, if officials were less likely to make explicit evaluations, and if a weaker tendency to make explicit evaluations went along with a weaker

TABLE 4

AUTHORS WHO MADE EXPLICIT EVALUATIONS WERE MORE LIKELY THAN THOSE
WHO MADE IMPLICIT EVALUATIONS TO PERCEIVE SOCIAL RELATIONSHIPS

| PERCEPTION OF SOCIAL RELATIONS | EXPLICITNESS OF EVALUATIONS | | |
|-----------------------------------|-----------------------------|--------------|--------------|
| | High | Medium | Low |
| High | 43% | 9% | 4% |
| Medium | 33% | 40% | 24% |
| Low | 24% | 51% | 72% |
| Total | 100% (21) | 100% (35) | 100% (46) |

tendency to perceive social relations, then it ought to follow, logically, that officials were less likely to perceive social relations. But this result does not occur in our data. For there are certain complications in the data which vitiate such a simple conclusion.

First, officials did not differ from nonofficials in their propensity to make policy recommendations. But they were more likely than the nonofficials to look to the state for implementing their recommendations. And, judging from data based on very small totals, those authors (especially those officials) who looked to the state were more likely to perceive social relations instead of aggregates.¹⁹ An author who counted on the automatic operation of the market, or on technical improvements in the mechanisms of credit for peasant landholders, or in agricultural methods, did not necessarily concern himself with social relationships. He might or might not. But an author who looked to the state did, necessarily and by definition, concern himself with certain kinds of social relationships, with relationships of power and authority. And perhaps this orientation carried over into the way he looked at the housing of the urban working class or at the condition of the peasants down on the farm. That is, a statist orientation is likely to be holistic, and to lead people to ask how everybody fits in with everybody else, into the fabric of society underneath the state. In contrast, perhaps those who looked to cooperatives or to self-help movements were closer to the individualistic, aggregative outlook of classical liberalism.

Whatever the reason, the expectation that officials were relatively less likely to perceive social relations because they were more likely to make implicit or pseudo-neutral evaluations is undercut by their greater tendency to look to the state, which goes along with a greater tendency to perceive social relationships. The expectation for bureaucrats to be less likely to perceive social relations holds only among those authors who did

¹⁹ When we compare authors who made no policy recommendations or who made recommendations but called for implementation by private means with authors who made recommendations and called for a combination of private and state action, or for state action alone, to implement them, 83% of 12 officials who did not look to the state, as against half of the eight officials who did, had a "low" perception of social relationships. None of the 12 officials who did not look to the state and one of the eight who did, had a "high" perception of social relationships. The corresponding percentages among the nonofficials were 61% (of 46) and 50% (of 22) for "low" perception and 11% and 18% for "high" perception of social relationships. The difference is clearer among the officials, despite the very small totals, than among the other authors. The totals are small because it was necessary to exclude from these data the fourteen reports in the series on local government in the rural areas of Prussia. The protocols sent to participants in this series called for policy recommendations concerning agencies and methods of local government. Hence, all authors in this series made recommendations; and, by the very nature of the subject matter, they all looked to the state for implementation. It was therefore not possible to include them in the results on the point in question here.

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not call for any state action. Among this group, officials do seem somewhat less likely than nonofficials to perceive social relations.²⁰

In brief, making explicit evaluations went along with the perception of social relationships. Although officials were somewhat less likely to make explicit evaluations, they were no less likely than the others to perceive social relationships. Regarding the ability to perceive social relationships, their statist orientation was the functional equivalent of evaluative involvement.

This contention is borne out in two ways. First, among those authors who do not look to the state, the officials seem less likely to perceive social relationships, as we would expect, given their tendency to make fewer explicit evaluations than the other authors did. Second, as noted above, the association between explicit evaluations and the perception of social relationships (table 4) remains strong under various controls. But, as seen in table 5, there is one control which considerably alters this association.

TABLE 5

EXPLICIT EVALUATIONS ARE ASSOCIATED WITH A "HIGH" PERCEPTION OF
SOCIAL RELATIONSHIPS ONLY AMONG NONOFFICIALS

| PERCEPTION OF SOCIAL RELATIONS | OFFICIALS: | | NONOFFICIALS: | |
|-----------------------------------|-----------------------------|------|-----------------------------|------|
| | EXPLICITNESS OF EVALUATIONS | | EXPLICITNESS OF EVALUATIONS | |
| | High or Medium | Low | High or Medium | Low |
| High | 6% | 11% | 28% | 0 |
| Medium | 56% | 11% | 30% | 32% |
| Low | 37% | 77% | 42% | 68% |
| Total | 99% | 99% | 100% | 100% |
| | (16) | (18) | (40) | (28) |

Explicitness of evaluation went along with a greater likelihood of a "high" perception of social relationships only among those authors who were not professional officials. Among the officials, the finding does not hold.²¹ Among them, a different dynamic is at work.

If "official" here really means "in power," or "viewing society from the perspective of those in power," and not the particular historical character-

²⁰ Restating some of the data in n. 19 somewhat differently, in order to bring this point out, among authors who did not call for any state action, 83% of the 12 officials and 61% of the 46 nonofficials had a "low" perception of social relationships. Among those who did call for state action, the corresponding percentage is 50% for both categories of authors.

²¹ Note, however, that in table 5 "low" explicitness of evaluations is associated with "low" perception of social relationships both among officials and among nonofficials.

istics of German bureaucrats in the 1880s and 1890s, then these findings suggest one approach toward an answer to our second historical question. As sociology in the 20th century became more professionalized, more established in the academy, and (through applied research, for example) more closely associated with centers of power, evaluative involvement was no longer necessary for maintaining the perception of social structure, and it was possible to adopt a supposedly value-free stance instead.

CONCEPTUALIZATION IN THE SOCIOLOGY OF KNOWLEDGE

Turning to our third, more general point, note that in our data on the perception of social relationships there are a number of contingent associations. For example, taking sides is associated positively with perceiving social relations only among the bureaucrats.²² And being a nonbureaucrat goes along with the perception of social relations only among those authors who do not take sides.²³ In contrast, among the correlates of explicitness of evaluation there are many simple, additive associations. For example (table 3), taking sides and not being an official are additively related to explicitness, as are taking sides and making policy recommendations.²⁴ What is more, the number of correlates or determinants of the perception of social relations is greater than the number of correlates of explicitness of evaluations. For example, looking to the state is related to the perception of social relations but is not related to explicitness.

Perhaps the various components of evaluative styles—in this instance, taking sides, making policy recommendations, and making explicit evaluations—hang together by their own internal logic, or their own internal psychology to a greater extent than is the case with purely cognitive orientations. And perhaps the connections between peoples' social positions and their cognitive orientations are more numerous, and more complex,

²² Among the 15 officials who took sides in social conflicts, 40% had a "low" perception and 20% had a "high" perception of social relations. Among the 19 officials who did not take sides, 74% (as against 40%) had a "low" perception and none (as against 20%) had a "high" perception of social relationships. Among the nonofficials 52% (out of 27) of those who took sides and 54% (out of 41) of those who did not take sides had a "low" perception, while 26% and 10%, respectively, had a "high" perception of social relations.

²³ Restating some of the data in n. 22 to bring out this point, among authors who did not take sides, 74% of the 19 officials and 54% of the 41 nonofficials had a "low" perception of social relations. Among those who did take sides, the corresponding percentages are 40% (out of 15) and 52% (out of 27).

²⁴ Excluding four authors who took sides but made no policy recommendations, 32% of the 31 authors who both took sides and made recommendations were "high" in explicitness, as against 11% of the 35 authors who did not take sides but did make recommendations, and none of the 18 authors who neither took sides nor made recommendations.

than the connections between social positions and evaluative styles. For example, and just impressionistically, it seems that professors in different disciplines, or in different schools within a university, or with different relationships to students, who face a campus crisis, differ much more widely in what they see going on around them than they do in their styles of evaluating what they see. Fine differences in social position seem to make for more differences in cognition than they do in evaluative style.

One line of speculation about this issue proceeds as follows. As regards cognitions: first, the greater the inherent ambiguity of an objective stimulus, the greater the impact of social determinants upon the way people respond to it. Second, the greater the number and variety of culturally given, alternative ways of viewing that stimulus, the greater the impact of social determinants. And, third, differences in social positions are associated with different styles of thought—legal, clinical, ideological, scientific, applied-technological, or commonsensical—which vary in the ways in which and in the extent to which cognitions and evaluations are experienced as different, separate, incommensurable activities. The greater the institutionalized fusion of cognitions and evaluations in a style of thought, the smaller the extent to which cognitions are simple reflections of objective stimuli. Social determinants, then, would have the greatest impact on people who are responding to objectively ambiguous stimuli, for whom a variety of culturally given modes of response is available, and who are thinking within a style of thought which fuses cognitions and evaluations. Such specifications of the conditions under which social determinants have varying degrees of impact would not apply to evaluations, however. For evaluations do not have objective stimuli in the same sense in which cognitions do. Hence, we should expect, as seen in the data of this paper, that the social determinants of cognitions are more numerous and more complex than the social determinants of evaluations. Whatever the validity of this line of speculation, these data do suggest that sociologists of knowledge need very different approaches to understanding the cognitive and the evaluative sides of any given ideology, doctrine, or system of beliefs.

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Marxist Thought in the First Quarter of the 20th Century

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Marxist theorists have contended that Marxism is exempted from the claims of the sociology of knowledge that all thought structures and ideas need to be investigated in relation to the existential conditions and social structures in which they originate and find reception. I reject this claim and make an attempt in terms of the methods of the sociology of knowledge to investigate a variety of Marxian doctrines that emerged in the first quarter of the 20th century.

This paper investigates Kautskyan Marxism, Bernsteinian revisionism, the teachings of Rosa Luxemburg and her co-thinkers, Russian Menshevism and Bolshevism, the political writings of Georg Lukacs, and those of Antonio Gramsci in terms of the societal milieu in which they originated, the social location of their authors, and the audiences and publics to which they addressed themselves.

It is found that ideas that originated in the industrial heartland of Europe and found mass reception there transformed Marxism into a largely evolutionary and positivistic doctrine, whereas the activist and voluntaristic interpretation of Marxism was mainly developed by intellectuals in the industrially undeveloped rimland of Europe. Attempts to transfer voluntaristic Marxism to the heartland of Europe failed since they lacked a basis in the social structure.

"It is not the consciousness of men that determines their existence," says Marx (1912, pp. 11-12), "but, on the contrary, their social existence which determines their consciousness." While this statement, which is at the very root of Marxian doctrine, would seem to have enjoined Marxists to inquire into the existential roots of their own ideas, this has emphatically not been the case. In fact, as Mannheim (1936, p. 249) pointed out long ago, "this relationship [between existence and consciousness] was perceived only in the thought of the opponent." Marxists used ideological analysis to undermine the thought of their adversaries but claimed that their own doctrine had a privileged status exempting it from such analysis.

It is logically indefensible to select Marxism, of all sets of ideas and winds of doctrine, for exemption from the type of inquiry into existential roots that is being applied to all others. I agree with Mannheim (1936, p. 111) that "as sociologists there is no reason why we should not apply to Marxism the perceptions which it itself has produced."

This paper will attempt to apply the method of inquiry of the sociology of knowledge to Marxist thought in the first quarter of this century. I shall review various trends in Marxian doctrine, from Kautskyan and Austro-Marxist orthodoxy to the revisionism of Bernstein, from Leninism to the various heterodox doctrines of Luxemburg, Gramsci, and Lukacs, in terms of their social location and in relation to their audiences and publics. Although not all varieties of Marxian thought can be considered, all those that presented a certain theoretical distinctiveness and enjoyed a measure of success will be discussed. What is intended here is a broad outline of what Mannheim used to call a "structural analysis" of the main Marxist trends from the beginning of the century until, roughly, the death of Lenin (1924). I shall attempt to show that the variants of Marxist ideas that emerged in the developed and industrialized parts of Europe differed significantly from those that had their origin in the nonindustrialized rimland of Europe, and that attempts to transfer Marxian ideas originating in that rimland failed to gain acceptance in Europe's industrial heartland.

KARL KAUTSKY AND MARXIST ORTHODOXY

The German Social Democratic movement was the exemplar and beacon of all other European socialist movements, not only because of its wide appeal to the working class, but because it based its politics and actions on well-developed political and economic theories. This movement pioneered in providing leadership to large masses of people, not only politically, but intellectually as well. In spite of being persecuted and outlawed by Bismarck for many years, it had gained an impressive stature on the social and political scene around the turn of the century.

In the elections of 1890, the Social Democrats returned 35 deputies to the Reichstag; this number increased to 110 seats just before World War I, when their 4.5 million voters were about one-third of the total. Even though the party was hampered in Prussia, where under the prevailing electoral system working-class votes counted for far less than those of the middle class, social democracy was solidly entrenched in the German parliamentary system on state and national levels. It was even more powerful on lower political levels. The party had become a mass movement, strongly supported by the trade unions which it led and based on a well-organized party apparatus. It employed impressive numbers of functionaries, most of whom were of working-class origin. The daily socialist press, for example, had a circulation of 1 million in 1909. Its editorial and distribution staff numbered 330 (Michels 1949, p. 276). Already by 1904, the union movement had nearly 700 permanent officials (Michels 1949, p. 276). The social composition of the party was solidly working class, and it offered means of upward social mobility for a significant number of

self-educated workers who made their career in the party and union hierarchy. While 65 percent of the members of the Social Democratic group in the Reichstag of 1903–6 were skilled workers by origin, 43 percent listed their current occupation as employees in the labor movement (Michels 1949, pp. 271–72).

This attraction of workers to Social Democratic politics expressed the realities of German social and political conditions. The rapid expansion of industry had strengthened the working class economically, and it had led to an improvement in working-class conditions, accompanied as it was by an increase in the social security benefits which earlier had been granted by Bismarck. Yet, the political structure of German industrial society did not match its economic development. Political power was not held by the rising middle class or by the workers but by the Prussian Junkers and their hangers-on. Ample middle-class and working-class representation in parliament was not translated into real political power.

The contradiction between the socioeconomic and the political structure of German society helped determine the ambivalent character of German social democracy as well as its political theory. The party attempted to combine a politically revolutionary stance with a pragmatic commitment to gradual social and economic evolution.

The major theorist and codifier of the doctrines of German social democracy was Karl Kautsky. Born in Prague in 1854 of a Czech Jewish stage painter and a German Jewish mother, he studied history and the natural sciences at Vienna University and first dreamed of becoming a painter or an actor. Soon after, he was converted to a Darwinian-evolutionist interpretation of history as well as to Marxism. After moving first to Zurich and then to London, he soon came to the forefront among the intellectual leaders of the Social Democratic movement. A close intellectual companion to the aging Engels, he became the foremost theorist of official German social democracy. The theoretical organ of the Party, the *Neue Zeit*, which he founded and edited from 1883 on, was considered in Germany as well as abroad to be the recognized fountainhead of orthodox Marxist doctrine (Osterroth 1960).

Practically the entire career of Kautsky was within the Social Democratic party. Moreover, it was a career within a party that grew and developed impressively during the whole period before World War I. The Marxism that he and his associates developed bore the imprint of these circumstances. Kautsky's Marxism was no longer the revolutionary credo which Marx and Engels had developed in the late 1840s. Benign optimism replaced apocalyptic vision. Marx himself, and Engels more decisively after Marx's death, already had considerably softened the early revolutionary creed. Marx had admitted the possibility that in a number of nations the working class might attain power through parliamentary means rather

than through revolution, and Engels had further bent the doctrine in the direction of an evolutionary and deterministic positivism. Hence, when Kautsky began to transform Marxism into an essentially positivist doctrine predicting the inevitable victory of socialism as following from the deterministic laws of historical development, he was not aware that he departed in any way from authentic Marxist tradition.

The fact is that Marxism under Kautsky's guidance became the official doctrine as well as the expression of a mass movement which had lost its revolutionary impulse. Kautsky's determinism was well-suited to a movement that was essentially tuned to passive acceptance of the benign course of history. It was the doctrine of a movement that had been shorn in fact, though not in rhetoric, of the activism and tense expectations of imminent revolution that had once characterized Marx and Engels (cf. Lichtheim 1961).

The German Social Democratic movement led by Wilhelm Liebknecht and August Bebel had little desire to transcend the social framework within which it operated with such impressive success. As Lichtheim has put it, "Marxism functioned as an integrative ideology, not as a theory of action" (Lichtheim 1970a, p. 234). This ideology helped differentiate and separate the working class from the middle class and instilled in it a sense of its peculiar mission. It helped to immunize the workers to liberal ideas and to articulate their corporate political and social awareness. It created a socialist subculture within the surrounding official culture, but it did not energize the workers into the preparation for large-scale revolutionary action. To be sure, it was programmatically committed to revolution, but by this was meant, in fact, a purely political upheaval which would do away with Germany's autocratic political structure and replace it with fully democratic institutions. Once democracy would be attained, so reasoned its major theoretical spokesmen, the replacement of capitalism by a socialist society would inevitably follow from the laws of social and economic evolution.

The Social Democratic movement and its official Kautskyan orthodoxy could not, given the political situation in autocratic Germany, commit itself officially to a policy of "the inevitability of gradualness" in a Fabian vein. The political structure of the Kaiser's Germany was antidemocratic and was designed to keep not only the workers but also the middle classes from effective political power. The Social Democrats, therefore, fought against a political regime that denied them political and citizen rights. Hence, the party kept talking of revolution and looked to its many non-German admirers, among whom was Lenin, as the incarnation of Marxist orthodoxy. But, in fact, it became the ideological expression of a social movement that had grown rapidly within the economic framework of expanding German industry and expressed its confident optimism in a

historical process that was conceived as essentially benevolent. Kautsky, the prototypical party insider, fashioned a doctrine that appealed to the mass of party leaders and militants who had in their own lives experienced the forces of beneficent evolution (cf. Roth 1963; and Rose L. Coser 1951).

Two incidents help highlight the ambiguities of Kautskyan social democracy. Soon after Eduard Bernstein had raised the banner of revisionist reformism and urged the party to throw off its revolutionary ballast, a young Russian revolutionary who had recently moved to Germany, Alexander Helphand-Parvus, asked his Dresden comrades to propose that the coming party congress at Stuttgart in 1898 pass a resolution that would state that reform could not eliminate the class character of the state; this could only be done through revolution. Such a statement apparently seemed to him to be fully in line with official party policy. Yet Babel wrote to Kautsky, "His resolution has not the slightest understanding of our condition. The last thing we need is the congress solemnly to resolve that it strive for a social revolution" (quoted in Zeman and Scharlau 1965, p. 40).

A few years later, when the party orthodoxy was fully mobilized in the struggle against Bernsteinian revisionist reformism, when congress debates and newspaper polemics resounded with attacks against Bernstein's backsliding, Ignaz Auer, a defender of Kautskyan orthodoxy, wrote privately to Bernstein, "My dear Ede, you *don't* pass such resolutions. You don't *talk* about it, you just *do* it" (quoted in Gay 1962, p. 270).

Official Social Democratic Marxist ideology was attuned to the needs of the movement in which it grew. Shorn of the dialectical, action-oriented elements of classical Marxism, it stressed inevitable and benign evolution, shunning any apocalyptic vision of catastrophic breakdown or violent final struggle. It was the doctrine of a movement that felt that it could trust in the course of history, even while it aggressively defended the interests of its working-class constituency and helped organize it as a powerful counter-culture willing to challenge the official culture which denied it full democratic participation.

BERNSTEIN AND REVISIONISM

Eduard Bernstein, the father of revisionism, was even more a Party insider than his close friend Kautsky. He was born in a lower-middle-class section of Berlin in 1850, the son of a plumber who later became a railway engineer. The family lived in genteel poverty. He never finished Gymnasium. His formal education ended at age 16 when he began an apprenticeship in a Berlin bank. Bernstein worked as a bank clerk until 1878. He became a convert to Marxist socialism in 1872. Six years later, he accepted the offer of a wealthy well-wisher to move to Switzerland, and soon

thereafter he became associated with the *Sozialdemokrat*, the official organ of the German Party, which had by then been outlawed by Bismarck. During the rest of his long career, Bernstein worked wholly within the party. Moving to London after his Swiss apprenticeship, he collaborated intimately with Engels. In fact, he and Kautsky came to be seen as the chief exponents of Engels's version of Marxist orthodoxy (cf. Gay 1962).

Yet, in 1886, only about a year after Engels's death, Bernstein broke with orthodox Marxism. First in a series of articles in the *Neue Zeit* from 1896 to 1898, and then in the famous book *Die Voraussetzungen des Sozialismus und die Aufgaben der Sozialdemokratie* (Bernstein 1961) of 1899, Bernstein urged the party to discard its revolutionary baggage, to recognize the inevitability of gradualness, and to travel the Fabian road. The Erfurt party program of 1891, which had been written by Kautsky, foresaw an increase in the bitterness of class struggles, increasing exploitation of the workers, and the gradual disappearance of the middle class in an increasingly polarized society. None of these prognoses had been fulfilled, argued Bernstein. Class contradictions had become attenuated, not exacerbated, and the middle classes were thriving. The whole catastrophic outlook of original Marxism, which was due in part to the snares of the Hegelian dialectic with its stress on contradictions, had to be given up. The socialist movements should no longer be beholden to metaphysical doctrines; it should build on positive and empirical investigations which had disproved Marxist catastrophism. Not revolution but evolution was the order of the day. Arguing that "in all advanced countries we see the privileges of the capitalist bourgeoisie yielding step by step to democratic organization" (Bernstein 1961, p. xiii), Bernstein urged his party to put aside all hopes in a great economic crash and "to organize the working classes politically, . . . develop them as a democracy, and to fight for all reforms in the state designed to raise the working classes and to transform the State in the direction of democracy" (Bernstein 1961, p. xv).

Bernstein's writings created an enormous sensation in the party and led to a frantic movement of defense on the part of the orthodox. This may seem surprising, since Bernstein clearly "represented," as Peter Gay has said, "no startling novelty but the rational recognition of an already existing state of affairs" (Gay 1962, p. 110). Yet, upon reflection, it seems fairly obvious that by revealing that state of affairs, Bernstein threatened the elaborate Marxian superstructure which Kautsky and his colleagues had so patiently erected. Much like an overeager psychoanalyst who brutally brings hidden springs of motives to the attention of his patients and destroys their defenses, Bernstein exposed the gap between theory and actual practice in his party and threatened to undermine the Kautskyan ideological defense. Such things, as Auer had said, can be done, but they must not be said.

Revisionism was condemned and defeated at several party congresses; yet, when Bernstein returned to Germany from England in 1901, he had become the leader of a significant group within the party. Originally, most of the leading revisionists were intellectuals, but soon many leaders who were only minimally concerned with the fine points of doctrine rallied to revisionism. Its main support came from two sources, the South German party and the trade unions.

South Germany had no three-class electoral system like Prussia. It allowed full participation of Social Democrats in the parliaments of Bavaria and other southern states. Here, as distinct from the North, collaboration between liberals and Social Democrats in pursuit of progressive social legislation was a live option (Gay 1962, p. 258). While, in the North, a political revolution still seemed required to institute democratic process, in the South the political arena was wide open to all entrants. Hence, reformism in Bernsteinian or other dress appealed to Social Democratic militants and leaders in the South.

In addition to the leaders in the South, revisionism found its strongest supporters among the trade-union officials. These men were hardly sensitive to the details of Bernsteinian theory, but they found its stress on the day-to-day struggle much more to their liking than the hopes for the "final collapse of capitalism." Solid and stolid functionaries, pragmatic strategists of bread-and-butter struggles for more, they found in revisionism a convenient ideological defense against the claims of Social Democratic political leaders who attempted to mobilize the unions for their wider political aims. Finally, many of the party functionaries, Reichstag members, municipal councillors, or local deputies who were engaged daily in a politics of compromise were attracted to a simplified version of Bernsteinian doctrine.

Bernstein's career was as completely tied to the Social Democratic party as was that of Kautsky. While his doctrine was officially rejected by the party, it still grew deep roots within it. It found an audience especially among those strata and sections that were engaged in the daily struggle and were less concerned with maintaining the distinctiveness of a working-class subculture to be kept in readiness for the coming democratic revolution. Kautskyism thrived in Prussia, where the working class was effectively excluded from the political arena. Revisionism developed where the socialists already could participate in the political game. Kautsky's doctrine remained victorious in the party as a whole for which the political struggle still loomed largest. Revisionism or pseudorevisionism predominated among the trade-union pragmatists.¹

¹ Austro-Marxism, although it differed in certain respects from Kautskyan orthodoxy, will not be discussed here. Victor Adler, the founder of Austrian Marxism, was a close political and personal friend of Kautsky and differed from him only marginally.

ROSA LUXEMBURG, PARVUS, AND RADEK: THE LEFTIST
REVOLT AGAINST KAUTSKY AND BERNSTEIN

In contrast to the revisionists who developed within the core of the party and who were faithful party stalwarts, the chief exponents of leftist heterodoxy in the German movement were outsiders. Its three most brilliant theorists, Rosa Luxemburg, Parvus, and Radek, had come from Eastern Europe.

Mannheim has remarked that "to the extent that Marxian proletarian groups rise to power, they shake off the dialectical elements of their theory and begin to think in the generalizing methods of liberalism and democracy . . . whilst those who, because of their position, still have to resort to revolution, cling to the dialectical element" (Mannheim 1936, p. 118). This remark is exceedingly perceptive of the trends we analyze. Those who represented Marxian groups on the road to (democratic) power, be they Kautskyans or Bernsteinians, in fact abandoned dialectical or voluntaristic thinking in favor of positivistic evolutionism, while those whose experience had been formed in the revolutionary struggles against Czardom clung to an action-oriented revolutionary Marxism even when they transferred the seat of their activity from the East to Germany.

"The contrast between *postulating* revolution and *being* revolutionary," Luxemburg's biographer, Nettl (1966, p. 5), has written, was the central issue that separated her from Kautsky. She never produced a comprehensive or even logically cohesive system, casting her ideas almost invariably in the form of criticism and polemics (Nettl 1966, p. 9), but what unity there was to her thought came from her revolutionary conviction, from her total identification with the idea of revolution. This idea, in turn, was nourished in an environment that was far removed from that in which Kautsky and Bernstein had grown up. Although she spent her most productive years in the German Social Democratic movement, Rosa Luxemburg had her roots in the Polish revolutionary tradition.

Rosa Luxemburg was born in 1871 in the Polish provincial town of Zamosc. Her father was a timber merchant, and the family was comfortably well off. Her parents were Jews who spoke and thought in Polish and rejected any identification with the traditional Jewish community. As a consequence, they were largely thrown back on their own resources. Neither in Zamosc nor in Warsaw, where they moved when Luxemburg

The later generation of Austrian Marxists, men like Max Adler, Otto Bauer, and Rudolf Hilferding, tended to stand somewhat to the left of Kautsky, even though some of them rejected both dialectical thought and positivism in favor of a Kantian ethical doctrine akin to that of Bernstein. Their "leftism" is explained in terms of their realization of the extreme frailty of the structure of the Austro-Hungarian empire and their sense of its impending breakup. It was much harder to believe in peaceful progress in an environment like that of Austria which was racked by exacerbated national struggles than in ethnically homogeneous Germany.

was a small child, did they seem to have formed wider attachment. Luxemburg's father had been educated in Germany, and German was read and spoken in his house, where German culture was much admired. Shunned by the traditional Jewish community, without ties to Polish friends, and admiring things German from a distance, the family seems to have lived in a condition of triple marginality.

During her last few years at the girls' high school in Warsaw, Luxemburg was already in contact with groups of illegal revolutionaries. When she was 15, four leading revolutionaries of the group Proletariat were hanged on the gallows. Soon after graduation, she became a member of a cell of the Revolutionary Party Proletariat. From that time on, the revolutionary socialist movement came to be for Luxemburg the only "home" she was ever to know, the only environment that allowed her to come to terms with her thrice-compounded cultural marginality. The movement allowed her to transcend her alienation through allegiance to socialist internationalism.

In 1889, warned of the imminence of her arrest, Luxemburg was smuggled out of the country and went to Switzerland to study economics and to deepen her knowledge of Marxism. In Zurich she established close political and personal contacts with the leaders of the Polish Social Democratic movement in exile. Here she met Leo Jogiches, her close companion for many years. She soon moved to the forefront of a revolutionary group which split from the main Polish socialist party because of its uncompromising rejection of the idea of Polish nationalism and national independence. From then on, until the war, Luxemburg was a leading member of a small band of brilliant Marxists who directed from abroad the underground *Social Democracy of the Kingdom of Poland*. Throughout these years she was intimately involved in the debates and factional quarrels of the organization, and she wrote major parts of its political statements and programmatic announcements. Even though she was to become an active participant in the German Social Democratic movement, she remained all the while deeply immersed in the revolutionary underground struggle of Poland.

When Luxemburg left Switzerland for Germany in 1898, it was not because she had special admiration for things German. In fact, she personally never liked Germany or Germans. She went because, says Nettl, "the political quality of German Socialism dominated her thinking" (Nettl 1966, p. 31). "Throughout her life in Germany she remained a self-conscious Easterner" (Nettl 1966, p. 32). Germany simply happened to be the country where the action was, the country with the strongest and, as she then thought, the most orthodox Marxist movement in the world. Her allegiance was not to Germany but to the German Social Democratic party (SPD).

What Luxemburg and her associates, such as Parvus, brought into German politics, was a quality hardly known there before. It was what Trotsky had called "the Russian method" — "the idea that action was of a superior order to any other facet of political life" (Nettl 1966, p. 34).

When Luxemburg arrived in Germany, the great Bernstein controversy had just broken out. The elders of the party had been slow to pick up the gauntlet Bernstein had thrown them. In fact, Parvus, another Easterner, wrote the first series of sustained attacks against "Bernstein's Overthrow of Socialism." Kautsky initially disassociated himself from Parvus's fiery attacks and only gradually came to realize the challenge revisionism posed to his position. In the meantime, Luxemburg joined Parvus in violent and often quite personal attacks against Bernsteinian heresy. At last Kautsky came to appreciate their polemical onslaughts, even though he continued to agree with many party leaders that their violent personal tone was "tasteless."

Although Luxemburg eventually became an ally of Kautsky during the Bernstein controversy, these allies in fact were operating on a different wavelength. For Kautsky this was a family quarrel to be carried out with all the personal tact and consideration owed to old party comrades. For Luxemburg it was a passionate fight against a heresy that threatened her very revolutionary identity. To her, Bernstein was not just in error but in sin. The German party spokesmen who complained of Luxemburg's tactlessness or who deplored the "unpleasant tone in the party press produced by the male and female immigration from the East" (Nettl 1966, p. 187) failed to realize that what was at issue was, not only a matter of manners and morals, but a profound difference in overall vision. Luxemburg felt that by questioning the final aim of revolution Bernstein was destroying the need for any proletarian class consciousness. "The entire difference [between revisionism and orthodox Marxism]," she wrote, "becomes this: according to the traditional conception, the Socialist purpose of trade-union and political struggle consists in preparing the proletariat for social upheaval, i.e., *emphasis on the subjective factor*. . . . In the traditional conception the trade-union and political struggle brings the proletariat to realize that it is impossible to alter its situation through such a struggle . . . and convinces it of the inevitability of its final seizure of political power" (Nettl 1966, p. 224; italics mine).

To the Polish revolutionary, emphasis on the subjective factor meant the rejection of any passive trust in evolution. It meant the need for the Social Democratic movement to energize the class consciousness of the working class. The concrete improvements in the immediate conditions of the workers which were in the forefront of Bernstein's concerns, and which in the last analysis also informed Kautsky's orientation, were to Luxemburg and her co-workers only important to the extent that they

helped prepare the workers for the revolution to come. For the Eastern revolutionaries, who were only inorganically appended to the German Social Democratic movement, what counted was, not the party as such or its multiple affiliated organizations, but only its role as an instrumentality for heightening the class consciousness of the German workers.

This fundamental divergence of views helps explain Luxemburg's break with Kautsky in 1910. Kautsky had welcomed her support against Bernstein, and he found her useful in many other respects, but when she embarked on a defense of the mass strike as the privileged instrumentality for the radicalization of the movement, he felt that the parting of the ways had come. Luxemburg's advocacy of the mass-strike strategy was especially galling to the party leadership because it was based on the experiences of the revolutionary movement during the first Russian revolution in 1905. When Luxemburg argued for a "leapfrog" effect by which the achievements of the Russian working class would serve as a model for the German workers, she attacked the ingrained sense of German superiority that was so strong an element in Kautskyan orthodoxy and in the SPD, and, in addition, challenged their evolutionary world view.

Most of Luxemburg's writings before 1905 were in defense of the SPD. After her return from Poland, where she took an active part in the revolutionary events, Luxemburg changed her general attitude. She now saw in the organization of the party, and especially in the trade unions, a potential hindrance to the development of class consciousness and revolutionary activity. "Where previously discipline and tradition had served to eradicate errors, now only mass action could sweep them away" (Nettl 1966, p. 513). From now on, Luxemburg trusted only in the revolutionary potential of the masses. She prodded them, appealed to them, idealized them, and believed until her last day that only they would bring salvation to suffering mankind. When, during the war, practically the entire party betrayed the cause of socialist internationalism, Rosa Luxemburg was saved from utter despair by her belief that the masses had been betrayed by their leaders but that they would, in due time, rise from the ashes to realize the vision the party had abandoned.

After Luxemburg's assassination in 1919, Kautsky wrote in his tribute to her, "I take my conception of theory from the French, German and Anglo-Saxon experiences, rather than the Russian; with Rosa Luxemburg it was the other way around" (quoted in Nettl 1966, p. 512). Kautsky here touched upon the crux of the matter. Despite her many years in Western Europe, Luxemburg's conception was basically shaped by her Eastern background and experience. She failed to realize that the German working class, settled, disciplined, solid, and on the way to *embourgeoisement*, had but little in common with the disinherited proletarians who had demonstrated in the streets of Saint Petersburg and Warsaw in 1905.

She endeavored to infuse into the German working class that revolutionary spirit and ardor which she had absorbed in her Polish milieu, and she never understood the extreme structural differences between a highly industrialized society in which the working class had a stake and conditions in which it was an outcast underclass.

Even after her break with Kautsky, Luxemburg seems not to have fully realized how far her alliance with the official SPD leadership had been based on mutual misunderstanding. Kautsky opposed Bernstein because the latter's outspoken revisionist realism threatened to upset the cozy equilibrium of revolutionary programmatic statements and reformist practice through which the party officials had established their intellectual dominance in the party. Luxemburg fought Bernstein because he opposed the messianic vision of a coming cataclysmic proletarian revolution on which her whole world view was premised.

In the last analysis, and this makes for her profound appeal as a person as well as for her ineffectiveness as a theoretician, she was moved by moral revulsion against capitalist society and by deeply ingrained humanitarian ideals. She represented one of the last offshoots of the great tradition of the enlightenment. This tradition was already dying in the West at a time when it still energized the Russian-Polish intellectual circles in which Rosa Luxemburg had spent her formative years. It was her tragedy that she approached German and Western European society with sets of ideas and ideals which had already become obsolescent there. She thought that she represented the vanguard of the European proletariat, while, in fact, the alleged renegade Bernstein had a better grasp of the shape of things to come.

When the war broke out, and international socialism collapsed, Luxemburg's world came to an end. The world of the Second International was her world even though for many years she had riddled most of its leaders with her well-aimed arrows. As the intellectual leader of the Spartakus League, that little band of self-sacrificing socialists who opposed the war, her activities were morally admirable but politically largely ineffective. The league had little political influence during the war, and it remained a marginal force thereafter. Even the disastrous uprising of January 1919, which led to the assassination of Luxemburg, Jogiches, and Karl Liebknecht, had not been planned by the league's leaders. They followed it out of the morally estimable and politically suicidal impulse "not to abandon the masses."

I take it to be of considerable sociological significance that the two most consequential exponents of the revolutionary Left in the prewar SPD besides Luxemburg, Parvus and Radek, stemmed like her from the Eastern revolutionary movement and never gained secure roots in German soil. Alexander Helphand-Parvus was born a Russian Jew in the province

of Minsk in 1867. His father was a locksmith or a blacksmith who moved to Odessa when Parvus was a small child. Already involved in socialist circles in his homeland, Parvus was fully converted to revolutionary Marxism when he moved to Switzerland in 1887. After the conclusion of his academic studies in Basle, Parvus left Switzerland in 1891 for Germany. Like Rosa Luxemburg, he wanted to join the ranks of the SPD to serve it as a revolutionary activist. "Whether Russian or German," he wrote, "the struggle of the proletariat always remains the same, and it knows neither national nor religious differences. . . . When I became unfaithful to my native Russia, I also became unfaithful to that class from which I originated: the *bourgeoisie*. My parting of company with the Russian intelligentsia dates from that time" (quoted in Zeman and Scharlau 1965, p. 21). Yet, although Parvus disengaged himself from his Russian homeland, he never fitted into his new home, the German SPD. Just as in the case of Luxemburg, its powers-to-be made use of his dazzling intelligence when it suited their needs, even when they deplored his polemical manners; but they rejected what they judged to be his revolutionary romanticism. As a result, Parvus was, as his biographers put it, "a strategist without an army." Having few of Luxemburg's endearing human qualities and living the life of a bohemian, Parvus never managed to establish the close personal relations with men like Kautsky that had stood Luxemburg in good stead in her years of German apprenticeship. As a result, he remained an outsider whose revolutionary ardor and analytical rigor served the cause of anti-Bernsteinism but whose subsequent theories, anticipating Trotsky's notion of a permanent revolution which would be initiated by the Russian proletariat and would spread over the whole of the globe, failed even to ruffle the doctrinaire feathers of Marxist orthodoxy.

What characterized Parvus's Marxism, as it characterized that of Luxemburg, was its emphasis on revolutionary activity. "Only a revision to the left of our party principles is now possible," he wrote in 1900, "in the sense of the extension of political activity, . . . of the intensification of social-revolutionary energy . . . of a bold endeavor and will, and not of fearful, reserved softness" (quoted in Zeman and Scharlau 1965, p. 45). It was precisely that revolutionary activism which went against the grain of the party orthodoxy and led to Parvus's isolation.

As with Parvus, so with the third major figure on the revolutionary Left, Karl Radek. The son of "modernist" Galician Jews of the lower middle class, the father a minor post-office employee, Radek grew up in a milieu in which traditional Jewishness was rejected and German culture was extolled. His father having died when Radek was a small child, he was taught by his mother and his uncles to consider himself a child of the German Enlightenment and to shun contact with the despised Poles or

with "retrograde" Jews. In reaction against this constricting and petty-bourgeois milieu of Austrian Poland, the young Radek turned to the tradition of the Poles and immersed himself in the Polish revolutionary heritage. By 1902, he had become a convert to Marxism, and in 1903 he emigrated to Zurich, the traditional haven for uprooted Eastern socialists. After a few years, like Luxemburg and Parvus, he went to Germany to become a militant in the SPD. And, as in their case, his revolutionary activism failed to make more than a minor dent in the armor of party orthodoxy (cf. Lerner 1970). Perhaps the most talented of all of Luxemburg's younger followers—although Luxemburg herself disliked him intensely and eventually had him expelled from her Polish organization—Radek, even more of a bohemian than Parvus, failed to find many supporters in the German party. This became obvious after he attacked Kautsky's relatively benign theory of imperialism in 1912, shortly before the upheaval of the war. (Soon thereafter, Luxemburg published her *The Accumulation of Capital*, a more consequential attack against Kautskyan optimism, in which she argued that imperialism was doomed to end in revolutionary breakdown.) Kautsky had argued that pacifist elements in capitalist society were opposed to such aspects of imperialism as the arms race and that socialists should cooperate with them to help tame imperialism from within. Radek bridled at such heresies. He summed up his lengthy refutation with a rhetorical flourish: "Out of fantasy one can create poetry; out of speculation, inferior philosophy; but the struggle demands a sword" quoted in Lerner 1970, p. 27). Soon after, Radek was expelled from the Germany party under the pretext that he was no longer a member of its Polish sister organization. Men who brandished revolutionary swords apparently had no place among the orthodox defenders of benign evolutionary necessity.

Emphasis on three Easterners as major representatives of the Left revolutionary tradition in prewar Germany does not imply, of course, that the Left did not have German members. As a whole, however, these men were not of the same intellectual stature, and they did not play major roles in the theoretical battles. Karl Liebknecht, the son of one of the founders of the Social Democratic party, was mainly an organizer and agitator; Clara Zetkin, Luxemburg's intimate friend, had considerable moral authority but little intellectual distinction. The only Left leader who was a native German and ranked high as an intellectual was Franz Mehring. He was a most atypical figure. Babel once called him "a psychological riddle" (quoted in Osterroth 1960, p. 220). Having started his career as a liberal and antisocialist writer, he was converted to socialism in the early nineties, when he was already 45 years old. As the author of a multivolume *History of the German Social Democratic Party*, of the classical biography of Karl Marx, and of a series of important historical studies,

he was highly respected in the party but carried relatively little weight in current debates. Long a defender of Kautskyan orthodoxy, he moved into the Luxemburgist camp only shortly before the war, when he was already an old man and his main energies were spent.

The Left attracted to its banner a number of younger intellectuals impatient with the staid ways of the party. Luxemburg's fiery oratory, her impassioned writings, as well as her teaching at the Berlin party school made her a beloved figure in militant and activist circles. The Left at times managed to take over party newspapers in Saxony, Bremen, and in the South and used them as springboards for its revolutionary agitation. But it never managed to touch the inner core of the party or its ordinary followers. The trade-union leadership, in particular, seems to have agreed with the pithy saying of one of its leaders, "General strike is general nonsense." When the war came, the two most consistent supporters of Luxemburg among socialist journalists and editors, Paul Lensch and Konrad Haenisch, became German superpatriots. Surely, the revolutionary message had sunk no deep roots in German soil. We shall now turn to examine it on its Eastern home ground.

RUSSIAN MARXISM

Kautsky and the main stream of Marxist orthodoxy in Germany expressed in their political theories the need for Germany to catch up with the achievements of the French Revolution which the German middle class had not managed to emulate. The early Russian Marxists aimed in a similar way at making their country catch up with enlightened Western Europe and replace "Asiatic" and autocratic czardom by liberal democracy. Since the middle class was exceedingly weak, they reasoned that the working class would have to accomplish the major share in the democratic transformation of Russia even as it prepared itself for the task of socialist transformation that was to follow a democratic revolution. But while the German working class was powerful and organized and had a heavy specific weight in society and economy, the Russian working class was still numerically weak, unorganized, and largely shaped in its attitudes and orientations by its recent rural origins. These circumstances helped determine the character of the Russian Marxist movement and created the dilemmas with which it was faced.

The first major theorist of the Russian Marxist movement, Georgi Plekhanov, followed closely in Kautsky's footsteps. His Marxism was rigorously deterministic and evolutionary. Yet the idea of evolution did not, to him, exclude the idea of revolution. "There can be no sudden change," he wrote, "without a sufficient cause, and this cause is to be found in the previous march of social evolution. But, inasmuch as this evolution never

ceases in societies that are in the course of development, we may say that history is continually preparing for sudden changes and revolutions. . . . These political catastrophes are absolutely inevitable" (quoted in Baron 1963, p. 293).

Plekhanov had been converted to Marxism from his earlier Populist allegiances when he became convinced that Russia could not skip the stage of capitalism on its road to socialism. Henceforward, he kept rigorously to the idea that the growth of proletarian consciousness, on which the achievement of socialism rested, depended on the continued growth and development of capitalism in Russia. Without large-scale capitalism, no powerful working class movement was possible.

Practically all Russian Social Democrats, whether Mensheviks or Bolsheviks, were originally disciples of Plekhanov. But, in their attempt to apply his teachings in the political and economic struggles of Czarist Russia, they soon faced difficulties that Plekhanov had but imperfectly foreseen. In particular, they began to realize that Russian workers, although they welcomed the radical intelligentsia when it aided them in their economic struggles, tended to remain cool to the larger revolutionary aims of the Marxist radicals. Through propaganda and agitation, socialist intellectuals hoped to energize a politically indifferent working class. Yet, most of the time, the proletarian masses, when they acted spontaneously, restricted their vision to purely pragmatic economic issues. Since the masses seemed unable spontaneously to embrace wider political aims, the partisans of spontaneity among the Social Democrats gathered in one camp; and the advocates of political consciousness, in another (cf. Haimson 1955).

The dilemma may be illustrated by the problems faced by Martov and Lenin in their effort to give Social Democratic leadership to the Saint Petersburg labor movement in the 1890s (cf. Pipes 1963; see also Wildman 1967). In this period, the movement consisted of two dissimilar segments. On the one side was the mass of unskilled and semiskilled workers, especially in the textile industry, most of them with yet strong attachments to the peasant milieu from which they had recently come. These laborers distrusted all intellectuals and for long remained immune to their influence. On the other side, a labor aristocracy of skilled workers, mainly from the metal industry, who had been urban dwellers for several generations, displayed considerable social and political awareness. These educated machinists, thirsty for knowledge, furnished the several hundred members of the study circles which Social Democratic as well as Populist revolutionary intellectuals organized in Saint Petersburg in the period from 1885 to 1897.

However, when the first mass strikes occurred in 1896 and 1897, involving as many as 30,000 textile workers, it turned out that the Social

Democratic intellectuals, united by then in the Union of Struggle for the Emancipation of the Working Class, gained only peripheral influence in it. They helped couch demands in precise and literate language, but the demands themselves emerged spontaneously. The whole movement was led and organized by rank-and-file workers who formulated the aims of the strike, among which a reduction in working hours loomed largest. The Social Democratic and the labor movement never merged in Saint Petersburg or elsewhere until 1905, when the two seemed to come together for a brief period. Labor was mainly concerned with economic improvement or, in the case of a section of the labor aristocracy, with intellectual self-improvement. The Social Democratic intelligentsia aimed at the development of revolutionary politics (cf. Pipes 1963, pp. 117ff; and Frankel 1969).

In 1897 Lenin was arrested in Saint Petersburg as a main organizer of the Union of Struggle and spent the next three years in exile in Siberia. When he reached Europe and digested his Russian experiences, he went through a major political and spiritual crisis. He now realized that the aims of labor and of the radical intelligentsia did not necessarily coincide. "On the one hand," he now wrote, "the labor movement separates itself from socialism . . . , on the other, socialism separates itself from the labor movement . . . the labor movement, separated from Social Democracy . . . inevitably become bourgeois" (Pipes 1968, p. 49). Lenin had now come to doubt the revolutionary potential of labor. If labor was not revolutionary, at least potentially, then the whole conception that he had heretofore shared with Plekhanov and all Russian Marxists, namely, that the development of capitalism necessarily entailed the growth of proletarian class consciousness, was put into jeopardy.

Lenin escaped his dilemma by a fundamental revision of previously accepted Marxian doctrine. He now argued that the workers could never spontaneously attain class consciousness, that the working class would become a revolutionary force only if it accepted the leadership of trained professional revolutionists who would inculcate class consciousness, as it were, from the outside. Working-class spontaneity now became the main impediment to political mobilization. "The task of Social Democracy is to combat spontaneity, to divert the labor movement, with its spontaneous trade union strivings, from under the wing of the bourgeoisie, and to bring it under the influence of Social Democracy" (quoted in Haimson 1955, p. 134). Marxists, Lenin now argued, could not afford to follow at the tail of popular movements, for that would be "subservience to spontaneity." Hence, the Marxist party, "the great whole which we are creating for the first time" (Lenin, n.d., p. 464) as the embodiment of disciplined activist drives, became the foremost instrument for achieving revolutionary goals. Consciousness and spontaneity were now definitely severed. Consciousness was

embodied in the revolutionary party which imposed its will on the "natural," "spontaneous" course of working-class activity. Revolutionary will became the main drive of history. Optimistic reliance on the idea that the beneficent course of evolution would spontaneously increase working-class consciousness was replaced by a voluntaristic doctrine which proclaimed the need to rape history if it was unwilling to submit.

There is no need here to go into the complicated and intricate political maneuvers, the moves and countermoves, the intrigues and polemics, which led to the split between Bolsheviks and Mensheviks. At the risk of oversimplification, it can be said that Menshevism, led by such men as Martov, Axelrod, Dan, and, after some initial hesitation, Plekhanov, opted for spontaneity and against Lenin's "consciousness brought from the outside." Only some Marxists adhered to what was called "economism," limiting themselves to support the economic struggles and demands of labor without immediately posing wider political questions. Most Mensheviks, in contrast, actively propagandized for their political demands among the workers and attempted to prepare them for the coming democratic revolution against Czarism. But all of them rejected the voluntaristic elitism of Bolshevism. To them, Lenin had abandoned orthodox Marxism for a new version of Jacobinism or Blanquism which subordinated the working class to the heavy hand of the dictatorship of the Bolshevik party.

The voluntaristic Leninist version of Marxism grew in a country and in a social situation in which a young, numerically small, and yet partly peasant-rooted working class did not, or not yet, spontaneously generate the class consciousness on which the politics of all previous Marxist movements had been premised. When Lenin realized that, after all, being does not always determine consciousness, he decided that the radical Marxist intelligentsia would foist it on the working class even against its will. He would force them to become conscious. Consequently, while Menshevism was a version of Western orthodox Marxism, adapted to the conditions of autocratic Russia and hence more revolutionary and activist in orientation than in the West, Leninism was an entirely new variant of Marxism, if it can be called Marxism at all.

We now turn to a brief examination of the Russian Marxist intelligentsia from which Lenin recruited those "professional revolutionaries" in whom he invested all his hopes.

Their social origin is to be found in three strata of Russian society. They were either sons of the gentry and service nobility (like Lenin and Plekhanov), or emancipated Jews (like Trotsky and Martov), or sons of *raznochintsy* (literally, "men of many ranks" or "unrankables"), that is, plebian offspring of priests, servants, shopkeepers, or freed serfs (like Stalin and many secondary leaders of both Menshevism and Bolshevism). It serves no purpose to attempt to distinguish between Leninists and

Mensheviks in terms of social origin. All three categories of origin were represented in both organizations, and emancipated Jews took pride of place in the upper echelons of both. Whether a particular individual chose to align himself with one or the other wing depended on specific circumstance and on ideological orientation rather than on origin.

What united the members of the radical intelligentsia was a deep hatred and loathing for Russian backwardness. To Lenin, just as to all other Russian Marxists, the West was "democratic, modern and progressive," and it was sweeping steadily toward social revolution. Russia, by contrast, was "Asiatic, barbarous, and backward" (cf. Wolfe 1955, p. 161). Trotsky expressed this common loathing of the radical intelligentsia most eloquently when he wrote, "The revolution means the final break of the people with Asianism, with the 17th century, with holy Russia, with ikons and cockroaches, not a return to the pre-Petrine period, but on the contrary an assimilation of the people to civilization" (quoted in Carr 1970). Whatever else the radical intellectuals may have been, they were above all men who wished to propel loathsome, sluggish, backward Russia into the 20th century; they were activist modernizers in a hurry.

Russian Marxist intellectuals considered themselves the heirs of the great Populist revolutionary tradition that had preceded their efforts. Even though they differed from populism in both program and orientation, they could not but consider the activist revolutionaries of the previous period as brothers under the skin. By contrast, they sharply attacked those members of the intelligentsia, the great majority, who declined to join the revolutionary struggle. Their emphasis on the supreme importance of *activity* can be interpreted, at least in part, as a reaction against the apathy and intense self-absorption of the nonpolitical intelligentsia. This rejection of the passivity of the intelligentsia was especially pronounced among the Bolsheviks. To Lenin, the heroes of Turgenev and Chekhov, so intensely introspective that they have no capacity to act, seemed the very embodiment of those pernicious tendencies that had to be rooted out if Russia was to be pushed forward. Turgenev's *Rudin*, of whom the author wrote that "nature has . . . denied . . . him the power of action, the ability to carry out his intentions"; the hero of *The Diary of a Superfluous Man*, who "analyzed [himself] to the last shred"; Goncharov's *Oblomov*, that antihero who consumes the first 200 pages of the novel in an effort to get out of bed and the rest with vague dreams of what he will do, dreams he will obviously never realize—these characters were to the Bolsheviks and to all Marxists symbols of Russian passivity and sloth. Passivity was the enemy (cf. Coser 1965, pp. 160–61).

When the Marxist radicals turned to the working class, conceiving it to be the privileged instrument for bringing about the revolution that would put an end to Russia's backwardness, they soon faced a dilemma

which in another form had faced the Populists when they "went to the people," that is, the peasantry. The Populist Social Revolutionaries found, by and large, that the peasants failed to understand what moved those *intelligentsia* who attempted to propagandize in their villages. Still deeply attached to throne and altar, they often turned the revolutionaries over to the police. The Marxists were not as thoroughly divorced from the workers. As had been seen, small cadres of skilled and self-educated workers served as transmission belts between the Marxists and the emerging labor movement. Yet the working class as a whole still consisted in large part of recent migrants from the villages who had just begun to make the first faltering demands that transcended their immediate economic interests. Hence, the dilemma of choice for the Marxists between "spontaneity"—that is, reliance on the inherent developments within the labor movement coupled with activist efforts to educate workers politically—and "consciousness"—that is, the attempt to force the development through the voluntaristic activism of professional Bolshevik revolutionaries.

The activist interpretation of Marxism was shared by most Russian Marxists who faced the Czarist autocracy and were forced, for the most part, to operate under conditions of extreme repression and illegality. Extreme voluntaristic activism, on the other hand, was the hallmark of Lenin's doctrine. Both tendencies stemmed from the conditions of a still largely agrarian country in which, in contrast to the West, there existed no strong and self-conscious working-class movement. But the Leninist tendency was rooted in the idea that, when nothing could be expected for the time being from action *by* the workers, then action had to be taken *for* the workers.

Despite superficial similarities, Lenin's and Luxemburg's activism differed fundamentally. Luxemburg based her ideas on her Polish experience, where the working class, although still weak, was considerably more self-conscious and better organized than in the rest of Russia. Disregarding the fundamentally different context in which the German labor movement operated, she then concluded, erroneously, that the German workers, if not held back by bureaucratic fetters, would spontaneously reach for revolutionary goals. Lenin, in contrast, developed his doctrine in the context of a society in which the working class seemed unwilling and unable to act spontaneously in a revolutionary direction. His whole experience having been shaped in this Russian context, Lenin attempted after he had attained power in Russia to transfer his conception of voluntaristic activism by professional revolutionaries to the European scene (cf. Borkenau 1962). He did not trust spontaneity be it in the East or in the West.

In the Leninist as well as the Luxemburgist case, endeavors to transfer the Eastern experience to the West proved a failure. Both foundered on the rock of the fundamentally different class structures of maturing industrial

societies. Yet, after the war, voluntaristic interpretations of Marxism found still other defenders in Western Europe. But here again, the theorists expounding such doctrines had their roots in nonindustrial areas and were marginal to the mainstream of the working-class movement in industrial Europe.

GRAMSCI AND LUKACS

It is significant that the only two Marxist theorists of marked originality who emerged after the First World War both came from the agrarian rim of Europe and not from the industrialized heartland. And it is equally significant that these men, just like their Eastern forebears, elaborated a markedly antideterministic and voluntaristic interpretation of Marxism.

Antonio Gramsci came from Sardinia, an utterly backward area, even by southern Italian standards. In Gramsci's time, not even 5 percent of the total population of Sardinia was employed in industry. Its landed aristocracy ran the island in semifeudal fashion and prevented the emergence of a self-conscious middle class. The island was isolated both geographically and culturally. It was one of Europe's most stagnant backwaters.

Gramsci was born in 1891, the son of a petty clerk in the state bureaucracy. When he was six years old, his father was arrested for "administrative irregularity" and sentenced to five years in prison. Two years earlier, young Antonio had fallen down a steep flight of stairs and, as a result, acquired a hunched back. When the father decided that the family could not afford to send Antonio to junior high school, he was sent to work for 10 hours a day in his father's land office for a salary of roughly two dollars a month. Two years later, his mother and sister scraped together enough money from their work as seamstresses to send the boy to *ginnasio*. To say that he had an unhappy childhood risks sounding like an understatement. In later years, he was to reflect that he had "known almost always the most brutal aspects of life" (cf. Cammett 1967, p. 12; and Hughes 1961, pp. 96 ff).

A sense of personal humiliation and alienation merged in Gramsci's mind with his loathing for the backwardness of the island on which he spent the first 20 years of his life. In later years, he referred to the "sewer of my past," and recalled his "continual effort to overcome those backward ways of living and thinking characteristic of Sardinians at the beginning of the century." Imprisoned in a stultifying atmosphere, he spoke of his strong need to break out of the Sardinian swamp to join "national" and "European" life and culture (Cammett 1967, pp. 3-11).

Late in 1911, Gramsci left Sardinia forever and enrolled as a student at the University of Turin. Turin was then the capital of Piedmont and the leader in Italian culture and technology. Gramsci must have felt that

he had suddenly moved from the 17th to the 20th century. At the university, he came under the influence of the philosophy of Benedetto Croce. This neoidealistic historian-philosopher dominated Italian intellectual life and attempted to fuse the heritage of Vico and Hegel into a relativistic historicism somewhat akin to that of Dilthey in Germany. Soon after encountering Crocean ideas, Gramsci joined the socialist movement and familiarized himself with Marxism. Throughout his career, he attempted to infuse a measure of Crocean "idealist" flexibility into the structure of Marxist thought.

The Turin labor movement was the most developed in all Italy. The young rebellious man from the backward provinces was tremendously impressed by it and began soon to see in it the instrumentality that would help bring about revolutionary change in Italy. His diffuse and inchoate hatreds for things as they were became focused and were given coherent shape through his identification with the Turin workers. Working at first as a journalist and pamphleteer at the extreme left wing of the Italian Socialist party, Gramsci came into his own during the large-scale strikes that shook Turin in 1920 and led to the occupation of the major factories by spontaneously emerging factory councils. He saw in these councils the crucible in which the working class could develop those cultural characteristics that would make it fit to impose its "hegemony" on the rest of society. He argued that the working class, before it can seize state power, must establish its claim to power in the political, cultural, and ethical field. It cannot only fight for "crude" economic interests, but as a conscious class must become the carrier of a distinct *weltanschauung* (Cammatt 1967, p. 205).

The factory council movement soon collapsed, and the disunited and rudderless socialist movement proved incapable of stopping Mussolini's march to power. Gramsci became a founding member of the Italian Communist party and worked in its leading cadre. Arrested by Mussolini in 1926, he remained in prison until 1937. He was released three days before his death. His most significant writings are found in his *Letters from Prison*.

Gramsci's unorthodox approach to Marxism had a decidedly voluntaristic cast. He argued that traditional Marxism was overly materialistic and deterministic. He opposed to the "economistic superstition" the "philosophy of praxis" (Cammatt 1967, p. 191).

Marxism could only become an adequate philosophy, Gramsci argued, by emphasizing the dialectical aspects of the interplay between proletarian action and the ideology developed by intellectuals. His biographer, Cammatt (1967, p. 191), puts Gramsci's thought well when he writes: "The Marxist intellectual develops the principles and the problems created by

the masses' practical activism, and the conclusions of this theoretical activity are then used to change the practical realm."

Gramsci aspired to a union of "spontaneity" and "conscious leadership" which would eventuate in disciplined praxis of the working class that would establish its hegemony over the rest of society. He rejected the belief that economic conditions determine irrevocably the human condition. Political initiative is always needed to "free the economic thrust from the shackles of traditional politics." In the last analysis, what counts is not the economically and socially given but a determined will to power infused in the working class by its leading intellectual spokesmen.

Despite his admiration for working-class spontaneity, Gramsci's doctrine boiled down to a rejection not only of Marxist determinism but an exaltation of the creative role of intellectuals. Ideas did not spring spontaneously from material conditions; they had an autonomy of their own. Revolutionary advances occurred only when intellectuals, imbued with creative will, fused their energies with a working class ready to conquer the nation. Gramsci did not feel that it was necessary to wait until "all the economic conditions were ripe" to bring about the revolution. Proletarian liberation could be achieved through voluntary praxis in the here and now.

The similarities between Gramsci's and Lenin's view are perhaps more striking than the evident differences in styles of thought and in philosophical sophistication. Gramsci's thought was in tune with contemporary philosophy of history, while Lenin's Marxism was crude indeed. But what is important for my purpose here is not this different level in style or argument but the fact that the son of the school inspector from Simbirsk and the tortured outsider from backward Sardinia both endeavored to impose their vision of a willed transformation of the human condition on a working class that seemed resistant and unwilling to undertake this "historical mission." These rebel "outsiders" could not trust in the comfortable and optimistic evolutionary vision of "insiders" like Kautsky. They bent Marxism in an activist and voluntaristic direction. In Lenin's Russia, this voluntarism led to a rejection of "spontaneity" and a generalized distrust of the as yet weak and inchoate labor movement. Writing from the vantage point of a more developed labor movement, Gramsci attempted to fuse spontaneity with the consciousness of intellectuals.

It is among the finest ironies of history that the rebellious Sardinian outsider has now become the patron saint of an Italian Communist movement which in its daily practice is almost as respectable and bureaucratic, as complacent and unrevolutionary as the SPD under the Kaiser. There was perhaps more to economic determinism than Gramsci was willing to allow.

At first blush, Georg Lukacs's social origins could hardly be more dis-

similar from those of Antonio Gramsci (cf. Lichtheim 1970*b*; and Watnick 1962). His early life was not marked by suffering and poverty; he grew up in a world of abundance. His father was the director of a major bank in Budapest and, despite his Jewishness, had been ennobled by the imperial government. And yet there is much in common between Gramsci's and Lukacs's backgrounds. They both grew up, not in the industrial heartland of Europe, but rather on its agrarian rim. Lukacs's Budapest was a metropolitan center, to be sure, but it was not an industrial center, and, moreover, it was the only major city in Hungary and was all but drowned in the surrounding sea of peasant civilization.

Lukacs spent his formative years in a country in which the major societal forces had reached a kind of stalemate, effectively prohibiting creative innovation in political and social affairs. An essentially traditionalistic peasantry dominated by both Church and landowners seemed impermeable to novel ideas. Labor still had little weight in Hungary's pre-modern society, and its organizational endeavors in the Social Democratic party and in the unions were more concerned with bread-and-butter issues than with comprehensive reforms. The dominant aristocracy and the impoverished gentry, which did the dirty work for the nobility in the administration of the state, were committed to the maintenance of the existing state of affairs since social changes would undermine the basis of their domination. The oppressed minorities in the countryside, though suffering under the yoke of the Magyars, were isolated from one another and from the urban centers and were therefore unable to make themselves heard in the seats of power. The largely Jewish middle class was conformist to the core and dreaded any transformation that might upset its monopoly on financial and commercial affairs.

Given the stasis of the society, it is understandable that the small layer of alert and activist intellectuals who gathered in Budapest around the turn of the century came to conceive of themselves as the sole voice and conscience of the nation. Even though they were doctrinally committed to the goals of democracy, conditions almost ineluctably drove them to an elitist stance. In their interstitial existence, they came to see themselves as reformers par excellence. Since there was little or no audience for their melioristic ideas, they alone, it seemed to them, could be the source of change, if it were to come at all. In Budapest at that time, as Mannheim used to say later, intellectuals saw themselves as the self-appointed guardians of the well-understood interests of the whole society.

Most of the reform-minded members of the Budapest intelligentsia belonged in the prewar days to the Society for Social Science, a select group of progressive social scientists who saw themselves as the Hungarian counterpart of the Fabian Society. Lukacs, however, was not a member of that group. He had spent a number of years in Germany, where he had

made a name for himself as a literary critic and writer on esthetic subjects and where he had gained the friendship of both Georg Simmel and Max Weber. His philosophical orientation at the time was in the tradition of German idealism and historicism, although he had also read widely in mysticism and vitalistic philosophy. Political concerns were then alien to him. When he returned from Heidelberg to Budapest in 1915, he organized a discussion group with like-minded idealist intellectuals which was broadened in 1917 into a Free School for the Humanities, stressing German *Geisteswissenschaft* and idealistic philosophy in contradistinction to the positivism of the Society for Social Sciences. Lukacs's group was beholden to "Soul and Culture," to quote the title of a programmatic lecture given by Mannheim, and scoffed at the primitive "materialism" of Marxist thought.

When the Hungarian revolution broke out in October 1918, the intellectuals of the Society for Social Sciences were in the forefront of the Republican and mildly socialist regime of Count Karolyi. Members of the Lukacs circle seemed unconcerned. But in December 1918, Lukacs suddenly, and to the great surprise of his friends, entered the newly formed Communist party and became a central intellectual figure of the Bela Kun regime during the short-lived Soviet republic (cf. Coser 1971; and Kettler 1967).

All major Hungarian intellectuals had suffered in the prewar years from their impotence and their political as well as social isolation. Many of them had sought for a way out in a sort of a Fabian progressive politics which in fact, though not in theory, would allow them to become a guiding elite. Lukacs, however, had not been part of that movement but had instead been deeply involved in German intellectual circles that had for years stressed the desperate crisis of the European spirit in an age of progressive mechanization and bureaucratization. Hence, the mildly reformist Karolyi regime did not appeal to his romantic yearning for an entirely new beginning, a fundamental recasting of the whole of European culture. The Soviet regime, on the other hand, promised just that. It offered a cure for the torn soul and a road to salvation for the despised *Geist* of European culture. Lukacs became a Marxist out of a deep yearning for cultural renewal. He had never been attracted by humdrum Social Democratic reform activity. Bela Kun's revolution, on the other hand, allowed him to form a vision of the revitalization of European culture on barricades manned by activist workers.

Lukacs himself said almost half a century later that the doctrines he developed in the aftermath of the Hungarian revolution stemmed from certain earlier preoccupations with problems of ethics. He was attracted to syndicalism and the philosophy of Sorel but was "repelled by Social Democratic theory, above all Kautsky." But "ethics exercised pressure

in the direction of *praxis*, of action, and hence of politics" (Lukacs 1968, pp. 12–13). With the Russian Revolution, and more directly and immediately with the Hungarian Soviet experience, Lukacs became an activist Marxian. When writing his famous *History and Class Consciousness* in 1922–23, he still believed that "the great revolutionary wave, which would lead the whole world, at least all of Europe, in very short time to socialism . . . had by no means receded." Lukacs later characterized his Marxist orientation of the time as "messianic-utopian" (Lukacs 1968, pp. 16, 27). He says that he passionately hated "mechanical fatalism" and "mechanical materialism" and that his whole thinking was predicated on the notion of revolutionary activity.

Lukacs now idealized the Leninist party as the incarnation of revolutionary voluntarism, as living praxis. The Hegelian *Weltgeist* seemed to him to be embodied in the wisdom of the Communist party. The party could not possibly be wrong. The class consciousness which Lukacs extolled was by no means an empirically ascertainable orientation of concrete workers. It was the "objective consciousness" that was imputed to the proletariat and incarnated in the Communist party. Lukacs argued that it was typical of opportunism to confound "the factual psychological state of consciousness of the proletariat with the class consciousness of the proletariat" (Lukacs 1968, p. 249). The "objective theory of class consciousness," he argued, "is the theory of its objective possibility" (Lukacs 1968, p. 255). And that objective possibility found its adequate embodiment in the omniscient Communist vanguard equipped with a "scientific" understanding of the course of history.

To Lukacs, the proletariat was the potentially revolutionary class par excellence. But potentiality would become reality only if the party would point the correct way through the direction of revolutionary practice. The true subject of history was hence the party, while the masses were only its object as long as they were not energized by the revolutionary will and the scientific praxis of the party. The actual working class, sunk in humdrum existence and nonauthenticity, did not interest Lukacs at all. He was transfixed by what it *could become*. When Caliban absorbed the *Geist* incarnated in the Communist party, he would redeem a fallen world and would make it whole again.

It serves no purpose here to follow Lukacs's later descent into Stalinist conformity and his subsequent recantation of the book on which his reputation as a political thinker inevitably rests. Nor can we deal with his subsequent literary criticism. What concerns us here is exclusively the attempt by a marginal intellectual from the rimland of Europe to develop a voluntaristic Marxism even more extreme than that of Lenin. Lukacs's and Gramsci's attacks against Kautskyan evolutionary orthodoxy were the last major attempts within the period under discussion to develop the

voluntaristic and activistic strains that were inherent in the early writings of Marx and Engels.²

SUMMARY

This analysis of the major current of Marxist thought in the first quarter of the 20th century focused on the differential interpretation of the teaching of Marx by the main theorists of orthodox Marxism and revisionism in Germany, and by a variety of Marxist revolutionary thinkers from the East, from Italy, and from Hungary. It has been shown that the German Marxists, whether orthodox or revisionist, were "insiders" expressing the desires and hopes of a highly organized working class in a strongly industrialized country. Their evolutionary optimism reflected a movement which saw itself engaged in a confident march toward democratic power and socialist ascendancy. By contrast, the revolutionaries from Russia and Poland who were active in the German movement were "outsiders" who reacted in terms of their different experiences in the East and thought that revolutionary activism would push the spontaneous struggle of the German workers toward an apocalyptic "final struggle" with the forces of capitalist domination. It turned out that they misjudged the character of the work-

² I originally intended to devote a few pages to the analysis of Georges Sorel's work. His *Reflections on Violence* seem to belong to and indeed anticipate later voluntaristic interpretations of Marxism. I have decided against such discussion, however, since Sorel did not have any impact within the Marxist movement. This was so, not only because a few years after writing his book (1908) he abandoned the Left altogether and endeavored to find a new audience among the conservative Right, but also because his call for energizing myths that would move the working class to action was so far removed from contemporary Marxist thinking of whatever specific coloring that it could not possibly be assimilated by anyone still considering himself a Marxist. Men like Robert Michels or Georg Lukacs might at times read him with a measure of sympathy, but, in the Marxist movement as a whole, the odor of heresy clung to Sorel's name.

It might suffice, hence, to suggest that the extreme voluntarism of this provincial from Cherbourg who had studied at the Ecole polytechnique and for many years pursued a conventional career as an engineer in the road-building services of the government, might best be explained by reference to the specific conditions of France around the turn of the century. France was, of course, one of the leading countries in Europe, but its industry and hence its labor movement was still in many ways more similar to that of Europe in the middle of the century than to conditions in contemporary Germany. Large-scale industry was still rare. Much production was still carried out in enterprises that had not fully shed their artisanal character. The working class was weakened from the bloodletting of the Commune and its trade unions were small, even though militant. Syndicalism, the movement to which Sorel attached himself temporarily, never managed to organize the majority of workers, as the unions of Germany, England, or the Lowlands had managed to do. Given these conditions, it would seem that Sorel's thought, insofar as it expressed real tendencies in the labor movement, if it expressed them at all, was a reflection of its fundamental organizational weakness. In this respect, although obviously not in others, Sorel's voluntarism had much in common with the other varieties that have been discussed here (cf. Ridley 1970).

ing class in a maturing industrial society. They found some acceptance in Germany to the extent that *political* revolution seemed to the orthodox to still be necessary, but they were rejected when they advocated *social* revolution.

In Russia itself, failure of the as yet weak working class to listen to the revolutionary message of the Social Democratic intelligentsia led to the historic split between Bolsheviks and Mensheviks. The former rejected the spontaneous movements of the workers and opted for a voluntaristic activism by professional revolutionaries who were to impose their will on the recalcitrant masses. The latter, though more activist than their Western counterparts, trusted in the main the evolutionary developments within Russia and hence were fundamentally at one with the evolutionary schemes of orthodox Kautskyan Marxism.

Finally, I analyzed in the case of Gramsci and Lukacs, other types of voluntaristic Marxist thought, nurtured like their Leninist prototype in backward areas by embittered revolutionary intellectuals in a hurry.

In conclusion, it may be stated that the revolutionary and activist Marxism first developed by Marx and Engels in the late 1840s found an echo among 20th century intellectuals in those areas of the rimland of Europe which still resembled in essential respects the conditions prevailing in the heartland of Europe half a century earlier. In the main centers of the European working class, on the other hand, positivistic evolutionary Marxism, adumbrated by Engels in the last period of his life and further developed by Kautsky, carried the day and was later largely replaced by Fabian or Bernsteinian gradualism. Social existence determined social consciousness. The receptivity to variant Marxist doctrine was largely conditioned by the values and attitudes of the men and women whose concrete social and historical existence was mirrored in their world view, whether as producers or as consumers of ideas.

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Epilogue: To Be a Phoenix—Reflections on Two Noisy Ages of Prose

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I

Heroic ages of poetry are often followed by hesitant ages of prose, literary creativity by literary criticism, and the doing of sociology by the sociology of sociology. At least as I see it, the central thrust of this volume of the *American Journal of Sociology*, largely concerned as it is with the sociology of sociology, nicely reflects our hesitant age of prose. The poetic core of sociology, I like to think on the other hand, lies in the doing tradition of the great problematic monographs like *Le suicide*, *L'ancien régime*, *The Protestant Ethic*, *The Polish Peasant*, *The Gang*, *Yankee City*, *Deep South*, and *Middletown*, *White Collar*, *The Lonely Crowd*, *Black Bourgeoisie*, *Union Democracy*, *Asylums*, and so forth. Merton's *Science, Technology and Society in Seventeenth-Century England* is very much a part of this poetic tradition. Its review by Benjamin Nelson, more than three decades after its original publication, may well be the most symbolically important thing about this volume.

All civilizations, at one unfortunate time or another, have been pushed to the polar, *lawless* extremes of autocracy or anarchy. If autocracy is the anarchy of lawless, lonely tyrants, anarchy is the tyranny of lawless, lonely crowds. It is interesting that such a timeless and poetic work as Solzhenitsyn's *First Circle* was produced in autocratic Moscow, while the timely prose of Mailer's *Advertisements for Myself* seems such a telling symptom of anarchic New York. Perhaps the silence of autocracy is less of an obstacle to real poetic genius than the infernal noise of anarchy. In our own discipline, for example, the major monographs of Erving Goffman, poetic doer par excellence, were all done before the noise began. On the other hand, it is probably inevitable that sociologists, at the end of the noisiest decade in modern history, should be preoccupied with the sociology of sociology (see the recent books by Friedrichs (1970), Gouldner (1970), and Tiryakian (1971)). Thus the relevant problem today, so they say, is *not* the doing of sociology but rather *how* it should be done, for *whom* (adults or children, boozers, or potheads!), and by persons of *what* theoretical, political, sexual, racial, class, or sartorial persuasions. But surely, as even Becker and Horowitz admit, it is "good sociology" which will last, regardless of what contemporaries think of the theoretical, political, or social

positions of the doers. When the doing of political sociology, as for example Lipset's *Political Man*, gives way to the concentration on the politics of sociological theorists (as shown in Lipset and Ladd), the discipline is betraying its best and most fruitful traditions. When my own graduate students, so often the less secure and talented among them, are noisily engaged in arguing the pros and cons of the Tumin criticisms of the "iniquitous" Davis and Moore thesis, I urge them instead to take some problem, preferably dear to their own hearts or personal experiences, and to go to work in the hopes of adding something, however great or small, to the great doing tradition, from Durkheim to Goffman.

When asked by the editor for my "reactions" to these articles, I hesitated to interrupt my own work on the problem of the rise of Quakerism in Puritan England. I accepted the invitation, however, because I was convinced that an understanding of this crucial period in English history had something to tell us about our own age.¹ During the anarchical and violent years of the Puritan Revolution, for example, John Milton shelved his poetic genius and produced a series of polemical and relevant pamphlets, including his famous defense of freedom of the press and a less famous justification of regicide; he survived the Restoration of 1660, though he was arrested as an Enemy of the State and two of his pamphlets were burned by the public hangman, only because of his friends at court; and he did not publish *Paradise Lost* until 1667, a year before his sixtieth birthday. Milton witnessed the rise and agonizing death of Puritanism which a modern historian, Alan Simpson, has briefly outlined as follows: "The origins of English Puritanism are to be found among the Protestant reformers of the mid-sixteenth century; it takes shape in the reign of Elizabeth; produces thrust after thrust of energy in the seventeenth century, until the final thrust throws up the Quakers; and then ebbs away."

Alvin Gouldner's brilliant book, which is the subject of two of the essays in this collection, as well as the articles by Janowitz, Merton, and Becker and Horowitz, are all very much a product of our anarchic age and the consequent "balcanization of social science," as Merton puts it. In other words, as I see it, they are a reflection of sociology's attempt to cope with and understand the agonizing death of liberalism, about which some future, intellectual historian may write as follows: "The origins of Western Liberalism are to be found among the Utilitarian reformers of the mid-19th century; it takes shape in the Victorian age; produces thrust after thrust in America during the Progressive, New Freedom, New Deal, and New

¹ According to the Oxford dictionary, the term "anarchism" came into the English language in 1642, the year the English Revolution broke out ("antinomianism," the theological counterpart of our own sociological jargon-term, "anomie," was introduced in 1645).

Frontier years of the 20th century, until the final thrust throws up the New Left and a host of other secular-sectarian movements; and then, like the Puritans of another day, ebbs away."

Now the content and the ideas of 17th-century Puritanism were hardly similar to those of 20th-century liberalism; for one thing the rhetoric of one was religious, the other secular. But 17th-century Puritans and modern liberals, on the other hand, played functionally equivalent roles, as reformers and advocates of change, in their respective societies. As Weber, Tawney, Merton, and others have shown, Puritans took the lead in science, law, medicine, manufacturing, and in military tactics. Their political leaders were largely members of the country gentry whose power lay in the House of Commons. These gentlemen reformers, allied with city merchants and lawyers, were seeking to enlarge their power and authority as against the establishment of Church, Lords, and Monarchy. Oliver Cromwell, for example, was a prominent member of this gentry class. He was born in the last years of Elizabeth's reign, in 1599, to the poorer branch of a wealthy and prominent family in the county of Huntington, near the Puritan stronghold of Cambridge. Before going to Parliament in 1628, he had spent a year at Sidney Sussex, a Puritan college at Cambridge, and then settled down as a gentleman farmer and leader in local county affairs. In social background and in politics, then, he was not unlike the gentlemen reformers who followed the leadership of Theodore and Franklin Roosevelt, both of whom challenged the established power and authority of big business in 20th-century America. And this 17th-century English gentleman would have been quite at home among the Roosevelts at Oyster Bay or Hyde Park. Just as the two Roosevelts were essentially Victorians in education, background, and temperament, so the leading men of the great Puritan generation were essentially Elizabethans and men of order, who nevertheless believed in change, the continual reform of the inevitable abuses of authority, and the steady equalizing of opportunity. They *never*, however, advocated the abolition of authority or the possibility, or desirability, of complete equality of conditions; they were reformers but not radicals, equalizers but certainly not egalitarians.

For approximately a century, Puritan and liberal reformers were successful in backing more or less orderly and evolutionary change in their respective eras. Then suddenly and in times of seeming victory, reform turned upon itself and died in a period of radicalism and anarchy. Among other things, there was a generation problem. Thus John Winthrop, who brought Puritanism to New England, was born in the Armada year of 1588, while William Penn, founder of Quaker Pennsylvania, was born the year of the great parliamentary victory at Marston Moor (1644). Winthrop was born the same year as Hobbes, while Penn was a friend of Locke. To put it another way, Winthrop and Penn were born as far apart in time and

societal values as Franklin Roosevelt was from the younger generation of the sixties.

At any rate, in England both the reformers and the radicals eventually lost out at the Restoration of 1660. What will happen here nobody knows. Marx, following Hegel, saw historical patterns repeating themselves at least twice, first as tragedy and second as farce. There is a haunting similarity between the pattern of anarchy which followed the execution of England's king, in January 1649, and the years in America since the assassination of our president in November 1963.² In the interest of avoiding farce, I should like to take a look at the first tragedy. Though I must be brief, I shall try to include enough concrete detail to make the historical parallels meaningful and more than mere abstractions.

II

The 1630s in England were much like the Eisenhower years in America. Society went through a period of confident calm under the benevolent authority of Charles I. As a contemporary poet wrote, with smugness and pride:

Tourneys, masques, theatres better become
Our Halcyon days; what though German drums
Bellow for freedom and revenge, the noise
Concerns us not, nor should divert our joys.

But the placid thirties were followed by the revolutionary forties and anarchical fifties, when Englishmen, for the first and last time in their history, fought each other to the bloody and bitter end, abolishing bishops and lords, beheading their king, and eventually setting up a kind of military dictatorship in the face of increasing anarchy.

This is no place to go into the causes of the Civil War. But it is safe to say, I think, that war broke out in 1642, not because the leaders on either side wanted it, but rather because, as with our own involvement in Vietnam today, they failed to see the collective and cumulative consequences of their individual actions.

² There are, according to Webster, at least two meanings of the term anarchy: (1) a state of political disorder, and (2) a state of confusion or disorder. In the British, French, and Russian revolutions, the states were overthrown, creating political as well as societal disorder (1 and 2). The 20th-century welfare state, on the other hand, is highly centralized and strong: thus the anarchy of our sixties in America was largely societal confusion and normlessness (2) rather than political anarchy (1). Anarchy, chaos, and lawlessness, according to Webster, are synonyms: anarchy implies absence or suspension of government; chaos, the utter negation of law and order; lawlessness, a prevalent or habitual disregard for law and order. I use the term "anarchy" here more in the sense of chaos or lawlessness than in simple political anarchy. In many ways, the theological term "antinomian" and the sociological term "anomie" might better serve my purpose than "anarchy."

The vast majority of leaders of both parties, for instance, believed in a monarchical form of government and an established church. When the members of the Long Parliament passed the Grand Remonstrance, a mild-mannered listing of the abuses of monarchical and Episcopal authority, they included the following statement of their belief in religious uniformity and authority: "We do here declare that it is far from our purpose or desire to let loose the golden reigns of discipline and government in the Church, to leave private persons of particular congregations to take up what form of divine service they please, for we hold it requisite that there should be through-out the whole realm a conformity to that order which the laws enjoin according to the word of God." The parliamentary army, moreover, was raised "for the safety of the King's person, defence of both Houses of Parliament, the preservation of the true religion, the laws, liberty and peace of the kingdom." The commissions of its officers, moreover, ran "in the name of King and Parliament."

The Long Parliament was certainly no revolutionary body. Its members were essentially a conservative cousinhood of knights, squires, and gentlemen from the counties, along with some city lawyers and wealthy merchants. When young Cromwell first went to Parliament in 1628, for instance, he found nine cousins there; at the opening of the Long Parliament, he sat with 11 cousins, including John Hampden, then the richest man in England; six more cousins and three other relatives joined him there later on. In addition to the gentry-merchant cousinage, no less than 48 members were sons of peers. That the extremely mild Grand Remonstrance was passed by a slim majority of only 11 votes out of 307 members present attested to the conservative and conciliatory mood as of December 1641. Unfortunately, Charles rejected the mild criticism and impeached six members for treason (including Hampden, one of the most widely respected men of his day). It was the slender straw that broke the camel's back, and war broke out at the Battle of Edgehill. The Parliamentary party eventually won a military victory due, among other things, to the efficiency and leadership of the New Model Army.

The New Model was something new in English history. In the Puritan tradition of the calling, Cromwell had a great sense of professional pride. In striking contrast to the aristocratic ideal, he organized the New Model on the basis of merit rather than status: "The officers," wrote his more conservative contemporary, Clarendon, "are of no better family than the common soldiers." The New Model not only provided for careers open to talents; it was also an ideological army made up of voluntary, true believers in the righteousness of the Holy Crusade. Unfortunately it was this Cromwellian tradition, transplanted to America, which has produced such moral disillusionment after each of our own military crusades in the 20th century, from the days of Wilson and Roosevelt through Kennedy and Johnson. All

this was, of course, quite in contrast to the aristocratic ideal of warfare as a sport, with rules protecting priests, women, and children, and fought for limited interests rather than ideological abstractions.

As was to be expected with this new ideological army, there soon developed, from colonel to common soldier, a belief in the right of self-expression, or a kind of participatory democracy. We often forget the revolutionary potential of all standing armies, of which the New Model was the first in English history. Down to 1640, for example, most Englishmen were extremely provincial. Even after years of Tudor and Stuart attempts at centralization, there was little sense of nationhood; one's *country* meant one's *county*, where everyone was bound together by local loyalties of family, lord and manor, vicar and village, all reinforcing the established customs of degree, priority, and place. Now, for the first time, large numbers of Englishmen were away from home and easily influenced by the sway of opinion so characteristic of the all-male and atomized life in the ranks.

And the men of the New Model were exposed to all kinds of new enthusiasms. According to George Sabine, the great political scientist and biographer of the "Digger" leader, Gerrard Winstanley, the debates and heated discussions which took place around the army campfires marked the first appearance of public opinion as a factor in British politics. These debates, mirroring and reinforcing those of revolutionary society as a whole, were part of the greatest outpouring of pamphleteering in British history (a fine collection of the pamphlets in the British Museum includes some 20,000 titles, obviously only part of the whole). In order to organize and regulate the expression of opinion, there was set up a Council of the Army, representing the officers, and a Council of Agitators, representing the ranks. As Clarendon, the first great historian of the Rebellion and a contemporary witness, wrote, the army had "set itself up as a rival parliament with the agitators as the house of commons and the officers as peers."

The most famous of the army debates took place in the little village church at Putney, in the autumn of 1647, after military victory in the Civil War had been won. Although the Leveller leader, John Lilburne, was imprisoned in the Tower at the time, the debates centered around his draft constitution, called *Agreement of the People*, which was concerned mainly with the franchise. This was the first time in English history that the possibility of manhood suffrage was taken seriously: in the now-famous words of Colonel Rainsborough (kin by marriage to John Winthrop of New England), "The poorest he that is in England has a life to live as the greatest he, and therefore . . . every man that is to live under a government ought first by his own consent to put himself under that government." The Putney debates dragged on through October, many of the generals and colonels being horrified at the extreme views of the agitators with all their leveling ideas. Finally in November, Cromwell terminated the debates and

ordered the men back to their regiments, shooting one agitator on the spot for insubordination.

Events now moved steadily to the left, toward the logical conclusion of the revolution: execution of the king. During the course of the next year (1648) the generals revived their alliance with the Levellers and proceeded to occupy London. In December, Colonel Pride forcefully expelled 96 conservative Presbyterians from Parliament. This left only some 60 more radical members, called the Rump, who abolished the House of Lords and brought Charles before the High Court of Justice, which sent him to the scaffold on January 30, 1649. "All is quiet," wrote John Winthrop's son, Stephen, from London, in the hush which followed the execution of the king: "All is quiet, but I know not how long it will last. . . . New England seems the only safe place."

Stephen Winthrop's premonitions of strife were justified. With the final abolition of all traditional authorities, only anarchy or the sword remained. Even the perpetual opponent of authority, John Lilburne, was horrified. He denied the lawfulness of both Pride's purge and the king's trial: "To have the name of Commonwealth imposed upon us by the Sword," he told Cromwell at the time, "wherein we are and shall be more slaves than ever we were under kingship. . . . and therefore I had rather be under a King reasonable bound than under you, and your new Sword Tyranny called Common-wealth."

As long as history had recorded, authority in England had been symbolized in the now-abolished orders of kings, lords, and bishops. The regicide had so uniquely shocked the people that, according to a contemporary witness, "Women miscarried, men fell into melancholy, some with consternation expired." The great physician, William Harvey, told a bishop at the time that he met with "more diseases generated by the mind than from any other cause." All indeed was now in doubt, all coherence gone, and a host of "seekers" began to populate the melancholy land of England, very much in the style of the lonely and lost souls of our own day who once gathered at Woodstock and are now joining the Jesus cults. Clarendon was horrified at the decline in traditional authority: "All relations," he wrote, "were confounded by the several Sects in Religion, which discountenanced all Forms of Reverence and Respect. . . . Parents had no Manner of Authority over their Children, nor Children any Obedience or Submission to their Parents; but everyone did that which was good in their own Eyes." Quite naturally Clarendon saw the extreme egalitarianism abroad in the land as a reversion to primitivism: "In all well instituted governments," he wrote, "the heirs and descendants from worthy and eminent parents, if they do not degenerate from their virtue, have always been allowed a preference and kind of title to employments and offices of honour and trust. . . . Whatever is of Civility and good Manners, all that is Art and

Beauty, or of real and solid Wealth in the World, is the . . . child of beloved Propriety; and they who would strangle this Issue, desire to demolish all Buildings, eradicate all Plantations, to make the Earth barren, and Mankind to live again in Tents, and nourish his Cattle where the grass grows. Nothing but the joy in Propriety reduc'd us from barbarity; and nothing but security in the same, can preserve us from returning into it again." Clarendon's view of the barbaric consequences of the proposed "greening" of his England would also apply to the proposals of our academic seekers for the "Greening of America."

All revolts against the inevitable hypocrisies of established authority have called for the return to the simplicities of naked and nonliterate primitivisms. Thus many of the early founders of Quakerism went naked through marketplaces, and even into churches, as a witness of their martyrdom to the "naked truth." Even as late as 1672, the one and only great Quaker theologian, Robert Barkley, went "naked as a sign through the chilly streets of Aberdeen." George Fox and other leaders of the Quaker movement preached the futility of "mere book knowledge" which only tended to pervert the purity of the untainted prompting of the "inner light." Gerrard Winstanley, in his relevant pamphlet *The New Law of Righteousness* (1649), wrote that "the Universities are the standing ponds of stinking waters." Even Emanuel College, at Cambridge, once the seat of Puritan intellectualism and the founding seed of Harvard College, went through a period of extreme mysticism during this period. It is no wonder that at both Oxford and Cambridge there was the smashing of stained-glass windows and the decimation of altar decorations, all in the name of Puritan and sectarian relevance.

Among a host of other parallels of our own noisy years, there was, finally, the digging up and replanting of the Common land on St. George's Hill, in Surrey, by the followers of Winstanley, a once-religious mystic turned utopian "communist." The Diggers, in appealing their arrest to the House of Commons, demanded, much in the manner of their modern imitators, "whether the common people shall have the quiet enjoyment of the Commons, or Waste Land, or whether they shall be under the will of the Lords of the Manor." In that anarchical regicide year, similar diggings took place in Buckinghamshire, Middlesex, Hertfordshire, and Berkshire, altogether in some 34 towns. The following lines, attributed to Winstanley, would have appealed to the modern "Diggers" of the People's Park at Berkeley:

The gentrye are all round, on each side they are found,
Theire wisdom's so profound, to cheat us of our ground. . .
The clergy they come in, and say it is a sin
That we should now begin, our freedom for to win.

Stand up now, Diggers all.
To conquer them by love . . .

E. Digby Baltzell

To conquer them by love, as it does you behove,
For he is King above; no power is like to love:
Glory here, Diggers all.

Viable civilizations, are, almost literally, *clothed* in authority; and when the emperor's clothes are removed his only recourse is the exercise of *naked* power. The Diggers dug up St. George's Hill in April, and in May mutinies broke out in the army which soon turned into a full-scale Leveller revolt. Parliament immediately declared mutiny in the army to be treason, and Cromwell led a lightning night attack on the rebels at Buford; three leaders were shot on the spot and a fourth was caught and shot three days later. Cromwell and Fairfax now returned to Oxford, Royalist stronghold in Charles's last days, where they were feasted by the conservative city fathers. After Buford, the Revolution turned conservative.

As part of the reactionary trend, John Lilburne was brought to trial for sedition in October. If Cromwell was respected, admired, and feared, Lilburne was undoubtedly the most popular man in England at this time. When he was finally acquitted by a jury of his peers, the people shouted with joy. A cheering mob followed him all the way from the Guild Hall to the Tower, where, in spite of his recent acquittal, and another in 1652, he was to remain until the end of his life. In the meantime, Lilburne lost all hope of secular solutions and became a Quaker, "renouncing all weapons except the Sword of the Spirit." When he died in 1657, his funeral was held at the Bull and Mouth Meeting House, in London; some 4,000 Quakers accompanied his body to the burial grounds.

Lilburne, who had spent almost two-thirds of his life in jail and was in perpetual revolt against authority, whether monarchical, Episcopal, Presbyterian, or Cromwellian, was an ideal spiritual ancestor of a sect of martyrs. The great Quaker preacher and historian, Rufus Jones, considered him, along with Winstanley, a noble ancestor of both modern democracy and Quakerism. Thus it was that "the Quakers passed on the Leveller torch to the New World."

When formalism declines fanaticism comes to the fore, and the church is replaced by a host of sects. Just as we are witnessing the spread of fanatical sectarianism in America today, so the Puritan Interregnum was faced with the rise of the Ethringtonians, Grindlestonians, Mugglestonians, Fifth Monarchy Men, Family of Love, Ranters, and a veritable swarm of other seeking sectarians. It is always the ex-radicals who hate and hunt the newer radicals. Thus the Reverend Thomas Edwards, who back in 1628 had been dismissed from his post as University Preacher at Cambridge for his violent attacks on the establishment of bishops, now turned around and attacked, in even more fanatical terms, the fanaticisms and heresies of the 199 sects which he now found abroad in the land. Many of the new sectari-

ans and seekers joined the Quaker movement, which was unquestionably the most interesting and important of them all.

In striking contrast to the university educated, and theologically sophisticated, builders of Puritanism, the founder of Quakerism, George Fox, was not unlike that "rough beast slouching towards Bethlehem" which Yeats saw as the only possible savior of our own seeking and anarchic generation. Born the son of a humble weaver in a small village in the North Country—the most backward, feudal, royalist, and Catholic part of England—Fox was almost entirely self-educated and a charismatic mystic whom many have called the only religious genius of the English Reformation. A year after the outbreak of the Civil War, young Fox, at the age of 19, left home like so many of today's soul-hungry seekers of the counter-culture, and wandered through the countryside, dressed in leather breeches, sleeping in haystacks, under hedges, and even, as legend has it, in the trunks of hollow trees. He was desperately seeking for some new vision of life outside the establishment of hated "steeplehouses" and their hypocritical "professors." After four years of lonely seeking, five years of preaching to other seekers (and two periods in prison), he came to Pendle Hill, famous in local folklore as the haunt of witches and warlocks, where he had a vision of "a great people to be gathered." Modern Quakers have always dated the founding of their sect with this vision on Pendle Hill, in the spring of 1652.

Just as the hated Anabaptists rose to haunt the reformers and Lutherans during the 15th-century Reformation on the Continent, so the Quakers marked the extreme left wing of the English Reformation. "The Society of Friends," wrote Ernst Troeltsch, "represents the final expression in its purest form of the Anabaptist Movement." After a century of reform, religious authority in England—once lodged in the pope and the priestly control of the seven sacraments, then in the Anglican bishops and the priestly control of two sacraments, and finally in local preachers interpreting the Bible—was suddenly transformed by the radically individualistic and subjective doctrine of the "inner-light." The hierarchical yet reforming ethic of the Puritans now gave way to the radical egalitarianism of the Quakers: "When the Lord sent me into the world," so Fox says in one of the key passages of his *Journal*, "He forbade me to put off my hat to any, high or low: and I was required to 'thee' and 'thou' all men and women, without any respect to rich or poor, great or small." While the Puritans were closer to Hobbes, the Quakers anticipated the tradition of Rousseau.

This is no time to go into a detailed discussion of the differences between the Puritan and Quaker ethics. The following skeleton list of the differences (in emphasis, of course) should, however, show how similar they were to our present conflicts between culture and counterculture.

E. Digby Baltzell

| Puritan Ethic | Quaker Ethic |
|---|---|
| God transcendent (man anxious to prove himself) | God imminent (peace of mind) |
| Old Testament (Decalogue) | New Testament (Sermon on Mount) |
| Head (learning) | Heart (feeling) |
| Law | Love |
| Danger: legalism and rationalism | Danger: anarchy and mysticism |
| Elitism (elect of Saints) | Egalitarianism (that of God in every man) |
| Institution building | Anti-institutional (spontaneity) |
| Aristocratic (antimonarchical) | Democratic (anti-all hierarchy) |
| Representative democracy (majority 51% rule) | Direct democracy (sense of meeting, like "general will" of Rousseau) |
| Calling (great professional pride in ministry and magistracy) | Calling (more like Thomism: to God rather than profession; exaltation of laymen and amateurs) |
| Ideal man: magistrate | Ideal person: martyr |
| Evil: in sinful man | Evil: in corrupt institutions |
| Major vice: arrogance | Major vice: self-righteousness |

"Anyone who knows anything about history," Marx once wrote to a friend, "knows that great social changes are impossible without the feminine ferment." It is happening today as it did in the 17th century, as a vogueish ditty of that day put it:

We will not be wives
And tie up our lives
To villainous slavery.

As might be expected, women played a major role in the Quaker movement. In fact, Clarendon thought the Quakers were a female sect, as well he might have: Fox's first convert was Elizabeth Hooton, of whom more later; his most important early convert, later his wife and mother of the movement, was Margeret Fell, mistress of Swarthmoor Hall which became the movement's headquarters; the first Quaker "publishers of truth" in London, in the universities, and in Dublin, were women; Mary Fisher, a young Yorkshire house servant, was the first Quaker in America; even in England today more women than men are registered as ministers.

The Quakers of the first generation were a hardy, fanatical, and apocalyptic band of martyrs who were hated, hunted down, and hung, imprisoned, and tortured for their convictions. Puritan England was no permissive age. That the female "Friends of Truth" had a particular affinity for fanaticism and martyrdom was illustrated in the career of Fox's first convert, Elizabeth Hooton. A woman of good position, 47 years old

and the mother of seven children when she first met Fox in 1646, Elizabeth took up the active ministry in 1650 and almost immediately went to prison, first at Derby then at York Castle (16 months). After continuing her ministry in England, where she was imprisoned six months in 1654 and three months in 1655, she set sail for America in 1657, but was soon shipped back by the Boston authorities (after they had nearly starved her in the wilderness). At the age of 65 she went back to New England where, according to her *Journal*, she suffered fantastic hardships including four days in prison without bread or water and being whipped "for a wandering vagabond Quaker at three towns, ten stripes at whipping post in Cambridge, and ten at Watertown and ten stripes at Dedham at the cart's tail with a three cord whip three knots at the end, and a handful of willow rods at Watertown on a cold frosty morning. Then they put me on a horse and carried me into the wilderness many miles, where was many wild beasts both bears and wolves . . . but the Lord delivered me." Before leaving New England, she was sent to prison for attending, and probably preaching at, the funeral of Governor Endicott, the great opponent of the Quakers who had previously ordered the hanging of Mary Dyer on Boston Commons, where her statue now stands. Elizabeth Hooton's last service to the cause was when she went with George Fox, as one of his "twelve" companions, on her third trip to America, where she died in Jamaica at the age of 73.

Like Elizabeth Hooton, hundreds of Quakers in the first generation were martyrs for their cause. In England, some 3,000 were sent to prison under Cromwell (20 died there) and over 15,000 during the Restoration (300 deaths). Almost every leader, from George Fox and Margeret Fell to Robert Barclay and William Penn, went to prison, usually more than once. But of course the martyr thrives on, and often seeks out punishment and persecution, and the movement spread rapidly throughout England, Ireland, and Wales, and thence to the New World. By 1700 there were over 40,000 Quakers in the American and Caribbean colonies and 50,000 at home in Britain. The evangelical zeal of the first generation eventually spent itself and the movement became a very solid, bourgeois sect which reached its numerical peak in the middle of the 18th century. The miracle of the Quakers is that they have survived both persecution and prosperity.

III

C. Wright Mills once wrote that we sociologists must "try to understand men and women as historical and social actors." My reaction to this volume of the *AJS* is to place it within some historical context. The two excellent articles by Coser and Dibble, for instance, might have appeared in this *Journal* a decade or two ago. On the other hand, as I have said above, the Gouldner book and the comments on it here, as well as Merton's concept

of the "Insider," are all infinitely relevant to the Oz-like world which Becker and Horowitz describe in the following lines at the beginning of their article:

Greater sensitivity to the undemocratic character of ordinary institutions and relationships (ironically fostered by social scientists themselves) has revealed how research frequently represents the interests of adults and teachers instead of those of children and students; of men instead of women; of the white middle class instead of the lower class, blacks, chicanos, and other minorities; of the conventional straight world instead of freaks; of boozers instead of potheads. . . . Younger men have debated whether it was moral to be affiliated with the sociological enterprise. Older sociologists have searched their work and their consciences to see if, far from being the political liberals they imagined themselves, they were in fact lackeys of capitalist repression.

In the midst of these reconsiderations, positions hardened. The language of scholarly journals became increasingly polemical. Meetings thought to be scientific were disrupted by political protest and discussion. Presidential addresses at national and regional meetings were interrupted. . . . Some teachers found themselves unable to bear the discourtesies of their radical students. Some professors saw attempts to change the hierarchical relations of a department as an attack on the very idea of scholarship. They assumed that a student who called their ideas "bullshit" was attacking rational thought.

Once again the "Church" has disintegrated and a host of self-righteous sectarians are loosed upon the world. In this climate of opinion, it is understandable that the anti-institutional, egalitarian, perfectionist, and mystical ideals of the Quakers are now more popular in America, especially among intellectuals and academics, than at any other time in our history. Since the close of the Second World War, for example, there has been both a reversal of the downward trend in numbers and a very real renaissance within the Society, such as the winning of the Nobel Peace Prize by the American Friends Service Committee.

The Quaker ranks have been swelled by all sorts of refugees from the institutional "Church" of their ancestors. In a recent, excellent study of the American Friends Service Committee by a *New Yorker* writer, for instance, it was interesting to observe that a majority of the leaders of various projects he mentioned were "convinced" rather than "birthright" Friends. As the rabbi put it: "some of my best Jews are Friends."

Most of the new Quaker meetings which have sprung up around the country since the war have been formed, often by academics, in and around college or university communities. Today, for instance, the largest meeting in Massachusetts is located in Cambridge, right off fashionable Brattle Street. Only a few years ago, moreover, several departments in the humanities and the social sciences (including Parsons's own department)

had chairmen who were "convinced" Friends and members of the Cambridge Meeting. As the Cambridge meetings are unstructured, permissive, and antiauthoritarian in the extreme, the sober and square families, in recent years, apparently avoid the freaks from the Yard by attending the earlier of the First Day meetings.

It is indeed part of the irony of this age of the absurd that the quiet Quakers have really come into their own once again in America during the 1960s, the decade of noise. But when martyrs of all kinds are abroad in the land, it is quite appropriate that the Quakers, by now having institutionalized their perfectionist and pacifist ideals, should be drawn into the very heart of the various protests and antiwar movements.³ Three centuries after George Fox personally appealed to Cromwell to lay down his "carnal weapons," his spiritual descendants were "visiting" with leaders in Hanoi and Washington, "treating" with them to do the same.

It was indeed a fitting forecast of the shape of things to come that, when he died in March 1962, the great disestablishmentarian guru, C. Wright Mills, the John Lilburne of modern social science, was buried after a Quaker memorial service. He, unlike Lilburne, died "unconvinced" and an atheist, or as Mills himself would have more dramatically put it, a Pagan. It is fitting, too, that virtually before the ink was dry on Mills's death certificate, to put it figuratively, Professor Horowitz should have published a collection of Mills's essays, in the introduction to which he called Mills the "singular intellectual 'hero' of our age." And Horowitz went on to say that Mills, in the good tradition of George Fox, "was singularly unimpressed with titles, honors, degrees, positions and the entire world of inherited feudal values [?] that have been mysteriously grafted on to present-day status-conscious America." Now I should have thought that men and women have been "status conscious" ever since they put on clothes after their emergence forever (except in the minds of perfectionist sectarians) from the Garden of Eden. In point of historical fact, men in feudal times were far more interested in the state of their souls than in their secular status, which, being fixed by fate, produced little of the status anxiety which so marks our own age. But it must be said that in my day at Columbia, when ambitious graduate students still looked to Brooks Brothers rather than the local Army and Navy Store for their sartorial standards, Professor Mills was a prophet in life-styles, as well as in sociology, as he roared up to

³ Even Quaker ideals do not operate in a vacuum. Thus during the First World War, Haverford College let one of its spirited young faculty members go when he allowed as how perhaps the German people were not all representatives of Satan. He went on to Harvard, where he eventually became Hollis Professor of Divinity and a great New Testament scholar. Hollis was a wealthy English Baptist, which did not prevent Cotton Mather from putting his loyalty to Harvard above ideology in influencing Hollis to endow this oldest chair in America.

Fayerweather Hall on a motorcycle, clothed more often than not in the style now cultivated, as a badge of baptism, by the followers of Professor Gouldner.

Gouldner, like Mills, is very much in the anti-institutional and egalitarian tradition of the 17th-century sectarians. Thus a great deal of space in his prophetic book is devoted to denouncing the sociological establishment, now dominated, so he says, by a priestly caste of Parsonian functionalists. Its prophetic tone can be summed up, I think, in his antinomian concept of "authenticity" which he wants to substitute for the priestly concept of "legitimacy." In the 17th century, John Donne, great metaphysical poet and famous preacher at St. Paul's Cathedral, put it this way:

The new philosophy calls all in doubt;
'Tis all in pieces, all coherence gone;
All just supply and all relation.
Prince, subject, father, son, are things forgot,
For every man alone thinks he has got
To be a Phoenix, and that he can be
None of that kind of which he is but he.

A society of Phoenixes would surely be a sociological monstrosity, though a few "authentic" geniuses are always needed, in every generation, to change the meaning of "legitimacy." What the egalitarian perfectionists will not face is the moral ambiguity of all institutional life; "authenticities" in one generation forever become legitimacies in the next. As the late Reinhold Niebuhr once wrote: "But the fact is that not only property, but the two institutions of property and social stratification are in the same position of moral ambiguity. Both are necessary instruments of justice and order, and yet both are fruitful of injustice. Both have, no less than government, grown up organically in traditional civilizations in the sense that they are unconscious adaptations to the needs of justice and order. The revolts against both of them by both the radical Christians and the radical secular idealists of the seventeenth and eighteenth centuries tended to be indiscriminate."

This is what I think Gouldner means in his description of the functionalists: "Functionalism postulates that, even if a society might be reformed in some ways, there are other profound ways in which it cannot be reformed and which men must accept." In spite of the poetic license taken by Edward Hicks in *The Peaceable Kingdom*, the most famous and popular painting ever done by a Quaker, the lion will probably never lie down with the lamb.

Ages are known by the questions they raise; not by the answers they may seem to give. By definition, important questions are never answered. Thus the debate between Gouldner and the Parsonian functionalists is, in many fundamental ways, a continuation of the Quaker-Puritan debate of the

1650s, which Hugh Barbour, in his book on *The Quakers in Puritan England*, summarizes as follows: "The Quaker preacher and the Puritan pastor worked in opposite directions and never understood each other. . . . The Puritan leaders were men who had known life in all its complexity. They knew the ambiguous mixture of sin and grace in their own best actions and in the motives they least admired. They had discovered new levels of sin and evil in the moment of seeming victory, when Cromwell and the forces of Parliament broke apart in the struggle to remake England. Inevitably they regarded the Quakers as self-righteous and unrealistic. . . . While Baxter was daily praying to receive God's Spirit, Friends insisted that they had it."

IV

Noisy and ideological ages raise questions which sober ages somehow have to solve, preferably in compromise. Who knows what solutions lie ahead for tragically troubled America? Here again, I think, we have something to learn from 17th-century England. For there is something in the English character which tells them that, in the long run, order and civility are only possible in sober societies where left and right extremists, though always necessary as critics of the status quo and stimulants to change, are *never* allowed to *win*. In both the French and Russian Revolutions, for example, the revolutionaries *won out*, but, at the same time, the French and Russian people *lost out* to the one-party and autocratic regimes of Napoleon and Stalin. In the English Revolution, on the other hand, both the revolutionary puritans and the radical sectarians *lost out* when the traditional authorities of throne and altar were finally restored.

Though cruel and vindictive at first, the Restoration Settlement eventually produced a vigorous two-party system which lasted down through the Victorian age; Cavalier squires, supporters of throne and altar, became Tories; and Roundhead, gentry-merchants, supporters of parliamentary authority, became dissenters and Whigs. After a period of bitter party battles, including a series of plots and counterplots, the more moderate leaders of both parties were finally united, as G. M. Trevelyan once put it, by "the wit and wisdom of George Savile, Marquis of Halifax, 'the Trimmer,' the Philosopher Statesman, whose dislike of extremes always caused him to 'trim' away from whichever party was at the moment enjoying and abusing power." Trimmer politics of Halifax, which incidentally became a Whig tradition in England, led the way to the bloodless, Glorious Revolution of 1688 and the Toleration Act of 1689, thereby securing parliamentary authority and religious toleration. This final settlement of the Reformation in England was, as usual, quite in contrast to the extreme positions taken on the Continent. While the reactionary princes of Germany, for instance,

abolished all sectarian dissent after the bloody Peasant Wars and settled for Caesaropapism (the religion of the prince is the religion of all his subjects), and Louis XIV, in 1685, exiled the valuable and prosperous Huguenots from France, the English Toleration Act allowed dissenters or nonconformists to act as a continuing and liberating balance to the conserving, Anglican Establishment. Power warmly clothed in authority can always afford to be tolerant, naked power never. Thus it was the tolerating of this dissenting-nonconformist tradition which produced the reforming approach to social change, so often led by the Quakers, which characterized 19th-century England; the lack of a dissenting religious tradition in France and Germany, on the other hand, allowed for the growth of a stronger socialist-Marxist approach to social change, with its endless cycles of revolution and reaction. If history and sociology are any guide, we in America are surely in for a conservative reaction of one sort or another. Let us hope that it is not too late to follow in the English, rather than the Continental, tradition.

V

Finally, does the spirit of the Restoration compromise and the Trimmer-inspired Glorious Revolution have anything to tell us about the possible future course of sociology? In the first place, I should like to argue that we are *all* functionalists. If sociology is a cumulative science rather than a mildly disguised ideology, a moot assumption at best, then social theory is primarily a *point of departure*, a conceptual guide in the endless adventure of doing research, and never an end in itself or a final answer. To be brief, and unavoidably to simplify, there are two functionalist points of departure: (1) the *order-hierarchical* and (2) the *conflict-egalitarian*. Take, for instance, the following statement by Karl Marx: "The more a ruling class is able to assimilate the most prominent men of the dominated classes, the more stable and *dangerous* its rule." This is a functionalist generalization, and a valid one of course, from the *conflict-egalitarian* point of departure. Change the word "dangerous" to "desirable," on the other hand, and one has an equally valid generalization from the *order-hierarchical* point of view. To say, however, that the use of the word "dangerous" derives from a more or less valid social theory than the use of the word "desirable" is, or so it seems to me, to confuse ideology with social theory. Or take another generalization about human behavior which I find this morning, as I write these lines, in the *Wall Street Journal*. Thus the *Journal's* diplomatic correspondent, in sociological rather than ideological style, contrasts the *order* with the *conflict* theories of international relations: "The U.S. contends that tensions in international relations are abnormal: smoothing things over is the prime reason for having diplomats. The Soviets, on the other

hand, find such tensions an inevitable result of conflicting social systems; their diplomats strive for competitive advantage while hoping to stave off disasters. Compromise and self-restraint for their own sake have little appeal; they are often considered weaknesses."

The quotation above, as it stands, is a perfectly valid outline of two approaches to international tension. And the *Journal's* correspondent does not, like all too many sociologists today, go on to say that one is more valid than the other.

In the Trimmer spirit of the last lines of Merton's article; then, I would urge that Order and Conflict functionalists unite: "We have nothing to loose but our claims. We have a world of understanding to win" (p. 44). Let us all, above all, get away from the ideological, from the sociology of sociology, and get down to the doing of it, as best we can, in the grand tradition of Durkheim and Weber. And if we have some sort of restoration of authority in this country, instead of a blind reaction into some kind of violent anti-intellectualism, we may yet experience a flowering of social science much like that which England experienced in their 17th century of genius in the natural and biological sciences. One more backward glance should serve as a guide to hope.

The foundations of science in England were laid in the great Elizabethan age of poetry. It first flourished, as might have been expected, outside the conservative and church-controlled universities. Many of the early followers of the New Philosophy, as the scientific attitude was then called, congregated at Gresham College, a kind of adult-education center in the heart of London, founded in 1575 by one of Elizabeth's first councillors and son of the Lord Mayor of London, Thomas Gresham. Despite pleas from Cambridge, his alma mater, that he leave his money to them, Gresham amply endowed his college and made sure that it would remain in control of merchants like himself rather than the clerical guardians of the old tradition. The theoretician of the New Philosophy was Francis Bacon, twice related to Gresham, and attorney general under James I. His famous *Novum Organum* was published in 1620. The popularity of the New Philosophy may be measured by the fact that Bacon's *Essays* went through 17 editions in the years before the outbreak of the Revolution. Another pioneer, William Harvey, discovered the circulation of the blood in 1617 and published his findings in 1628. Harvey was appointed physician extraordinary by James I and was on the Royalist side during the Revolution. The two ideological decades of anarchy witnessed a definite decline in the doing of science, which finally flowered after the Restoration. In the single year of 1662, for instance, the Royal Society was founded (four of the 12 founding members had been teachers at Gresham College), Robert Boyle published his famous law on the behavior of gases, and William Petty, in his *Treatises of Taxes and Contributions*, founded the science of vital sta-

tistics. Three years later, Thomas Sydenham, often called the "English Hippocrates," published his first book, which brought him fame throughout Europe. In the doing tradition, Sydenham insisted on the importance of clinical observation rather than theory. Finally, the *summa* of mechanistic science was given to the world in the year preceding the Glorious Revolution, in 1687, when Edmond Halley, discoverer of the comet named in his honor, brought out Newton's classic, *Principia*. Halley not only constantly encouraged Newton to finish and publish his *Principia*; he also published his good friend's work at his own expense. I have, of course, only mentioned the few outstanding leaders of this great age of English science. For more quantitative evidence of how the doing of science declined during the two ideological decades and then flowered under the Restoration compromise, I should like to refer to a table in Merton's early book on England:

TABLE 3
NUMBER OF IMPORTANT DISCOVERIES AND INVENTIONS (5)
ENGLAND, 1601-1700

| Years | Number | Years | Number |
|-----------------|--------|-----------------|--------|
| 1601-1610 | 10 | 1651-1660 | 13 |
| 1611-1620 | 13 | 1661-1670 | 44 |
| 1621-1630 | 7 | 1671-1680 | 29 |
| 1631-1640 | 12 | 1681-1690 | 32 |
| 1641-1650 | 3 | 1691-1700 | 17 |

SOURCE.—Robert K. Merton, *Science, Technology and Society in Seventeenth-Century England* (New York: Harper Torchbooks, 1970), p. 40. Originally published in *Osiris: Studies on the History and Philosophy of Science, and on the History of Learning and Culture* (Bruges: St. Catherine Press, 1938).

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July 3, 1940

Professor Robert K. Merton
Tulane University
New Orleans, Louisiana

Dear Professor Merton:

Professor Hughes has called to my attention the fact that we have not reviewed your book entitled Science, Technology, and Society in the Seventeenth Century England, and I immediately began an investigation to account for our failure to do so.

I found that the book was received some time in 1938 and that before the book could be sent out for review, it disappeared from our book room. For some reason the disappearance of this volume was not brought to our attention until your recent inquiry addressed to the book review editor.

Needless to say we are extremely sorry that this has occurred and we set about attempting to make tardy rectification of our error. We found, however, that the place of publication of your volume was listed as St. Catherine Press, Bruges, Belgium. In view of the current political situation, it seemed pointless to attempt to procure a copy from that source. Consequently, we are writing a letter of frank confession of our "sins" and are asking that you advise how we may procure another copy. If we can obtain another copy, we shall endeavor to get a prompt review and early publication for it.

Sincerely yours,

Robert F. Winch
Robert F. Winch
EDITORIAL ASSISTANT

RFW:CS

Merton-Winch Correspondence

Department of Sociology
Tulane University
New Orleans, Louisiana
31 July 1940

Mr. Robert F. Winch
AMERICAN JOURNAL OF SOCIOLOGY
5750 Ellis Avenue
Chicago, Illinois

Dear Mr. Winch,

I have just found your letter of July third upon my return from Mexico. It was good of you to trace the receipt and disappearance of my Science... study. As you properly suggest, it would be wholly fruitless to seek another copy from the publishers in Bruges -- for the second time in a generation this little town has been closed to the outside world by a friendly visitation of the gray-clad boys from the north. Under separate cover, I am sending you another copy of the monograph. Thank you for your promise of a prompt review.

Sincerely yours,

Robert K. Merton

July 1, 1972

Dear Mr. Merton,

We do regret that World War II and other not quite such momentous events prevented the publication of a review of *Science, Technology, and Society in Seventeenth Century England*. We hope that the following review essay by Benjamin Nelson will rectify our previous negligence.

Sincerely,
Richard P. Albares
Book Review Editor

Review Essay

Science, Technology and Society in Seventeenth-Century England. By Robert K. Merton. New York: Howard Fertig, Inc., 1970. Pp. xxix+279. \$11.00 (cloth). New York: Harper & Row, 1970. Pp. xxxii+279. \$2.75 (paper).

Benjamin Nelson

New School for Social Research

This precocious masterwork has had to wait 32 years to find its way into separate and full publication in this country. Originally issued in 1938 as a long monograph in *Osiris* magazine (4:360-632), edited by George Sarton under the imprint of the St. Catherine Press in Bruges, the study soon became largely inaccessible, more often cited than read. Now at long last it is available to use in both hardbound and paperback editions, with an intensely revealing new preface in which the author speaks directly to his critics and future readers about his original intentions and his present points of view. Anyone who hopes to follow the developments in Robert Merton's interests and emphases from his days as a graduate student shall henceforth have to study both documents together—the dissertation and the new preface—very closely.

Although I shall here be limiting myself to only a few aspects of these documents, the reader should not be surprised if certain larger features of Merton's paradigmatic itinerary appear in a new light. Readers of the present essay are offered a continuous center of orientation by one of Merton's most haunting reminiscences now freshly offered us:

The inquiry began as he [the author] was rummaging about in seventeenth-century England, trying to make some sense of the remarkable efflorescence of science at that time and place, being directed in the search by a general sociological orientation. The orientation was simple enough: various institutions in the society are variously interdependent so that what happens in the economic or religious realm is apt to have some perceptible connections with some of what happens in the realm of science, and conversely. In the course of reading letters, diaries, memoirs and papers of seventeenth-century men of science, the author slowly noted the frequent religious commitments of scientists in this time, and even more, what seemed to be their Puritan orientation. Only then, and almost as though he had not been put through his paces during the course of graduate study, was he belatedly put in mind of that intellectual tradition, established by Max Weber, Troeltsch, Tawney and others, which centered on the interaction between the Protestant Ethic and the emergence of modern capitalism. Swiftly making amends for this temporary amnesia, the author turned to a line-by-line reading of Weber's work to see whether he had anything at all to say about the relation of Puritanism to science and technology. Of course, he had. It turns out that Weber concluded his classic essay by describing one of the "next tasks" as that

of searching out "the significance of ascetic rationalism, which has only been touched in the foregoing sketch, for a variety of cultural and social developments," among them "the development of philosophical and scientific empiricism . . . technical development and . . . spiritual ideals." Once identified, Weber's recommendation became a mandate. [Preface, 1970, p. xvii]

The more carefully we seek to follow the author's entire course against the horizons here adopted as backgrounds, the more surprising the signals suggested by the irregular flickerings of the beacon he has just generously set before us.

I

It seems both prudent and exciting to begin by seeking to retrace the steps Merton took in completing the original dissertation. Already the first lines of the preface and the introductory first chapter of that work reveal the special blend of boldness, caution, rigor and tentativeness which has everywhere become the author's hallmark. The pains he takes to make his purposes and propositions clear are truly extraordinary. It can hardly be his fault that these cautions did not serve to protect him from the stringencies of inclement reviewers and readers.

After a moment's foray—not without its own interest—into the cultural history of Western civilization, Merton proceeds to marshal his evidence in the manner of seasoned professional historians. He sets forth the fruits of in-depth studies of sources, original and secondary alike, illustrating religious and scientific developments among 17th-century British and late 18th early 19th-century German Pietist writers. He strives mightily to master the extensive Continental literature written from varied points of view in the history of science and technology (A. de Candolle, F. Klein, F. M. Feldhaus, Franz Borkenau, and others).

Already in the first two chapters we find Merton doing a great deal more than even the most seasoned historians were attempting in those days: he develops a "data bank" for systematic statistical analysis by intensively collating "some 6000" biographies of 17th-century worthies in all spheres which had appeared in the *Dictionary of National Biography*; he tabulated the record of technological and scientific developments in the work of Darmstädter and in the British materials. On the latter score, to let Merton tell his own story, he goes so far as to codify 2,000 pages published in the *Philosophical Transactions* and hundreds of reports in the minutes of the Royal Society as transcribed in Birch's *History of the Royal Society*. Merton's readiness to bring statistical analysis to bear in his work is further attested by his 13 numbered tables, his many charts, and his use, for comparative purposes, of the findings of M. Offenbach and F. M. Feldhaus.

Beyond all this Merton draws heavily on the frontier researches and developing disciplines of his day: P. A. Sorokin, supervisor of Merton's

dissertation and a major influence on Merton's development in sociological theory and research (Sorokin had already begun issuing segments of his work on *Social and Cultural Dynamics*); George Sarton, a mentor in the history of science; several other Harvard luminaries, especially E. F. Gay and A. P. Usher in economic history and history of invention.

It is no wonder that Merton, already a significant contributor to sociological reviews though hardly more than 25 years of age, showed such wide-ranging awareness of the uses and limits of advanced sociological theories. Sorokin and Ogburn were the principal points of reference, but among Americans Merton also drew upon or cited Veblen, Howard Becker, or others. So far as the European writers were concerned, he was already extraordinarily sensitive to the shadings of the work of Max Weber, Alfred Weber, Simmel, Mannheim, Scheler, Pareto, Tarde, Troeltsch, Vierkandt, and others. Even then Merton recognized the importance for his theme of the problematics of Jean Piaget. Nor did he lose any opportunities to draw into his orbit the work going on as he was writing, for example, the lively critique of Hessen by G. N. Clark and the careful discussion of H. M. Robertson by Talcott Parsons, then a junior member of the Harvard staff. Nor is it to be missed that he took a very strong interest in the philosophies and logics of science (Whewell, Rickert, Whitehead, Carnap, and others).

How long did it take Merton to do his dissertation? We have his own word: two years!

II

Merton's work was a unique gem on the day of its first appearance. All his later teaching about the social nature of innovation notwithstanding, his own essay was, without parallel, a "singleton."

Yes, there was the striking 1931 monograph by the Soviet scholar Bernard Hessen, who spoke of the reputed economic and social sources of Newton's *Principia*; yes, there was the scintillating *Ancients and Moderns* (1936) by the American scholar Richard F. Jones, who offered learned discussions of Baconian and other cultural and religious factors in the development of *the scientific movement* in England; indeed, beyond these two works, which were woven into the fabric of Merton's dissertation, were two other wide-ranging studies by men associated with the *Frankfurt Institut für Sozialforschung*, Franz Borkenau and Henryk Grossmann, who offered radically different accounts of the sources of the mechanical world view of the 17th century. (Borkenau claimed that the new world images derived from the rationalization of labor in the era of manufacture in the 16th and 17th centuries. Grossmann sharply challenged Borkenau, insisting Leonardo, Galileo, Descartes, and others were much less inspired by incipient rationalizations of the labor process in manufacture than they were by the observations of—and concerns with—the workings of actual machines and mechanisms.)

But these works and others notwithstanding—of the four mentioned above only Grossmann's is not mentioned by Merton—there was no book which could compare with Merton's unheralded dissertation as a conspectus of the theoretical and empirical problems *at issue from his special perspective*; there was no book which went so far in making explicit the ways in which evidence and proof in need of systematic review might be fruitfully correlated in statistical fashion. (I am not saying that the work was free of flaw!)

Indeed, here the riddles begin. Why has this precocious work had so strange a fate? Why have so few done studies which carefully go over the ground that Merton charts and make advances in the theoretical and empirical mappings of new findings? Why has so much of the discussion of his work come from unfriendly historians of science rather than from sociologists and cultural historians who share his outlooks and methods? Why has Merton himself spoken so little over the years to the issues and sources involved in this area? Why does he now go to such great pains in his important 1970 preface to suggest that very few of his readers and critics have accurately grasped the turns of his argument or, for that matter, have truly appreciated the design and intent of his book?

In the *AJS* (July 1972), reference is made to the fact that this work had to wait 34 years to be reviewed in its pages. This act may be more a symbol of the puzzle than an explanation of the riddles we here hope to explore.

III

To find starting clues to our puzzle we must again and again review the carefully chiseled explanations of Merton's original theses and the—at times caustic and unsparing—assessments of critics he presents in his 1970 preface.

Merton frames his main argument by pointing to the figures which result from applying George Sarton's procedure of quantitative content analysis to Merton's own work: his critics have paid least attention to matters to which he had devoted the greatest number of pages and the highest percentage of content; the "hypotheses about the economic and military influence on the range of scientific inquiry" are given more space in the monograph than "the hypotheses that link up Puritanism with recruitment and commitment to work in science" (p. xix).

Here Merton breaks into open complaint: "The *trio* of chapters on the second subject has received all manner of attention in scholarly print while the *quartet* of chapters on the first subject has received little attention" (p. xii, my italics).

In Merton's reading of his own complex orchestration, the *trio* and *quartet* were harmonically integrated from the beginning. Too few of his readers were able to recognize that the trio and quartet were exploring the same issue in the same manner and in the same key. His 1970 preface is

a mighty effort to show how the two segments resonate together under his magisterial baton.

It seems strange to me that Merton should now so strongly fault this choice of perspectives. In changing settings different degrees of interest attach to different themes in the orchestration. The latter chapters on economic and military influences probably seemed to his reviewers less problematical, less novel, and composed of quite different thematic elements from the chapters in the trio. Actually, in my own view, these latter chapters do have a wealth of material which carry the reader beyond 17th-century England and come closer than any other parts of the work to having an international flavor. Also, having lately gone over the evidence, I can confirm Merton's recollection on another emphasis of the quartet. In Merton's graduate student days, American sociologists were not nearly so clear about the distinction between "technology" and "science" as they later became, thanks in part to Merton's own efforts.

Moreover, as he now claims, Merton *was* careful "to avoid the mock choice between a vulgar Marxism and an equally vulgar purism"; he did go beyond "arguing that the selection of problems for investigation was either entirely governed by economic and military concerns or was not at all influenced by such concerns"; yes, he did even then establish a distinction "between the *motivational* and *institutional* levels of analysis . . . offering a procedure which, however coarse-grained, may not be inappropriate for analyzing the spectrum of scientific work today."

Despite the strong evidences in support of his current retrospect, I am not surprised that Merton's critics have fastened upon his trio rather than his quartet, nor am I surprised that he himself now strongly calls attention to his quartet rather than his trio. Times change, men change, and the contexts of scientific interests, inquiry, and proof change.

When Merton talked about economic and military influences on the shifts of interests in and estimation of science, he was in many—but not all—respects attempting much more than Hessen or Borkenau or Grossmann. He was, however, not breaking wholly new ground in principle—although careful scholarship and his many subtle distinctions promised a marked improvement in the state of the discussion.

Surely, however, Merton would have to agree that it was the "Puritanism and science hypotheses" which constituted the distinctive feature of his effort in 1938 and which, indeed, constitute it even now. In their original form, the theses about the cultural and social backgrounds of the "institutionalization" of modern science—to adopt Merton's parlance—had the flavor of an adventure into largely unknown but highly contested ground and, in the opinion of many then, and even now, remain an area needing to be carefully studied against wider backgrounds than have so far been brought into focus.

If Merton's 1970 preface does seem to establish that none of his critics has fully perceived his original aims exactly, one finds oneself drawn to retain a lingering doubt despite—perhaps, indeed, because of—the strenu-

ousness of Merton's exertions to set the record straight on page after page of the new statement. Accept these caveats at face value in their full rigor (especially those on pp. x, xvi, xviii), and we are locked into a mystery as to why so few of Merton's most informed readers have known how to read the orchestration of the trio, or why, for that matter, so many echoes of the trio even now seem to sound in the quartet and elsewhere in the work.

There are other too rarely observed indirect corroborations of his present emphasis which Merton might have done more to stress in the new preface. His book did not purport to be a contribution to the history of scientific ideas or of technology. At no point does Merton attempt a detailed or consistent explication of the problems, theories, or presuppositions of 17th-century science and its culture. Nor does he claim to explain "the progressiveness" of science or the replacement of one scientific theory by another, a fact which is currently being forgotten in the writings of those who are trying to wed Merton to Thomas Kuhn by vastly expanding the scope of Merton's work on reference groups and role sets.

The 1970 preface leaves readers little recourse but to agree that the overriding aim of the dissertation was to explicate the processes of sociocultural *legitimation* of an "emerging social institution" and outlook—an *institution* and *outlook*—which required strong support from society on a sustaining basis in order to thrive. At one point Merton puts the matter in a rather general way: "The substantial and persistent development of science occurs only in societies of a certain kind, which provide both cultural and material conditions for that development. This becomes particularly evident in the early days of modern science before it was established as a major institution with its own presumably manifest value" (p. xix). Elsewhere the stresses are stronger and elaborated in greater detail; in other places they are weaker. Soon these variations may be expected to become the theme of many special monographs by a new generation of readers.

It will be for them to decide how truly the 1970 preface registers the latent content—and intent—of the original dissertation. Will they agree that the issues are now resolved by Merton's latest statements? I can hardly close this section without citing one such statement:

In the case in hand, it is certainly not the case that Puritanism was indispensable in the sense that if it had not found historical expression at that time, modern science would not then have emerged. The historically concrete movement of Puritanism is not being put forward as a prerequisite to the substantial thrust of English science in that time; other functionally equivalent ideological movements could have served to provide the emerging science with widely acknowledged claims to legitimacy. The interpretation in this study assumes the functional requirement of providing socially and culturally patterned support for a not yet institutionalized science; it does not presuppose that only Puritanism could have served that function. [P. xviii]

It is hard at this point to refrain from turning to an implicit wider horizon of these remarks—the work of Max Weber.

IV

A second piece in the puzzle is brought into focus by exploring the contrast between the fortunes of Merton's dissertation and Weber's essays on "The Protestant Ethic and the Spirit of Capitalism." The latter papers were no sooner published in the *Archiv für Sozialwissenschaft und Sozialpolitik* for 1904-5 than they were under discussion by political and economic historians who felt that Weber had failed to prove and had overstated his case. The discussions continued extensively until 1910 when Weber himself decided to call a halt and served notice, in an essay entitled "Anti-Critical Last Word," that he preferred to leave this phase of the issue without further polemic. There were other questions crying out to be answered.

In the case of Merton's dissertation, most of the controversy is of relatively recent vintage. Efforts to relate to the pages to which, by Merton's own account, he was most partial—namely, those dealing with the economic and military spectrum of scientific work—have been extremely rare. Nor have many cared to review the details he offers as evidence of the fluctuations in the cultural profiles and trends he has particularly studied. There is something in the contrast which asks for clarification.

A more impressive aspect of the puzzle is the difference in the way Weber and Merton moved from these first statements. For Weber, the *Protestant Ethic* was a prelude to a larger effort that was begun with the publication in 1916 of the first of his sequence of masterly essays on the "Economic Ethic of the World Religions," which is now known as "The Social Psychology of the World Religions." He had now embarked upon a series of extraordinary special studies in which he sought to explain by a special comparative method how rationalism and rationalization had fared in the Orient—in Far East and Near East alike, in China, ancient India, ancient Judea. He was determined to develop a wider understanding of the particular influences which may have worked to promote or to inhibit the full functionalization and rationalization of all elements of the cultural, political, economic life of the different areas which had been the settings of the development of world religions.

There have been some moves of a comparable sort in the work of Merton, but these have been only hints. His thrust has been different; instead of moving out toward the comparative historical sociology of socio-cultural process, he has tended to develop an ever more keen concentration on the areas or the boundaries of the sociological psychology of social and cultural process.

Actually, an impressive fact about his dissertation which might easily escape notice is that Merton was already launched on the sorts of studies which were to highlight his later career down to the present day. Thus we must be attentive here to the footnotes or we shall miss this fact. Already he is pointing to the critical importance of struggles over priorities and competitive advantage—shades of the Matthew Effect; he is promising to do later studies on science in its social ambience; he is striving to

establish the relation of rates of change to the levels of frequencies of social interaction among fellow professionals; he is striving to document the fact that extra-logical factors of allocations operate to create settings for scientific research which produce outstanding innovations.

An interim summary may help us here:

A) Merton's doctoral dissertation was the work of a young pioneering scholar with an extraordinary range of skills, interests, and sympathies—a range rarely matched in the history of sociology. Few aspects of human activity, experience, and expression failed to interest him deeply. He had read widely and had come to have insights which allowed him to innovate on almost every page.

B) To the great loss of instruction and research in our country—in my view—the several strains of his orientation tended to draw apart during the years after his dissertation. In place of the model of analysis he exhibited in what he calls the trio, we witness a thrust toward frames which come to serve as the formats of highly institutionalized theoretical and empirical work mainly in the sphere of a sophisticated sociological social psychology oriented to narrower temporal horizons.

Here we are on the verge of solving a critical part of the puzzle. For Merton this dissertation may be said to represent the high point of his efforts in historical sociology rather than a point of departure for new work along these lines. Even before his dissertation appeared, Merton had already published several essays dealing with fluctuations in the development of rates of industrial invention and in the course of Arabian intellectual development (the latter was done in collaboration with Sorokin). Merton's own "Puritanism, Pietism, and Science" appeared in 1936. Sorokin's *Social and Cultural Dynamics* was already in progress and was, indeed, to be published in the following year (1937), a year before the appearance of Merton's dissertation in *Osiris*.

As I read the evidence, it was Merton's seminal paper, "Social Structure and Anomie," published in the same year as his dissertation, which was to set the pattern for the "normal science" in departments of sociology at American universities, to become the "launching pad" for a great number of American doctoral dissertations and research monographs. This was the study which was to become the model of the so-called structural-functional analysis in the manner now regularly ascribed to Durkheim.

I am saddened to report that much of the research which developed out of his interest in the sociology of science served to elaborate neither his trio nor his quartet, nor, for that matter, his duo (the two first chapters), but came to be concentrated on the documentation of the more miniscule theoretical elements at the level of social interaction. Nowadays work done in his fashion, except when it is done by him, is likely to seem a far cry from his original intentions. Elsewhere in this issue we will be reminded of how Merton sounds when he recovers his full range. I refer to his exceptional article on "Insiders and Outsiders."

A LAST WORD

Nothing I have here written is to be construed as arguing that Merton has ever abandoned the immensely deep interest he showed from the beginning in historical sociology of culture. Any review of the essays he has written since 1938 would prove that this field has remained near and dear to him across the years. It is simply being suggested that a shift in focus reduced the importance of the wider perspectives which manifested themselves with great penetration in his paper on "Puritanism, Pietism, and Science" and throughout the course of his dissertation.

One is reminded of Robert Frost's poem that recalls how two roads diverged in the wood and he (the poet) "took the one less travelled by."

The suggestion of the present review is that the road Merton was to take was to become the high road of American sociology and the one he was to visit only occasionally was to undergo very great neglect. The fact is that the issues, as they come to be circumscribed within sociology largely developed in the shadows of his work, forfeit much substance and scope and too often seem to lose rather than benefit from the immense attention accorded to them. On the other hand, the areas too soon abandoned because of apparent difficulties in introducing system and rigor continue to call for the kinds of insight and skill which are only rarely wedded in extremely well-trained men like Merton.

The "road less travelled by" was the road which led toward the comparative historical sociology of sociocultural process in the spirit of Max Weber, Alfred Weber, Durkheim, Mauss, the Durkheimians, Joseph Needham, and a number of contemporary historians and philosophers of science. Merton's dissertation is clearly a pioneering first step on that road. May one not hope that he and some of his future readers will recover the wider purposes which first drew him forth on this quest?

I have my own reasons for being persuaded that this must happen before too long.

Book Reviews

The Scientist's Role in Society. By Joseph Ben-David. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1971. Pp. xi+207. \$6.95 (cloth); \$2.95 (paper).

Bernard Barber

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A classic of sociological analysis, of which we have had strong intimations in his articles published during the last ten years, has now been fully born in Professor Joseph Ben-David's *The Scientist's Role in Society*. I wish to stress, because the growing differentiation of somewhat insulated specialties in sociology makes it necessary to do so, that Ben-David's book is a classic of sociological analysis in general, not just of the sociology of science. Its analysis of the emergence, structuring, legitimation, and diffusion of a major social role presents a model for everyone to follow; all sociologists use some type of role analysis.

In a brief review of a highly condensed book (it should have been and could have been much longer, but the publishing format prevented this), I cannot do justice to the subtlety of the analysis and the richness of the historical and contemporary data that Ben-David presents. Some of the data are derived from his "secondary" analysis of an extremely diverse and multilingual historical monographic literature. But the analysis of the 19th- and 20th-century developments in the scientific role is also based on quantitative data constructed by Ben-David himself from what are, again, diverse and multilingual sources. Throughout his book, he uses the logic of comparison as a quasi-experimental method for testing his analysis. Thus, the early modern world is compared with earlier traditional societies such as China and Greece; medieval Europe is compared with modern Europe; 16th- and 17th-century Italy is compared with northern Europe at the same time; and modern France, Germany, Britain, and the United States are very effectively compared with one another.

Why, then, did the scientific role not emerge before early modern times? "This lack of development," says Ben-David, "cannot be explained either by the absence of the notion of science or by the absence of talent" (p. 22). Rather, it was due to the failure to differentiate the scientist's role from the religious role, the magician's role, or the social-moral philosopher's role in which such scientific activity as existed was imbedded. Further, the scientist's task of constructing abstract, systematic, and comprehensive theory, tested by empirical data, was never differentiated from the several *practical tasks* in which men had already fairly successfully manipulated concrete reality, whether in medicine, architecture, or even astronomy. In sum, there was no sufficient autonomy, specialization, legitimation, and continuous support for the scientific role, all of which are necessary for the continuous growth of science.

Ben-David shows how a number of *independent developments*, beginning

in medieval Europe and reaching a peak in 17th-century England, led to these necessary conditions for the proper performance of the scientific role. Science finally did get the degree of autonomy which makes it one of the major institutionalized role structures of the modern world. Some of these independent developments are the emergence of the university as a relatively autonomous intellectual community, the differentiation of philosophy from theology, the subsequent differentiation of science from philosophy, the new values and ideologies that centered in the "scientistic movement," "the growth in importance of new social classes whose outlook was sympathetic" (p. 58), and the combination of new religious ideas and new religious diversity in northern Europe, where these new social classes were especially strong.

In the modern world, once the scientific role was fully institutionalized, new social processes have influenced its further development. Ben-David especially stresses the importance of the processes of centralization-decentralization, competition, and organizational flexibility.

Let me mention a few more analytic matters that Ben-David handles very well:

1. Using Edward Shils's concepts of "center" and "periphery," Ben-David is instructive about the movement of science from its early modern "center" in Italy to successive "centers" in England, France, Germany, and now the United States. Those interested in the "developing" countries will profit from his discussion of the "peripheral" place of such countries in modern science and the implications for their science policy.

2. Those who think that sociology cannot deal with social change should read this book. Among many other things, Ben-David's discussion of the unintended, unplanned outcomes in modern American scientific organizational development provides the delight we always feel when sociological analysis discovers for us something really new.

3. As a moment's thought about the close connection between modern science and the modern university would suggest, Ben-David has much to say about the latter. Indeed, I felt I learned as much about the modern university here as in any other book I have read.

One "critical" remark. To date, the history of science has too much stressed the idea content of science and its autonomy. Ben-David has here redressed the balance in the history of science by stressing its social or interactional aspects. He seems to disclaim any influence whatever of the interactional aspect of science on its conceptual or idea content. But I think these two aspects need now to be brought together; we need to see whether and how they are interdependent. Some work of this kind has already been done. But mostly this is a task for the future.

Obviously this is a book which deserves the widest possible study. Sociologists will find it of great value to them as sociologists, but it is also of direct relevance for educational and scientific policy. Ben-David makes it very clear that good sociology has much to contribute to the making of social policy.

Masters of Sociological Thought: Ideas in Historical and Social Context.
By Lewis A. Coser. New York: Harcourt Brace Jovanovich, Inc., 1971.
Pp. xxi+485. \$8.95.

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Of all differences between sociology today and sociology a generation, even a decade, ago, the greatest seems to me to lie in the substantially more humanistic character or temper of the discipline at the present time. There are several ways of illustrating that, but I shall content myself here with the increased, and still increasing, attention given to the intellectual roots of sociology; to the relation between the main currents of sociological thought and those of philosophy, literature, and other areas of culture; and, not least, to the contexts within which the chief perspectives and values of sociology have risen and flourished. Few indeed were the sociologists two decades ago who expressed interest in the history of their major ideas and premises. How fortunate we are that today exploration of the sociological past is the serious work of some of the very best minds among us.

Lewis A. Coser is one of these minds. For all the years he has been at work as a scholar, in several fields of inquiry, he has made evident enough his interest in and command of the masters of sociological thought. There was never any more question of the role of Simmel in the shaping of Coser's notable theory of conflict than there was of what Coser had added to Simmel. Coser's writing has never lacked awareness of roots. This book is, however, his first systematic exploration of the history of sociological thought.

Coser's book is more than intellectual history, though, in the ordinary sense. It is a first-rate venture in the sociology of knowledge. As Robert K. Merton points out in his instructive foreword, Coser goes beyond filiation of ideas to the adoption of sociological perspectives to analyze and interpret the development of these ideas. Coser himself at one point in the book refers to his work as "the social ecology of sociological ideas," and the characterization is apt.

The author has chosen an even dozen of the masters: Auguste Comte, Karl Marx, Herbert Spencer, Emile Durkheim, Georg Simmel, Max Weber, Thorstein Veblen, Charles Horton Cooley, George Herbert Mead, Robert Ezra Park, Vilfredo Pareto, and Karl Mannheim. Let those who have other names in mind, either as replacements or additions, write their own books. All things considered, it would be extremely difficult to demonstrate why, except from the point of view of one's personal disposition, any of these names should be dislodged in a list of twelve from a work that is concerned with the question: How has sociology come to be what it is?

The great merit of the book—apart, that is, from the author's learning, insight, and humane feeling for ideas and contexts—is its structure. Coser follows an unvarying pattern in his analysis of each of the masters: first,

an account of the major themes and concepts, whether substantive or methodological or both, in the works of the man; next, a treatment of the biography of the man, his own direct sources, personal contexts, and signal relationships with others; following this an exploration of what Coser calls the intellectual context—the central intellectual influences of the man's period, those which may be seen as independent of the man himself but are shown by Coser nevertheless to be inseparable from the man and our understanding of his work; then an account of the social context of a given master's significant ideas, of the interactive relation between ideas and the social and economic and political forces which in part shaped the ideas and to which the ideas were directed; and, in useful conclusion, a succinct summary of life, times, and ideas.

In hands less skilled and sensitive than Coser's the pattern might become mechanical, with the distinctive quality of one or other of the titans subordinated to a format large enough and general enough to include them all. Suffice it to say here that at no point does the book seem mechanical. Structure does not dominate or distort crucial individual details.

No doubt Coser, like the rest of us, has his favorites. How could he not have? One respects all the same the balance and the equity of treatment to be found in the book's chapters. There is little or nothing in Coser's earlier writings to suggest any special interest in, say, Spencer, Pareto, or Park. Never mind. One would have to look a long time to find fairer and more dispassionate accounts of these three. And the same holds for all others in the book.

And in every chapter there is to be found the arresting detail, the single clarifying incident, the concrete fusion of background and biography, that can at times give the quality of first-rate literature to Coser's analysis. His account of what he calls Marx's "isolation and double marginality" is splendid, and given extra point by a passage drawn from the literary imagination of Edmund Wilson. Nowhere else have I seen the relation of Durkheim, and also of Weber and of Simmel, to the complex political and moral issues of the turn of the century set forth so convincingly. Coser has what few intellectual historians have, and so badly need: a recognition of the vital difference between influences upon a mind that are actually generated by surrounding scene and influences in the form of major ideas and perspectives which antedate surrounding scene in their origins and are at best given quickened intensity by elements of the scene.

Coser's book comes at a very good time, and I wish it the widest possible reading at all levels of academic sociology. As is true of all genuinely first-rate books, clarity of thought and style combined with importance of subject matter put *Masters of Sociological Thought* within reach of all. As I noted above, there is an unmistakable current of interest right now in the humanistic character of sociology, and not least in the intellectual and moral roots of the discipline. This current, aided by such a book as this one, may well succeed in keeping the sociological imagination alive. I profoundly hope so. The disparity today between the generally high quality of undergraduate minds (despite self-serving faculty myths to the contrary)

and the desolating, mind-numbing junk that is passing for textbooks in sociology—with their cartoons, candid camera photographs, and other pieces of infantile gimmickry, not to emphasize the whole Dick-and-Jane-reader mentality that suffuses them—will, if it continues much longer, drive all remaining good minds from sociology. For a while, during the 1940s and 1950s, academic sociology in this country showed signs of moving up the curricular status pole. All signs are to the contrary now, thanks largely to the textbooks driveling that exploitative, callous, or ignorant instructors in sociology insist upon pushing down the throats of their captive sheep.

I like to think that Coser's distinguished book may help lead us to the next and higher phase of the academic revolution in our time: the annihilation of trivialism in the academy. Let the Terror descend upon all exploiters and pillagers of the student mind. Death to all enemies of the sociological imagination!

On Humanistic Sociology. By Florian Znaniecki. Chicago: University of Chicago Press, 1969. Pp. vii+309. \$9.50 (cloth); \$2.45 (paper).

Edith Kurzweil

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"Plus çela change, plus c'est la même chose." Florian Znaniecki's work, at the time of its appearance (between 1919 and 1952) was somewhat neglected. His contributions to sociology, though discussed, had little influence. It seems significant that his thoughts are reintroduced to sociologists at this time. In spite of all the advances made, particularly in techniques and research methods, we are again searching for answers about the validity of data, participant observation, observers' bias, weight given to one item over another, the selection of questions as in themselves implying answers and/or results. In a sense the publication of this book seems a step into the past. Yet, a critical rereading and rethinking of the problems confronted by Znaniecki might help us to better understand our present problems.

Robert Bierstedt's selections and his excellent introduction immediately convey to the reader that Znaniecki was a philosopher as well as a sociologist. Even where the reader disagrees with the conclusions reached, the logical construction of his arguments and critiques attest his devotion to disciplined thought. In order to prove that sociology was a special social science—namely, a general social science with its "humanistic coefficient," in addition to all the other social sciences—he had to delimit the fields of the others and debunk many of their claims. He traces the concept of sociology from the thoughts of Comte onward. Comte's positivistic sociology—the sixth and last of the sciences, evolved from biology, which is the fifth science—and Spencer's evolutionary theory are disproved as too organismic and therefore inadequate to explain a science of society or a general science of culture. The work of Durkheim and his disciples—that

is, Mauss, Halbwachs, Granet and Levy-Bruhl—bases the unity of society on solidarity and collective representations. While valid in its methodology when applied to social phenomena such as religion and primitive cultures, with its very progress it “slowly undermined the conception of society as an integrated system within which all the culture shared by its members is included” (p. 281). Although American sociologists first accepted Comte’s and Spencer’s theories, they then conceived of cultural integration as a system of differentiated but interdependent collective functions or institutions—demographically and territorially defined. Cooley, McIver, and Hiller, as well as Simmel and von Wiese, are seen as essentially defining sociology as the study of human relations, or social relations. Their approach, writes Znaniecki, raises other problems, namely, of order and of the connection between sociology and the established cultural sciences, and between sociology and the natural sciences. While Znaniecki defines social psychology and behaviorism as particular social sciences, he delimits their scope to “actions bearing upon men as their objects and intending to provoke definite reactions” (p. 185). He debunks the “common sense” approach to the study of social phenomena and planless empiricism by haphazard selection of various possibilities, and the “latent or manifest supposition that we know social reality because we live in it” (p. 56). Psychoanalytic theory, because of its derivation from the unconscious, and therefore not directly observable action, is rejected: “We cannot find proof that a painter paints a picture as a result of sublimated sexual tendencies” (p. 234). While rejecting the general theory, Znaniecki assigns psychoanalysis a place in the study of genetic and causal processes.

While the scope of the specific social sciences is gradually limited, the role of sociology in their midst becomes clear. While all modern sciences share the problem of causal explanation and must attempt to understand the process of “becoming,” social theory too must find its way along these lines. “Social becoming, like natural becoming must be analyzed into a plurality of facts, each of which represents a succession of cause and effect” (p. 82). In essence, then, Znaniecki is a follower of Max Weber. Znaniecki’s sociology is essentially the science of human and social relations as experienced by the participants in a social action and defined as interaction. The “definition of the situation” (usually attributed to W. I. Thomas and used in their joint work *The Polish Peasant*) seems to have been derived from Weber’s concept of *Sinnzusammenhang*. When the chapter on social actions is summarized, both in terms of methodology and content, Weber’s influence is obvious. Znaniecki’s addition of the humanistic coefficient—which includes supplementary evidence by other agents (actors to Weber), participation, or reproduction of the experience—has at times been criticized. While in this particular selection the concept of role is not elaborated, the author suggests that sociologists ought to pay more attention to it. Znaniecki’s concept of “values” is similar to Weber’s and Parsons’s.

It is interesting to speculate whether Znaniecki’s work on *Social Action*, which appeared in 1936, shortly before Parsons’s *The Structure of Social Action*, would have had more of an impact had it appeared at a different

time. No doubt, a theory proving convergence of thinkers as diverse as Durkheim, Marshall, Pareto, and Weber would attract more attention than would a partial theory of social action. Furthermore, Parsons's approach was positive, insofar as it emphasized what was similar, while Znaniecki's was negative in its rejection and/or truncation of existing theories. Parsons subsequently presented a grand theory, while Znaniecki's remained partial—he kept a grand theory in mind for the future. As Robert Bierstedt says, "It is still too early for those Olympian judgments in which historians of sociology will one day indulge" (p. 33). In the meantime, we can all deepen our own knowledge of the discipline by reading and thinking about the ramifications of Znaniecki's contributions.

Sociology in Its Place and Other Essays. By W. G. Runciman. Cambridge: Cambridge University Press, 1970. Pp. vii+236. \$7.50.

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The first of W. G. Runciman's 11 essays bears upon the enduring debate over the place of sociology as a field of scholarly inquiry. He argues that "sociology cannot be autonomous in the sense that Comte, and subsequently Durkheim, wished to maintain" (p. 7), and that "no social science, however demarcated, can have laws of its own any more than geology can" (p. 11).

Sociology is seen as lacking an identity of its own because it lacks internal cohesion. Runciman identifies four British "sociological" traditions (his quotation marks) which have remained consistently separate from each other, namely, the evolutionary, the political-economic, the ethnographic, and the administrative-reformist. Consequently, "it is as difficult to find a distinctive method as a distinctive content. In content, sociology ranges over virtually the whole field of social behavior. In method, it can range from documentary research to participant observation to sample surveys to computer simulation" (p. 4).

It is unfortunate that Runciman interlaces cogent and clearly stated arguments with tawdry polemics in whose support he obfuscates the obvious with pompous self-righteousness. Thus he is merely knocking down a straw man of his own manufacture when he accuses Talcott Parsons of palming off taxonomy as theory. Similarly, Runciman misses the mark when he tries to refute Homan's generalization that "social interaction promotes friendliness" (p. 32) with the observation that "it is . . . easy to cite well-documented cases where interaction decreases liking—for example in overcrowded prison camps" (p. 32). Runciman overlooks that overcrowding actually obstructs intensive interaction between specific individuals.

It is, furthermore, disturbing that Runciman fails to mention the work of Georg Simmel and his thesis that sociology constitutes an autochthonous

scholarly academic discipline. Simmel readily acknowledged the relevance of history and ethnography (i.e., anthropology) for sociology as the study of forms of relationships and interaction. Since Simmel argued that the study of sociology is an autonomous discipline, references are certainly in order in a work that deals with the place of sociology.

Runciman's observations regarding the demarcation, scope, and variety of methods of sociology call for the attention of sociologists in spite of manifest weaknesses in his argument and his approach. His pronouncements upon the indivisibility of sociology, history, and anthropology rate careful attention from all who are engaged in working out programs of instruction in sociology. Runciman's essay, its shortcomings notwithstanding, demonstrates effectively the necessity of relating the study and the application of sociology to other disciplines.

In the three remaining chapters of part 1 we find discussions of sociological key concepts. The chapter "What Is Structuralism?" aims a philosophical critique at Piaget and Levi-Strauss in which Runciman grants that Piaget, at least, invokes "the standards of public testability for his explanation of the origins of logical thought, and thereby his own psychological theory" (p. 56), while "it always remains to be asked why the mythography of Levi-Strauss is not itself a myth" (p. 56). The anticlimactic suggestion "that 'structuralist' theory may not be distinctive to quite the degree that is apt to be claimed for it" (p. 58).

The chapter on "Explanations of 'Religious' Beliefs" demonstrates "that in accounting for collective beliefs there is no valid distinction between 'sociological,' 'anthropological' and 'historical' explanation" (p. 89). And he further makes clear his view "that there is a difference of kind between psychological theory and sociological (and therefore ideographic) explanation" (p. 90). After a critique of the work of Weber and Durkheim, the chapter concludes: "If, however, the argument of this paper is correct, the future body of validated explanatory theory about systems of belief will have at any rate these three predictable characteristics; it will not be, or embody a theory of either 'religion' or 'magic' as such; its predictive value will rest on its success in the application within precisely classified social conditions of some more sophisticated psychological theory than is yet available to us, and it will be an evolutionary theory as, the world being what it is, all theories of social behaviour must be" (p. 101).

The chapter on "Class, Status, Power?" substantiates at excessive length "that conceptually, at least, economic class status and power are genuinely distinct" (p. 109). Actually, the fact that categories of persons exist who rank inconsistently (such as priests high by prestige and low in wealth, or foreign merchants who are rich but powerless and not respected) obviates the author's protracted argument and justifies, at the same time, a completely unqualified conclusion to the effect that class, status, and power are indeed distinct. Once again Runciman might have benefited from reading Simmel.

The three chapters in part 2 report Runciman's empirical studies. Under the title "'Embourgeoisement,' Self-rated Class and Party Preference," he

interprets data that reflect the effect upon party preference of the relationship of self-rated class identification with actual class membership in England. The data suggest comparative analysis with the data from similar studies in the United States, Sweden, Germany, and other countries.

The discussion of "Charismatic Legitimacy and One Party Rule in Nkrumah's Ghana" elaborates on Max Weber's concept of routinization of charisma against the background of Nkrumah's rise to power.

In his chapter on "Status Consistency, Relative Deprivation, and Attitudes to Immigrants," Runciman concludes from an analysis of data on attitudes to immigrants in England and Wales "that 'status inconsistency' is better abandoned as an independent variable purporting to explain variations in the frequency (or, for that matter, the intensity) of such attitudes" (p. 193). He also claims that his findings support other studies that in his opinion refute the argument by Bettelheim and Janowitz to the effect that "status disequilibrium" as a result of downward occupational mobility leads to exhibition of attitudes of racial prejudice (p. 178). Runciman overlooks the fact that the differences in educational and occupational levels in the data he analyzed were so small that inconsistencies were bound to be of no consequence. Therefore Runciman's findings in no way invalidate the conclusions regarding status inconsistencies and status disequilibrium drawn by Bettelheim and Janowitz, Lenski, Mark Lefton, and others who interpreted data where the inconsistencies were perceptible not merely to the researchers but also to the subjects of the surveys.

The three rather philosophical chapters of part 3 deal with "Misdescribing Institutions," "'Social' Equality," "'False Consciousness,'" and "Games, Justice and the General Will." They too are very well written, but they do not deal with the themes of the preceding chapters.

The essays brought together in this text do not constitute an integrated whole, and the author's arguments suffer from serious shortcomings. Yet all of them are well worth reading because the author expresses himself with such clarity that even where his arguments are assailable they stimulate thought and insight.

Academic Values and Mass Education: The Early Years of Oakland and Monteith. By David Riesman, Joseph Gusfield, and Zelda Gamson. New York: Doubleday & Co., 1970. Pp. xvii+332. \$7.95.

Rita Kirshstein

University of Massachusetts

The sociologist studying higher education is often influenced by much of the popular criticisms which flood the presses. Subsumed under the rubric of "sociology of education" is much watered-down philosophy and emotional-laden moralizing which neither leads to an understanding of what is actually happening within educational institutions nor contributes any theoretical background to the scattered bits of information that do exist.

Academic Values and Mass Education must be complimented for its attempt both to present information in a straight forward manner and to provide its readers with an interpretation and understanding of its subject matter.

This book is a study of the first few years of Oakland and Monteith Colleges, both of which are located in Michigan and were founded in 1959. An offshoot of Michigan State University, Oakland began with the idea of providing a standardized but high-level type of education to individuals living on the fringe of Detroit. As a commuter college in an area which lacked a similar type of institution, it hoped to educate persons who otherwise would not have had such an opportunity. Monteith, connected with Wayne State University, was more experimental in its origins. Wishing to base itself on some of the smaller and more prestigious liberal arts colleges in the United States, the founders of Monteith believed that vocationally oriented students would not only benefit from but be interested in a general educational program.

Although Riesman, Gusfield, and Gamson present information relating to all principal components of the two colleges, their central focus is upon the faculty. The small scale of these schools at the time they began permitted the researchers to obtain and synthesize data pertaining to many aspects of the experiences of the faculties. The authors ask, for example, who is attracted to newly founded schools, lacking established reputations and academic styles? How do the ideals and values brought in by newly recruited teaching staffs respond to the goals of unestablished educational institutions? To what extent are the aims of faculty altered by their concrete experiences with students? Are there significant differences in the reactions of representatives of various disciplines to situations which arise? These are a few of the main issues which are dealt with in this book.

At both Oakland and Monteith, differences in opinion and orientation were evident along departmental lines. During Oakland's first year, the high proportion of failing grades received national attention and led to sharp criticism of the students by faculty members. An examination of the grades revealed sharp differences between social science and natural science courses. With the passing of time, differences in the number of failures between departments decreased, as an unwritten type of standard seemed to develop for the school. At Monteith, the issue of personal, informal relationships with a select group of students sharply divided the social science and natural science staffs. Because the social scientists represented the largest proportion of the faculty and for the most part had received their graduate training from the same university, their presence as a united body influenced much of what occurred at Monteith in its formative years.

Irrespective of one's interest in the issues themselves, the manner in which these questions were researched and the way in which their findings are presented offer the sociologist methodological insights which can readily be applied to research within many fields other than the sociology of education. Besides interviews with faculty members at Oakland and

Monteith, sources of data included attendance at faculty meetings and classes, overheard telephone conversations, the reading of student papers, examination of documents, and informal luncheon discussions. Demographic data were complemented by personal impressions of the researchers; statistical tables were given more meaning by the addition of interpretations gathered from other information sources. How these varied kinds of data are evaluated as a contribution to sociological knowledge depends upon one's methodological perspective. However, I am convinced that, in studying the *process* of development at Oakland and Monteith, Riesman, Gusfield, and Gamson have grasped the social reality of what was happening at these institutions.

This variety of methods did result in confusion at a few points in the book. One is led to wonder at times how the authors came to know certain alleged facts, and elsewhere one is puzzled about the basis of some of their interpretations. Although quoted statements by faculty members and students generally prove useful in bringing out significant points, there are occasions when the reader might feel that these were taken out of context in order to offer evidence.

As a whole, *Academic Values and Mass Education* is an interesting and illuminating piece of research. It takes account of the social and historical context out of which Oakland and Monteith arose and developed. It also makes use of the existing literature in related areas. Smoothly written and well organized, this book is worthwhile reading for those interested in higher education, the development of organizations, and sociological methodology. Although questions must be raised as to the validity of certain interpretations, these very methodological questions are of central importance in present-day sociology.

University Authority and the Student: The Berkeley Experience. By C. Michael Otten. Berkeley: University of California Press, 1970. Pp. xviii+222. \$7.50 (cloth).

Terry F. Lunsford

University of California, Berkeley

Michael Otten has produced a stimulating and informative historical study of students' relations to administrative authority in the century-old University of California at Berkeley. To his considerable credit, he has provided much-needed historical perspective on the recent stormy years at Berkeley, and he has done this against an explicit theoretical perspective. Otten's premise is that "the underlying issues are the same" (p. 2) in most student "unrest" of recent years. The basic "issue" is "the very legitimacy of academic authority" (p. 4) as a structure of morally based power. Against this framework, he tells the Berkeley story with rich specificity and extensive documentation—from publications of the time and from some previously unavailable official files. It is a fascinating record, filled with revealing contrasts with and parallels to contemporary conflicts.

The story is organized into periods. During an early three decades (1869–99) of “authoritarian paternalism,” unquestioned officials of a “vulnerable” and “reputation”-conscious young institution exacted swift, informal retribution against the student rowdies of a predominantly “collegiate” campus culture. There followed 20 years of “paternalistic self-government” (1899–1919) coinciding with the presidency of Benjamin Ide Wheeler, who associated actively with students, granted them a genuine (though narrow) area of decision making, and skillfully manipulated campus “public opinion” in the interests of “this University . . . the family’s glorious old mother” (p. 39). The effects of World War I, the university’s size (10,700 by 1920), and a tide of “literary dissent” against “cultural barbarism” led next to a decade’s “decline of student authority” (1919–30), under the presidency of legalistic General David Barrows. The 1930s saw the birth of student political radicalism, the first “free speech strike” (p. 110), and the first explicit student challenges to administrative authority as such. During this period, President Robert Gordon Sproul established the claim of the University’s political “neutrality,” demanded an “educational” rationale for all campus activities, and greatly expanded the scope of formal, written administrative regulations governing student conduct. This pattern of “paternalistic bureaucracy” lasted through the post-World War II years of student political apathy and California-style Communist baiting. In 1956 a “liberalization” of student conduct regulations was begun, but these changes in general rules only reinforced administrators’ wide “discretion” in specific cases. Clark Kerr’s presidency, beginning in 1958, extended the formalizing and “secularizing” of campus authority into a “managerial bureaucracy.” This was finally disrupted by the Free Speech Movement of 1964, when open challenge to the good faith of benevolent administrators shattered all pretense of “consensus” in the University’s massive, internally diverse “community.”

Despite this record, Otten sees possibilities for university democratization in the growth of “legal-rational” authority: Because administrative as well as other “actions can be judged in the light of their consistency with stated principles” in a legal-rational order (p. 198), there is (“theoretically at least”) a built-in potential for movement away from “managerialism” toward “private government”—in which control is “conditioned by institutionalized acknowledgment of the consent and the rights of the governed” (p. 196). This potential, however, “is not likely to become a reality in the near future” (p. 201), because of open student conflicts with regents and public, and because “even within the University some constituents—principally the faculty—are not anxious for a structural revolution of this sort” (pp. 202–4). Otten sees, instead, a “political managerialism” (which he does not here define) as the “next phase of university control” (p. 206).

The book’s analysis is strongest, I believe, in fitting the historical materials to a broad, Weber/Durkheim model of evolving “forms” of authority, involving both official justifications and the changing “social

context" of student life. It seems to me least satisfying in its concluding analytic section, which discusses both the internal dynamics of campus authority and the "public pressures" of "outside" influentials—but substantially in separation from each other. One consequence is that the section says little about the importance of campus administrators for future developments, despite clear earlier indications that it is administrators who orchestrate the claims of "a self-contained rationality" (p. 194) and transmit the "public pressures," in a legal-rational system. The tendency of administrators to find ever-subtler euphemisms for "institutional reputation" also is given little attention, despite its part in producing the charge of "hypocrisy" against both administrators and faculty, destroying the "trust" said to be so vital for stable authority (see pp. 58, 178). Similarly, academic bureaucracy is implicitly treated here more as a structure of explicit rules and principles of conduct than as a structure of rule-protected *jurisdictions*, within which status groups exercise largely unfettered "discretion" in support of complementary interests. Greater emphasis on jurisdictional "discretion" would have pointed beyond the "potentiality for private government" in "legal-rational authority" as such, to some of the structural requisites for realizing this potential in practice. For example, students may need new legal skills and effective organization for bargaining collectively if they are to create a new balance of power within which "reasoned criticism" can do its sweet work. In my view, it is a valuable book that brings historical detail and analysis so close to these issues.

Being and Doing. By Marcus G. Raskin. New York: Random House, 1971. Pp. xxviii+450. \$10.00.

Robert Blauner

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Marcus Raskin's book is an ambitious effort to provide a large-scale analysis of contemporary American society, combined with a philosophy and practice for its essential transformation, or *reconstruction*—to use the author's own term. Raskin, a political philosopher, cuts through the boundaries of specialized disciplines and the academic norm which separates theory and practice, feeling and intellect. His study is a synthesis of political theory: an "existential pragmatist" philosophy with strong debts to Sartre and Dewey, Marcusean and other critiques of American society, the best insights of the New Left and the youth counterculture, and various action projects which he and his colleagues developed as staff members of the Institute for Policy Studies in Washington, D.C.

On the level of social analysis, *Being and Doing* makes an important contribution to the general study of social oppression. Raskin's master concept is colonization. American society is approached in terms of colonizers and colonized. The major institutions in public life colonize reality

as a whole as well as people because they are based on pyramidal structures of domination and on *characterization*, the freezing of persons into institutional roles, self-definitions, and views of the social order. The state is the Violence Colony, work life takes place within the Plantation Colony, the schools make up the Channelling Colony, the mass media constitute a Dream Colony.

In Raskin's view, then, the vast majority of the American people, including the white middle classes, are oppressed because their lives and experiences are not their own but instead are regimented and prepackaged through the core institutions. The book thus pushes forward a recent tendency to interpret colonization outside of its original historical context of colonialism and ethnic-racial domination. Though the stresses on the general processes and pervasiveness of oppression in American life is correct and essential, Raskin's approach tends to overlook or discount the importance of the specific ways in which certain social groups are dominated and exploited. Whereas he has some insightful comments to offer on the failure of class analysis to grasp the basic dynamics of American society, he does not confront the issue of racial oppression per se. In fact racism is neither a central feature of his model of how our society works nor a concept to be used in its analysis. Since Raskin appears to have borrowed the notion of colonization from contemporary Third World social thought and political rhetoric, he should at least have discussed the relationship between the model of racial colonialism in America and his own broader formulation.

Perhaps the greatest strength of *Being and Doing* is its vision of the possibilities for social and political action within the limited free space of present-day society. Raskin provides utopian images of what reconstructed institutions might look like as well as an abundance of imaginative proposals for immediate political projects that would contribute to such outcomes. Egalitarianism, the participation at decision-making levels by little people, the development of local and work-based communities, and nonviolence are the values which inform his approach to social reconstruction. There are serious omissions as well as inconsistencies in this study of American cultural and political dynamics; however, it is one of the few existing radical analyses that eschews both outdated sectarian ideologies and antitheoretical "revolutionary" rhetoric.

Imperialism. By George Lichtheim. New York: Praeger Publishers, 1970. Pp. vii+183. \$7.50 (cloth); \$2.45 (paper).

Irving Louis Horowitz

Rutgers University

There are prima facie difficulties in reviewing a work such as *Imperialism*. I would like to share them with the readers at the outset. First, this is not a work of social science; it neither presents a thesis nor gives evi-

dence in its support or refutation; hence, the criteria for measuring the worth of the book are its internal logic and powers of deduction. Second, this is not so much a study on the theme of imperialism as a sustained, and at times brilliant, critique of the Leninist theory of imperialism; hence, it belongs to a species of intellectual history peculiar to the Marxist dialectic rather than the sociological function. Third, for the most part, I admit to a lack of sympathy with this sort of overblown apologetics that simply refuses to take seriously that *all* ideology must adjust to the real world—and that includes even the *petit bourgeois* view of the world common to intellectuals dedicated to Weimar social democracy.

The work under consideration is described by its author modestly enough as an "essay." Although it contains nine chapters and is 183 pages in length, in the classical sense of a theme covering one subject in one style and in one sustained mood, this is indeed an extended essay. Properly speaking, this is the missing link in the analysis given a decade earlier by Lichtheim in his excellent work on *Marxism*. And here we are presented with the first problem. Whereas the earlier volume was an effort at understanding and interpretation, this is a study in criticism and polemic. For Lichtheim must demonstrate that imperialism is not properly a part of Marxist studies; yet he must somehow come to terms with imperialism precisely because it has become the central category of contemporary studies conducted by Marxists.

Essentially, Lichtheim's study is an attempt to establish a dualism between Marx and Lenin. Certainly this is not the first such effort, but it is probably one of the most cleverly wrought. It is his belief that the identification of Marxism with Leninism has become untenable precisely because of splits within the socialist camp that cannot be explained in Leninist terms. While at one level he is no doubt correct, the reasoning is somewhat obscure. For if an explanation put forth 50 years ago by Lenin cannot explain the contemporary pluralization of socialism, why an explanation put forth 100 years ago by Marx "is too important to be left to the post-Leninist sects" (p. 11) is not really grappled with. If at the beginning of this extended essay Lichtheim works to preserve Marxism from the dead hand of Bolshevism, by the end his concern is to preserve Marx from any effort of modernization at the hands of the New Left. "Among new recruits adhering to the radical left, the term 'imperialism' is currently employed interchangeably to designate four quite different kinds of relationships which Lenin had been careful to distinguish: (1) national oppression of the sort practiced in the old dynastic East European empires before 1914-18; (2) colonialism of the Anglo-Indian type during and after the mercantilist era; (3) "liberal imperialism," classically represented by the British and subsequently the American drive to throw foreign markets open to Western capital; (4) the transfer of surplus value from the poor countries to the rich through trade relationships which in practice discriminate against undeveloped economies" (p. 134). Here we have the anomaly of an author subjecting Lenin to criticism for being old-

fashioned and outmoded but not permitting the same sort of analysis to be made by Marx.

This fundamental faith in Marx, and assumption of the ideological disfiguration by Lenin, mars an otherwise interesting study. For while Lichtheim scores heavy points at the expense of neo-Marxian explanations of imperialism—illogicality, oversimplification, propagandistic tendentiousness, and dogmatism—the sad fact is that Lichtheim does nothing to dispel the myths by creating an alternative relevant theory of imperialism. However, this work is fortunately more than a polemic; it is, in its essence, an effort at historical debunking. As a result, its chapters on history are by far the most significant. Some of the more telling points, in an effort to indicate the weaknesses of the Leninist theory of imperialism, are as follows. The irony of British history is such that although constantly victorious in war, it is constantly being defeated by its allies in the peace that follows, and its imperialist pretensions collapse through victory no less than German imperial pretensions collapse through defeat. Britain, along with the rest of the Western European world, reveals a situation in which capital invested abroad continues to yield a growing return, but such capital investments no longer suffice to cover the deficits resulting from the export balance. And this is as true for the victorious as for the defeated countries. This further means that the character of imperialism itself has changed from the extracting of mineral wealth to the export of finished commodity goods.

Throughout, Lichtheim points out, and with special clarity in his chapter on "Imperialism and Nationalism," that the latter is a force in its own right; it did not simply serve to buttress imperialism but, quite the contrary, frustrated the aims of imperialism (p. 92). Earlier, he points out that "the notion that an empire must necessarily consist of overseas colonies is a fantasy" (p. 58), as is, in fact, the whole notion that nation building also breaks up any imperial collusion and creates rivalries along nationalistic lines precisely among those who—at least from a theoretical point of view—have the most to gain by an imperial concordat. A third major point that Lichtheim makes is that the process of imperialism did not lead to fissures between the proletariat and the bourgeoisie, but quite the contrary: as long as the European bourgeoisie retained its faith in the liberal democracy, the proletariat did likewise. And it is only when the middle classes turned toward fascism that the working classes, too, turned toward fascism, so that the imperialist phenomena did not create solidarity along class lines but rather reinforced national tendencies and even class cooperation.

One dilemma with the analysis, precisely when it is soundest, is that there are all kinds of quotations from Lenin that could have been adduced to support the very points Lichtheim is making, were he somehow less jaundiced toward his biographical subject. From the ironies of British history to the vagaries of European proletarian behavior, no more astute commentator or critic existed than Lenin himself. Indeed, Lenin pointed

out the very same reasons raised by Lichtheim for the socialist revolution taking place in backward Russia rather than in the advanced sectors of Western Europe.

Lichtheim is weakest when he leaves history and biography for the terrain of theory. It is simply not true, for example, that "the industrialization of the Third World is in fact the only major outlet still open to Western capitalism" (p. 145). What is, in fact, open is the modernization of the Third World in contrast with its industrialization. It is precisely the struggle between modernizing factions and industrializing factions that clearly divides most thinking in the Third World at the level of strategy and tactics. Involved here are fundamental differences between structuralist and monitorist explanations at the level of economics and militarist-civilian divisions at the level of politics.

It probably is true that "the Leninist model has become unworkable and that imperialist rivalry over spheres of interest is something qualitatively different from a global confrontation in which the very existence of mankind is at stake" (p. 155). However, it is interesting to note that the Chinese Maoist position, precisely in terms of Leninist doctrine, has been willing to accept the process of global confrontation as a necessity in the event imperialism is not done away with. Hence, Lichtheim's uneasy conclusion to the book would make more sense within a Leninist context than within any other. For Lichtheim's fears of nuclear war seem less well grounded in view of the Chinese nuclear striking power than at an earlier period. In other words, Lichtheim makes a classical error of his own: the assumption that new technology somehow overthrows all existing class and national relationships, when in fact, the very balance of such technology only recreates the need for even greater awareness of such phenomena as imperialism and colonialism.

The last sentence of the book itself is a high irony. Lichtheim mentions that "the transformation of Asia, by whatever means undertaken, must alter the world balance of power, and history tells us that changes of this magnitude are rarely accomplished peacefully" (p. 169). This undoubtedly is true in the abstract, but one wonders what Lichtheim would make of the forthcoming détente between China and the United States. Perhaps meta-history tells us even less than Lichtheim adduces on its behalf.

When all is said and done, Lichtheim's European sensibility fails, or at least pales, in comparison with the plodding mathematical-model approach of a Johann Galtung to construct an operational model of imperialism ("A Structural Theory of Imperialism," *Journal of Peace Research* 2 [1971]: 81-117), and seems timid and tepid when compared with the admittedly naïve, even bungling efforts of young New Left economists and sociologists to find a way to connect their experience of repression with the objective facts of repression (*Readings in U.S. Imperialism*, ed. K. T. Fann and D. C. Hodges [Boston: Sargent, 1971]). For, ultimately, this work of Lichtheim's is a profound retreat from rather than an addition to his earlier work on Marxism—since it is in the 19th-century tradition of sophisticated debunking characterized by Eugen Böhm-Bawerk, rather than the 20th-century

scientific effort to determine the nature of international political economy. It is this stance of learned indifference to the problem of international exploitation, rather than any single blemish, which ultimately dooms the book as a work of scholarship.

The Rise of Free Trade Imperialism: The Empire of Free Trade, and Imperialism, 1750-1850. By Bernard Semmel. Cambridge: Cambridge University Press, 1970. Pp. x+250. \$11.50.

Bert F. Hoselitz

University of Chicago

The book by Bernard Semmel discussed in this review is really the other side of his earlier book, *Imperialism and Social Reform*. Whereas there he commences with imperialism and writes about social reform as a mitigating circumstance, in this book he starts with the rise of free trade and imperialism, especially beginning in England under the influence of Wakefield, and it becomes a leading theme. But this is not the real beginning of the book. In the first two chapters and continuing at intervals throughout the book, he discusses a doctrine which was essentially unknown to me. This is the physiocratic content of the theories of Malthus and the essential role it played there.

I believe that the teaching of the physiocrats was pretty much dead after the 1770s and only rediscovered on the one hand by Marx and on the other hand by students who wanted to inform themselves on French agrarian history during the 18th century. I now find all through his life Malthus was quite deeply engaged in physiocratic argument. For example, in the second edition of his *Essay* he declares, "The great position of the Economists [physiocrats] will always remain true, that the surplus produce of the cultivators is the great fund which ultimately pays all those who are not employed upon the land." This, of course, is Quesnay's theory. It emphasizes the productivity of agriculture and leaves out the productivity of industry. Why this theory evolved it is difficult to say. Probably the technological conditions existing in France in the 1760s and the 1770s relied so heavily on agriculture that the change in this picture, which occurred in the 1810s and 1820s in England, could not be foreseen. In England, by that time, the main cause for making labor productive was industry and not agriculture.

It is very interesting to me that Malthus was so strongly influenced by physiocracy, and that therefore many of his arguments against the classic economists seem to make more sense, even though they are still debatable.

Another economist who is given more space than he usually is is Josiah Tucker. Tucker is generally dismissed as a person who in the 18th century wrote several pamphlets, but the position Semmel gives to Tucker in the first chapter shows that he was much more than a mere repetitious pamphleteer.

The third economist emphasized is Wakefield. The book covers very adequately the theories of Wakefield and his party. The principle of Wakefield's school was the selling of colonial lands by the government to adequate bidders, instead of granting them freely or at a nominal price. In Wakefield's theory, this required in the English colonies the setting up of financially capable colonial societies, which would in turn sell their land to relatively poor English farmers. In this way the more able farmers who did not have much money would be encouraged to emigrate from England to the various colonies, where they constituted the backbone of the societies there set up.

Emigration to the colonies, which was Wakefield's solution to the problem of overpopulation in England, was a valid theory. It is perhaps best described in the following passage:

So, while the party of Hume and Cobden defended free trade as the one grand remedy for all Britain's problems, following, in this, certain arguments, with modifications, of Tucker, Smith, and Ricardo, the "Philosophers" of the Wakefield school insisted upon the need for both free trade and a continuing program of colonization if England were to avoid the calamities which threatened an advancing industrial and commercial system. If the party of Hume and Cobden could optimistically view an international free trade as fulfilling the conditions for the harmonious operation of Say's Law, the school of Wakefield, following the more physiocratic strains of Smith, Malthus, and Chalmers, saw the continuing necessities for empire-building if a stultifying glut of goods, men, and capital, were to be overcome.

If this thought is carried to extremes, clearly a sort of imperialism emerges. Semmel provides only a short chapter on imperialism in the 19th and 20th centuries. No more is needed because his view on imperialism emerges clearly throughout the book.

On the whole, this book, for someone who likes economic and sociological concatenation, is very helpful. Semmel shows at each introduction of a "new" author what changes his theories made and in what direction. For anyone who wants to become knowledgeable about the real background of imperialism in England, this book is a must.

Political Anthropology. By Georges Balandier. Translated from the French by A. M. Sheridan Smith. New York: Pantheon Books 1970. Pp. viii+214. \$6.95.

Marc J. Swartz

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The ancient lineage of political studies is well known, and the inability of these studies to enlighten our understanding of politics, probably equally so. The possibility of understanding politics when it is approached as a separate field of study exists, but success in doing this has been meager even by the canons of social science in general. This may partially ac-

count for the rise of interest in politically oriented studies by sociologists and anthropologists. Anthropologists have, in fact, been seriously, if only occasionally and rather unsystematically, interested in the political aspects of nonindustrial societies for a very long time, but it is only since the 1940s that this interest has been vigorously pursued.

In his recent book, *Political Anthropology*, Georges Balandier presents a comprehensive review of what anthropologists have done in the field of politics. It is a careful translation of his 1967 *Anthropologie politique*, published by Presses Universitaires de France. The perspective of the review is based upon the premise that power relations exist in all societies and always arise from hierarchical relations between groups and individuals. He conceives the task of political anthropology to be "to reveal the particular forms adopted by political power *and* [author's italics] the inequalities on which it relies in the so-called 'exotic' societies" (p. 78).

In this task he considers studies of kinship, social stratification, and religion (almost all from tribal Africa) from the viewpoint of what they have to contribute to our understanding of inequality in social relations and, hence, of power under various social and cultural conditions. He has a deep interest in the evolution of the state, which stems from his assumption that historical development always involves an increase in the number and type of "dissymmetries affecting social relations" and that as these become more numerous social relations require that they be ordered in relation to each other. He follows Engels's view that "the state is born from society; it appears when society is in 'insoluble contradiction with itself' and its function is to reduce the conflict by keeping it within the bounds of order" (p. 156).

Balandier's concern with the social and cultural sources of political relations and activity is fruitful, and his thoughtful application of Marxian principles to the analysis of the sources and operation of power in societies, which often possess minimal economic differentiation, is stimulating.

Still, this book is less than wholly satisfactory. Tautological as it may be, it is worthwhile to point out as he does that power can be viewed as stemming from hierarchy or "dissymmetries" (and vice versa); but what aspects of what sorts of hierarchies promote power of what particular kinds and amounts? Balandier does indicate some of the differences between religious and secular hierarchies with respect to the sorts of power they generate, but this is a topic which needs to be pursued more vigorously. To note that differences in hierarchical bases lead to differences in types of power is useful, but to stop short of tracing in detail the particular social and cultural processes that produce and maintain particular sorts and quantities of power advances our understanding in only a very limited way. A full integration of the study of society, culture, and personality with concern for political processes remains to be accomplished or, even, undertaken in a determined and sophisticated way. Balandier provides an overview of what has been done in political anthropology and a

clear statement of one synthetic perspective, but he shows us less of the complexity of the problems and of the ways they might be approached than one might wish.

The Politics of Unreason: Right Wing Extremism in America, 1790-1970. By Seymour Martin Lipset and Earl Raab. New York: Harper & Row, 1970. Pp. xlv+547. \$12.50.

Joseph R. Gusfield

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Following the flamboyant career of Joseph McCarthy, it became fashionable in intellectual and academic circles to speak of a "radical right" and of right-wing extremism. The senior author of this significant work was among those who directed attention to the seeming paradox of the radicalism of the right. In this grandly conceived and grandly executed study, Lipset and Raab have presented the most thorough investigation of American right-wing "extremism" to date. They have put the flesh of empirical data on the bare bones of a concept, and inadvertently they have displayed the weakness of that skeletal structure for the understanding of American politics.

I have put the term "extremism" in quotes because I find many difficulties with the concept and with the spatial metaphor of right and left that underlies their framework of thought. Even in this highly sophisticated and cogent study, it is a difficult concept at best. In Lipset's and Raab's usage it contrasts with a political attitude which grants legitimacy to differences and to dissent; it is an attitude which seeks to repress "the open market place." "Extremism basically describes that impulse which is inimical to a pluralism of interests and groups, inimical to a system of many nonsubmissive centers of power and areas of privacy" (p. 6). The authors recognize, however, that this also characterizes the Left at many times in American life. They find the hallmark of the Right in the reaction against changes which presage displacements of power and status; the Right is preservative, its theme is the politics of backlash.

This thesis becomes Lipset's and Raab's analytical instrument for their dissection of nativist bigotry, which they use operationally as the subject matter of the study. It is the Know-Nothings, the Anti-Masons, the KKK, the American Protective Association, the Coughlinites, McCarthyists, Birchers, and Wallaceites who make up the movements they have chosen for analysis. The anti-Catholic, anti-Semitic, anti-Negro sentiments are chief among the impulses they have selected as the measures of intolerance.

There is a breathtaking quality to the range and audacity of this work that makes it so significant. We have grown accustomed to the method of exhaustive search for comparative studies, to the historical scope and the inventive use of survey data associated with Lipset's past work. It has produced much of the major work in the political sociology of this gen-

eration. Here it is at its fullest power, culling from the historians of the 19th century and from current survey research a vast, almost unimagined amount of work on the social bases of American movements. To this gold mine of data the authors have added the results of a survey of the American electorate in 1968, conducted by the Berkeley Survey Research Center for the authors and for the other investigators involved in the Anti-Defamation League's five-year study of anti-Semitism. (This book is one of five emerging from that work.) The three chapters on the Wallace movement are perhaps the most valuable and original of the materials presented here.

The George Wallace movement is for Lipset and Raab the epitome of right-wing extremist movements of American history. It has its roots in the archetype of American backlash—the preservatist economic and status concerns of American nativist upper-middle-class components and the status-loss fears of white American working-class groups. In its conspiracy theory of events, in its nativist anti-Negroism, and in its fundamentalist moralism the diverse class and cultural strands find a unity. Its working-class populism and economic welfare liberalism clash with the upper-middle-class anti-statist and conservative economics associated with the Right. Like the earlier movements discussed by Lipset and Raab, bigotry and cultural fundamentalism are not the sources of the movement but are rather the cultural and symbolic baggage around which the status fears of the movement's adherents are given visibility. It is the fear of status displacement and the low level of democratic restraint that sparks the backlash of the Right. It is this "Quondam complex," this harking back toward the past, that distinguishes the extremism of the Right from that of the Left. The authors find a crucial difference, however, between the earlier, pre-1930s movements in which the targets were collective—Catholics, Jews, etc.—and those of the post-World War II period when the preservatism has become anomic and directed toward abstract targets—such as communism. Through such symbols the aggregate becomes united into a group by their mutual acts of opposition.

Despite the ingeniousness of theoretical formulation and data retrieval, I must shout, "Stop, enough, too much." Too much has been brought to bear on a flimsy structure. Two points seem to me salient and make the bulk of the Lipset-Raab theory and formulation less than convincing. First, the designation of preservatist interests is not specific enough or sufficiently comparative to account for the many negative cases. For example, why were Catholics so supportive of Father Coughlin in a period in which they were rising in the social and economic structure? Why were the Jews waning as targets of Rightist movements in the 1950s and 1960s when they had moved into a leading position in the intellectual and economic structure? Why should nonpreserving *nouveaux très riches*, like H. L. Hunt, be Rightist leaders? Is it not the case that many preservatist orientations are active in American politics without becoming as antithetical to dissent and opposition as the authors find in Rightist movements? Lipset's "preservatist" views on American college politics (with which I

am mainly in agreement) are well known. They have not moved him, nor many like him, to an "extremist" position. In any period of change some are losing and others winning. Politics is the arena of many of the conflicts arising from social change, and its openness to status conflicts is a part of the process of politics of "who gets what, when and how." Much preservatism is encased in traditional, centrist politics.

Second, if preservatism is not unique to or ubiquitous in "extremist" movements, neither is bigotry or low democratic restraints. The authors, in fact, present much data to demonstrate the generally low level of attitudinal support for pluralistic democracy in survey data. Their use of the term "bigotry" seems a peculiar one. Why is the Protestant Democrat who refused to vote for Kennedy any more of a bigot than the Catholic Republican who did? False beliefs, group loyalties, out-group animosities are the stuff of human life. Are not "class consciousness" and its attendant distortions and monisms similar to anti-Semitism and anti-Negroism? The authors suggest a pallid democratic electorate far more appropriate to academic seminars than to popular political life. In this they confuse pluralism as a diversity of groups and power with pluralism as a culture—as a set of norms and values. The latter, as DeTocqueville saw, is neither necessary nor universally existent in American democracy. Lipset and Raab have confused ideals with realities. Even the differences between extremist and nonextremist groups in survey responses are seldom large and glaring. To speak of movements as "nondemocratic" or "extremist" seems confusing when they have not been accompanied by repressive or violent acts. Despite much rhetoric, the politics of these movements has largely been confined to institutions of liberal politics.

I put down this weighty book with both satisfaction and disappointment. The satisfaction comes from reading an important, thorough, and excellent study. The disappointment comes from recognizing that the concepts of its concern—"right-wing extremism"—should now be abandoned as useful analytical tools. They distort as much as they clarify; they contain too much that is as yet uncontainable. Theory and data can blind as well as illuminate.

People, Power, Change: Movements of Social Transformation. By Luther P. Gerlach and Virginia H. Hine. Indianapolis: Bobbs-Merrill Co., 1970. Pp. xxiii+257. \$2.95 (paper).

Richard Forbes

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Although nominally a study of the psychosocial diffusion of Pentecostalism and "black power," only two short chapters in this book are given to cursory overviews of these "movements." The rest of the book is organized around the authors' "five factors of movement dynamics": organization, recruitment, commitment, ideology, and opposition. Unfortunately, the data

base here is insufficient and will make little contribution to the study of either of these subjects.

The contribution of the theoretical construct is more difficult to assess. The authors assert that "the role of the scientific investigator" is one of "the mere elucidation of a social process"—even if others might expect more—and that such elucidation is a "contribution to knowledge" (p. 200). However, the appeal to "knowledge" will not do. Nor will the authors' disclaimer that their subsequent recommendations represent a shift to "value judgement" that is "broader" than their scientifically "objective" approach.

Rather, the contribution here is implicitly to the knowledge of those who are concerned with the "threat" of "black power," or of those who are concerned about the stability and serenity of the present regime. This "threat" consists in the alleged "fact" that "a small, dedicated minority [blacks] can cause a nation to commit moral and institutional suicide" by stimulating a reactive "police state" (p. 206). To avoid a police state, the authors' "established order" (what an archaic, deliberately vague term!) has two options: genocide or co-optation of social movements. Being good liberals, of course, the authors are chary of proposing genocide. Instead they give us (or rather, "the established order") some proposals in the other direction: "It is only by looking *beyond* the movement to the radical social changes it foreshadows that *members of the established order can hope to direct these changes or participate in directing them* [my italics here], rather than default to movement participants" (p. 196). And we have the quixotic "solution": "The most effective response to a movement is another movement, for in times of rapid social change survival lies in not in stability but in flexibility" (p. 218).

It is of some importance that the first of these statements preceeds the authors' alleged switch to "value judgment" (which occurs at p. 200). The authors' bias toward the established order runs throughout their supposedly "objective" theory of social movements; in fact, the theory leads directly to such ridiculous proposals.

As noted, there are supposedly "five factors of movement dynamics" that are presented as specifically *internal* configurations of social movements: (a) "segmented organizational units linked . . . by various personal, organizational, and ideological ties"; (b) recruitment of friends through existing social networks; (c) personal commitment preceeded by separation from existing values; (d) ideology, which is a "comprehensive conceptual framework"; and (e) opposition from the "established order." The fact that the later is hardly the "internal dynamic" the authors say it is gives some impression of the difficulties of this formulation (see p. 199).

By concentrating on internal dynamics and upon the psychosocial variables involved, these authors largely fail in contributing a theory of "movements of social transformation." They are unable to place social movements in their social environments or to speak of them as historically generated. Referring to a vague concept of "generating conditions," the authors instead maintain that movements are successful when they become

completely autonomous: "According to our observations, when the five factors of movement dynamics are all present and interacting, the movement becomes independent of its generating conditions. When evangelistic face-to-face recruiting by truly committed individuals begins to create a network of organizational cells united by a shared ideology, the movement lifts off the launching pad of causal conditions [!] and becomes an *autonomous social entity*" (p. 195; my italics). Thus, social movements are supposedly autonomous not only from "generating conditions" but from other conditions as well! The obvious root of this difficulty is that the authors make the study of social movements a matter of mere psychology from which it follows that the present regime can create any old movement to counter any old movement that appears as a "threat." Presumably, the only obstacle is "lift-off."

Yet the authors contradict themselves on this rather crucial point. Stipulating that a social movement "is a social institution designed for implementing change," they say that "the form of the social changes brought about . . . depend[s] on the type of oppositional interaction between participants in the movement and members of the established order" (p. 196). Here one wonders how change is to take place at all if the established order is to field its own movements. It seems logically absurd to maintain, up to this point at least, some kind of opposition or contradiction, then to suggest that an "established order" engage in *autonomous* "oppositional interaction" *with itself*.

Overall, the difficulties in this effort stem from the author's inability to deal with the material conditions and/or resources of social movements and from their failure to see that a social movement is a concrete historical process. Instead, they have gone on a fishing expedition for a "causative principle," which their theory fails to supply. Their use of the vague term "established order" is merely a pretension to scientific objectivity and in fact represents a regression to mysticism and wish fulfillment.

It would be very nice if the objectives of social movements could be peacefully implemented. Then we would have the liberal utopia. But it is a patent contradiction to assert that they can be implemented by the very persons who are the targets of change—especially without first demonstrating how the "educator" is to be "educated." This book does not fill *that* bill either.

Social Fragmentation and Political Hostility: An Austrian Case Study.
By G. Bingham Powell, Jr. Stanford, Calif.: Stanford University Press,
1970. Pp. xiv+207. \$7.50.

Dwayne Marvick

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This is a case study of political life in a small industrial Austrian city. In early 1967, the author carried out a sample survey of 200-plus citizens

in Hallein; from his base in nearby Salzburg both before and after his survey he visited the city, talked with party and municipal officials, studied budgetary data, and browsed through newspaper files. From his survey, data were obtained about Hallein citizens: their partisan sentiments, issue orientations, group memberships, social cleavage positions, and the sense of political hostility and distrust manifested by SPO and OVP adherents toward one another—the so-called *lagerdenken* mentality which others have seen as pronounced in postwar Austria. Much of his text seeks to use these data to clarify how “social fragmentation” and “political hostility” affect one another, not only in the way citizens view their community but also in the performance of local political elites. A rather useful analysis of the municipal budget is made, highlighting the ways in which structural constraints affect the locality’s “system capabilities.” Patterns of city-council and party decision making are described, as exemplified in a small number of political struggles that arose in the 1960s, but Powell’s examination of elite cleavage lines does not rest upon any hard evidence. The study is not rich in contextual details; it is neither historical nor developmental in its handling of materials; the data base used is static, thin, and disappointing.

Can a more favorable judgment rest on its theoretical contribution? The basic theory of fragmentation is said to be a flourishing body of knowledge, but “more accurate distinctions and more precise research are necessary before the theory of fragmentation can become fully useful” (p. 5). Only cursory discussion is made, however, either of the theory itself or the proposed augmentations thereto. Fragmentation theory appears to be an alias for pluralist theory, which is virtually equated with cross-cutting memberships and their benign effects. By contrast, fragmentation theory focuses on cumulative group memberships and their malign political consequences—the spiraling and destructive forces that can tear a community apart when its subcultures nourish feelings of in-group isolation and when intergroup antagonisms are projected into politics and even exacerbated by the political rivals who champion them.

Powell’s strategy is to use theory to guide his exposition of the single case, and to search for unanticipated features of the specific case to “inform” the development of the basic theory. Through his presentation of “basic theory” in its “simplified version,” complications do emerge requiring more sophisticated reformulations of that theory. It is difficult, of course, to find authors who can be quoted as sources for the “simplified version,” so none are. Instead, in the first discussion, the sequence runs like this: specific points actually made by Dahl and Verba are briefly examined; next, reference is made to a line of argument by Converse and Lijphart that challenges “pluralist theory on other levels as well” (p. 4). In 150 words four working hypotheses are set forth for discussion. To round out the search for more incisive theory, it is then noted that social fragmentation probably will affect political conflict differentially, depending upon such undefined conditions as subculture awareness, political intensity, social segmentation, cumulative group affiliations, mass hostility,

elite perceptions of mass attitudes, system instability, and system malperformance. None of these intervening factors is seriously examined, then or later, either theoretically or empirically, to clarify how they might qualify the link between fragmentation and hostility. Later excursions onto theoretical ice are similarly hard to pin down.

The poverty of Powell's theoretical effort is as distressing as the flimsiness of his data base. Hostility—his key variable—is measured by two questions; it means distrust of the rival party should it come to power *nationally*, and/or it means aversion to cross-party marriages involving the respondent's family. Applied to Hallein, the dangerous implications of *lagerdenken* are difficult to trace. Even among partisans locked into a socialist or a Catholic subcultural matrix of cumulative group isolation, only one-fourth worried about the rival party's coming to power nationally. Fewer still were distressed at what might happen to their son or daughter. In the sample as a whole, about one-eighth could be scored as politically hostile—at least on the functionally relevant plane of running the national government, where such a level might well seem surprisingly low, given the rhetoric of modern politics. Still, partisans under cross-pressures were less likely than those under cumulative pressures to register hostility. With findings like these, since the author is able to assert that they "unambiguously confirm the hypothesis," who needs evidence (p. 39)? Six pages later, after failing to show that either class or religion alter the hostility levels among Hallein citizens, he argues that "these findings provide increased contemporary relevance for Marx's predictions of class conflict."

His closing suggestion is an understatement. "The nature of political hostility itself . . . should be subjected to a careful analysis, using more elaborate and more numerous attitudinal indicators than were used here" (p. 143).

Pont-de-Montvert: Social Structure and Politics in a French Village, 1700–1914. By Patrice L.-R. Higonnet. Cambridge, Mass.: Harvard University Press, 1971. Pp. xv+217. \$8.50.

Richard Robbins

University of Massachusetts

With no trace of either imperiousness or condescension, sociologists can make two demands upon a historian who undertakes a monograph on the history of a local community and who is inclined to move beyond straightforward chronology. Horizontally, in terms of place, we want to know the extent to which the work succeeds in linking up the village or town with larger structural units—marketing complex, region, nation. Vertically, in terms of the time frame chosen, we want to know whether the succession of events is adequately set in a social context sufficiently broad to provide coherence and continuity for the narrative. *Pont-de-Montvert*, the study of a small, relatively isolated Protestant village of the Lozère department

in south central France, is good, crisp descriptive history. However, Higgonet essays to meet the more general sociological requirements just outlined; and it is not so much criticism as amplification to say that his book leaves some questions unanswered, others unposed.

In summary, the 200 years of village history are described as an interplay of geography, climate, economic resources, class structure, religion, and politics. Until well into the 19th century, harsh climate and forbidding mountains kept the village isolated. Also, in the 1700s the economy, while not altogether self-enclosed, did not mesh tightly with the regional system. At the threshold of the 18th century what counted internally was the class structure: the pyramid of landless peasants, marginal landholders, smaller clusters of artisans, emergent bourgeois, and notables or gentry. Religiously, the threshold was defined by the bloody Camisard Wars (roughly 1702–10) when the Catholic troops of the monarchy carried out the Protestant repression; the Montipontin poor fought and died, while the notables bought an accommodation. Yet, despite these burdens, despite abject poverty, the Montipontins held on, without significant change, through the Revolution and into the early 19th century, a vigorous example of mechanical solidarity for all the sharp class cleavage. But by the end of the 19th century decline had become dominant. "In 1900 the Pont was slowly bleeding to death." Paradoxically, decay stemmed in large part from improving economic and social conditions. The poor and the bourgeoisie could now emigrate more readily in search of different kinds of economic opportunity. In an increasingly commercial society the land base of the vital *gros paysans* shrank in value. Population slowly declined. Republican measures, such as education, opened up the class structure for those remaining. "Before they had been villagers first, and then Protestant, Languedocien, and French. Now they were French first, republican next, and the village was low on the list as a matrix of life." Higgonet is not trapped in nostalgia. He argues, rightly, that life in Pont-de-Montvert was on the whole impoverished and brutal in 1700, much more cushioned in 1914. Nonetheless, those who marched off to Verdun in 1914 were leaving a shell, not a community.

The story—"the simplification, decline, and eventual demise of a tripartite class structure based on the subsistence economy of a closed village"—is well told, with economy and grace. All the same, to return to my opening remarks, Higgonet does not seem to me to take full advantage of the opportunity hinted at in his afterward, "Village Studies and the History of France." There he is content principally to argue that Alfred Cobban's interpretation of the French Revolution as more a political-intellectual movement than a deep-rooted social upheaval probably fits the Pont better than the classic analyses of Michelet et al, and to argue that the decline of "the glory of rurality" in France was general, that the village was a variation on a theme. I wish Higgonet had moved on to an effort at constructing a closely reasoned typology of village-town-regionation in 19th-century France. Yet I concede this would have involved an emphasis on sociological history at the expense of local history, and local

history was his basic intention, not a "latticework" construction of the little community in the style of Robert Redfield. To invent a "folk" proverb that must exist somewhere in some village: "You should not ask the butcher for bread and the baker for meat."

Black Rioters. By Benjamin D. Singer, Richard W. Osborn, and James A. Geschwender. Lexington, Mass.: D. C. Heath & Co., 1970. Pp. ix+117. \$10.00.

Harold C. Bloom

University of Chicago

This book, in spite of its general title, is primarily a study of the July 1967 riot in Detroit. As the authors indicate, it suffers from both the advantages and disadvantages of instant research. The project was conceived in a day, during the latter part of the riot, and interviewing began as the riot was ending. Those interviewed were, first, a sample of 499 black male arrestees who were alleged to have participated in the riot and, second, a sample of 499 residents of the black community matched with the arrestee sample on the basis of sex, race, and locale.

In the first sections of the book, the authors review various approaches, theories, and findings relating to racial violence and conclude that Neil Smelser's value-added approach (*Theory of Collective Behavior* [New York: Free Press, 1963]) provides a useful framework for understanding the material they have reviewed. They then go on to demonstrate how various structural factors in pre-riot Detroit can be placed into (or understood in terms of) Smelser's framework, and conclude that the conditions which Smelser specifies were present in Detroit before the riot. They are careful to point out that other cities which did not have riots also experienced the same conditions, and that therefore the factors should be considered necessary but not sufficient. Even this is overstating the case, for the presence of these conditions in one city which had a riot does not prove that they are necessary. What can be said is that the conditions preceeding the Detroit riot do not invalidate the value-added hypothesis. It is difficult to construct or test theories on the basis of post hoc analysis of one case.

More interesting than the attempt to demonstrate a general theory of riots are the results of the interviews with the arrestees, after one realizes that they are not necessarily representative of riot participants. Data are presented on how they found out about the riot (mostly through direct experience or from another person rather than through the mass media) as well as what the content of the message was. Also included are data on whether the respondents had seen other riots on TV, what they remembered about them, and how they felt when seeing them. In these chapters, as well as in chapters about sociodemographic variations and social isolation, comparisons are made between the arrestee sample and the community sample. The style of these chapters makes it easy for the reader to believe

that the authors are making comparisons between rioters and nonrioters; while this is never actually done, it is difficult to see another reason for including the community sample unless the book is a study of factors affecting the probability of getting arrested. The problem is that there are no data about whether members of the community sample participated in the riot, and it is thus difficult to draw meaningful conclusions from the comparisons.

Although the book is flawed by this methodological problem (probably because of the instant-research design), it still makes a useful contribution to the study of collective behavior; the authors managed to interview the arrestees quickly and gather data undistorted by the passage of time. This advantage is put to best use in the discussion of communication—it is more than likely that the respondents accurately remembered what information brought them to the riot area and how they obtained it. If the interviews had been carried out after a long period of planning, the accuracy of the respondents' memories would have become a significant question. It is unfortunate, however, that methodological rigor had to be sacrificed under the pressure of time.

City Politics: A Study of Leopoldville, 1962–63. By J. S. La Fontaine. Cambridge: Cambridge University Press, 1970. Pp. xiii+247. \$13.50.

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City University of New York, Brooklyn

In a period in which urban sociology is receiving more attention than it probably ever has in the past, it is perhaps unnecessary to explain why cities in one part of the world are especially interesting. Yet, African cities manifest a number of characteristics which put them in a special category, and among them Kinshasa (formerly called Leopoldville) is a choice example.

First, there is the recency of origin. The city of Kinshasa, like so many other African cities, was founded by European colonizers; and not only was there no urban center on its location before then, but the whole society had not produced cities in the modern sense of the word. Added to this newness is the fact that the "city" was not only an imported social organization but that it initially served an exclusively colonial function. Nonetheless, when hundreds of thousands of Africans settled there, a vast array of peculiarly African urban social processes were "invented." In one sense, the urban-rural split is, therefore, much greater than in most other parts of the developing world. Yet, paradoxically, the impact of colonial rule on sub-Saharan Africa has often been so great, and population mobility so widespread, that this young city has not only invaded the countryside but often overwhelmed rural culture. For instance, the needs of the colonizer and the city produced the modern elite; yet today it is not unusual for rural society even in remote villages to accept the skills of the modern elite as a requirement for all levels of leadership. The alienation

from rural life which this has produced, especially among the young, is again special because of its magnitude.

Kinshasa is one of the largest of Africa's cities. Its population growth has been so rapid that it has nearly tripled in the last 10 years. It is the capital city of one of the two or three states with real industrial potential. It was the arena in which one of the most spectacular independence struggles was born and developed, to be followed by a unique and tempestuous postindependence evolution.

In sum, J. S. La Fontaine has set her sights on a complex, difficult, but infinitely exciting subject. She has had the wisdom to approach the subject in a holistic manner, so that the reader gets a glimpse of the history of various social groups as well as an analysis of the dynamics of their interactions. The author offers a full breakdown of the population according to size, age, sex, and ethnic affiliation. The city's economy and economic role in the country and the structure of its government are also described. A major part of the book deals with social relationships among the city's population. This section includes a description of religious groups. The last part of the book deals with political parties and also offers a few biographical sketches of leaders who were important during the independence struggle and immediately after.

Inevitably a book such as this will repeat much information already known to someone familiar with the Congo (now Zaïre). In terms of original research the most valuable contributions are drawn from the author's survey of households in the city. She also draws much interesting information from M.A. theses by Congolese students which are practically unavailable to scholars outside the Congo.

But, despite these assets, the book is disappointing. First, one is struck by the absurdity of publishing a book in 1970 which deals with events in 1962-63 in the present tense. For example, we are told that Limete, a residential suburb, "now has a mixed population, with a high proportion of United Nations officials" (p. 23). Readers will be interested to know that UN officials left the Congo in 1963-64.

There are uncountable factual errors. Kasavubu did not demand "immediate independence" in 1956 (p. 13). Thomas Kanza was not "actively involved in the Mutelist [*sic*] rising in eastern Congo" (p. 222). The Mulelist rising, incidentally, was in the west. It goes without saying that Kanza's activities can therefore not be employed as some sort of evidence that "the passage of time has confirmed some of my hypotheses" (p. ix), as the author seems to claim. Patrice Lumumba's party did not obtain a majority in the May 1960 elections, as the author claims was "no doubt" (p. 14) the case. Kamitatu did not preside "over the Leopoldville branch" of the PSA (p. 198). The Parti du Peuple was not representative of "conservative elements in the city" (p. 199). The list could go on and on.

Another basic criticism which has to be made is that the author is so often unscholarly in her assertions and conclusions. For instance, it is astounding to hear an anthropologist state: "the fact that rights in parcels are heritable has meant that there is a section of the population

born in town who consider Leopoldville their true home: . . . They are Leopoldvilleois . . . rather than tribesmen" (pp. 59–60). Is the author unaware that ethnicity continues to form a major, if not *the* major, social and political loyalty among African urban populations? The term "tribe" is, incidentally, employed in the loosest, most undefined fashion.

The author is also in the habit of making assertions about what "is" believed or claimed without citing sources or any supporting data. For instance, "the Congolese . . . claim that many Luba women are successful prostitutes and were often concubines of Europeans" (p. 44). Which Congolese? Are they representative? Is it a fair description? On these questions the reader is left to his own resources. Or, concerning Kamitatu's tenure as president of Leopoldville Province, "It is said that he used his influence . . . to consolidate his position with urban migrants . . . by indulging in a largescale allocation of parcels to those . . . with . . . a PSA membership card" (p. 198). Or "in Dendale it was claimed that fifteen out of about twenty of the staff of the commune were of the same tribe as the bourmestre" (p. 71). Who said? Who claimed? These are, after all, serious and damaging assertions. Why were they not tested or checked? And, apart from scholarship, should a foreign researcher include such views so lightly in an academic study?

La Fontaine also has trouble balancing her material properly. A good example is her description of religion in Leopoldville, which starts by claiming that "two types of religious organisations are important . . . the first type consists of Moslem and Kimbanguist religions: the second comprises the major Christian denomination" (p. 84). First, why this dichotomy? Second, we draw the inference that the Moslems are important and that the Kimbanguists are not Christian. But both conclusions are wrong. Indeed, having identified the Moslems as important, she then dismisses them with a superficial discussion, less than one page in length, entirely based on "some hours in conversation with the Fulani-speaking men" (p. 91) whom she (and a colleague who translated) met. Or, to cite another example, why are we given biographies of six leaders who, it is claimed, are "typical of successful urban leaders" (p. 216). In what way are they typical? How was the choice made? The fact is they are not typical of anything.

Finally, there is a quality of provincialism and—since the author is a lady—Western maternalism in the choice of words which is again hard to explain in an anthropologist. For example, "the climate of Leopoldville, *although* hot, is extremely wet" (p. 60). Or, in describing the social security derived from the extended family system, people are reported to be "eking out a precarious living by *begging* from relatives" (p. 50). Or "the *listless-ness and subdued chatter* in many compounds in the poorer parts of town are attributable to hunger" (p. 67). All italics are mine.

All this is capped by some not quite earthshaking conclusions. In a book published five years after a successful and lasting military coup, we are earnestly informed that "the importance of the armed forces as a factor in the Congo's politics can no longer be ignored" (p. 234). And then, on a

higher level of abstraction, "it is the main conclusion of this study that the political organisation of Leopoldville, within the framework provided by the Constitution [which one is that?] is the product of the activities of members of the elite who seek power" (p. 235).

The introduction makes clear that the author was encouraged to expand a limited but potentially interesting survey into a book because a new publications series on African cities welcomed manuscripts. Solid research on African cities is a crying need; it is a pity it cannot be produced by expanding monographs.

Power and Conflict in a Mexican Community: A Study of Political Integration. By Antonio Ugalde. Albuquerque: University of New Mexico Press, 1970. Pp. xiv+193. \$10.00.

Norman B. Whitten, Jr.

University of Illinois, Urbana

This expensively bound data report bears continuing testimony that lists of formal associations, tabulated membership and attendance records, statistics on office holding, and quotes from interviews may have nothing to do with the subject of research and therefore contribute nothing to an author's argument. Quick to evaluate and to label pejoratively, but seemingly unable to construct a meaningful generalization, Ugalde struggles through material on organized labor, bureaucracy, and political party affiliation in Ensenada, state of Baja California, Mexico, to try to understand local and national politics and to inform the reader about Mexican labor movements. His endeavor is far from successful.

Ugalde assumes that "conflict" (which he never defines) "exists in all social systems, and that the survival of the system requires the creation of conflict-solving mechanisms" (p. xvii). Ugalde is unable to move beyond this question-begging proposition. He relies on it in his study of labor "conflicts"—which are apparently any sort of argument, disagreement, dispute, or even negotiation at any level, between any two individuals or parties—particularly as disputes involve the national Partido Revolucionario Institucional (PRI) or its unsuccessful "competitor," the Partido de acción Nacional (PAN).

The setting for the study is a scenic international port a short drive down the coast from San Diego, California. By most indices of development, Baja California is one of the *least* developed states in Mexico. Ensenada, and the border towns Tijuana, Tecate, and Mexicali do see a lot of economic and political mobility; in the case of Ensenada three obvious factors stand out: international shipping, proximity to southern California, and tourism from the United States. Ugalde *minimizes* these factors and insists: "Baja California is probably the most highly developed state in Mexico" (p. xviii), and therefore "as other Mexican states

modernize they will follow political and organization patterns similar to those observed in this study" (p. 179). I find this perspective untenable.

Basically, the study finds that there are a great number of formal associations in Ensenada, including 101 labor unions with overlapping affiliations (*ejidatarios* are excluded). The main function of the unions is to "negotiate" with their local management. These unions are organized into five national labor confederations with local federations in Ensenada. The federations articulate economic demands to the federal government. Participation in the federations, Ugalde tells us, brings workers together and broadens their perspectives. This is said to be more important than confronting management with labor demands because workers themselves often hire other workers and hence assume a managerial role. Regarding the "labor movement" (formation of organizations that hold regular meetings, have officers, and broaden the perspective of the members) as a "microcosm of society" (p. 21) allows Ugalde to say that the union "movement" in Ensenada reduces the intensity of the "class struggle." But he does not tell us anything about what this class struggle might be. This is odd, since he also asserts that the "labor movement" is most cohesive and effective when "defending the interests of the lower socioeconomic strata of the community [presumably against the upper strata?] and acting as their representative, and also in matters of political support for the state and federal regimes" (p. 24). In a footnote to this statement the reader learns that such progovernment, pro-lower-class stance of the labor union is virtually unique in Mexico.

The tie between national federations and political parties and the local unions is primarily a downward flow of information, according to Ugalde. The national party (PRI) communicates through the federation to local parties, and union members learn a lot about the nation by going to meetings. The local unions also distribute small subsidies and receive canceled debts, land grants, and other material benefits in return for political allegiance to politicians in office. This, it seems, could be weighted as far more important than the "information flow" business, but Ugalde does not see fit to give such weights, for some reason—probably because he lacks any sense of theoretical consistency. Rather than try to reason through the system which he adumbrates, the author pejoratively labels the system as "corrupt."

Following a discussion of management and bureaucracy (chaps. 3 and 4) which fails to reveal the local and national structure within which labor movements must operate, and where some tables disguise more than they reveal (e.g., table 15, p. 97), the reader comes to the core of Ugalde's thesis: "one of the important dysfunctional effects of the regime's [PRI] dominance in the state as seen from the point of view of the provincial city is that the tax money collected by the state is not returned to the city in services, but is spent on offices that are sinecures" (p. 122). The governor creates the sinecures for people in the public sector, against the rich people (according to Ugalde) who pay taxes. The governor maintains his position

in the state through his personal contacts in Mexico City with high officials, including the president. One of Ugalde's "conclusions" from his study of Ensenada is that "the decision-making mechanism in Mexico City is extraordinarily personal" (p. 123).

Having established this point about federal, state, and local sinecures aimed against the wealthy taxpayers and supported by the labor movement, Ugalde concludes that the high support for PAN in Baja California arises from its braking effect on the PRI political brokerage and co-optation of labor. So far so good. For a page or two near the end of chapter 7, it appears that Ugalde will almost complete the discussion of community power and conflict resolution; but he fails. Having merely defined political brokerage, Ugalde quits and moves to a "conclusion" which is much more a venting of beliefs that democratization of the one-party system and economic centralization will be inimical to Mexican development than a careful drawing together of the materials tabulated, listed, and displayed. Tacking on unnecessary concepts of "countervailing power" and "pluralism" in the last few pages demonstrates the futility of social science jargon in the absence of a coherent theoretical framework of community, regional, and national structure. That many excellent models have developed from Latin American (including Mexican) studies detracts even more from Ugalde's effort.

The Action-Image of Society: On Cultural Politicization. By Alfred Willener. Translated from the French by A. M. Sheridan-Smith. World of Man Series, edited by R. D. Laing. New York: Pantheon Books, 1971. Pp. xv+366. \$10.00.

Vera L. Zolberg
University of Chicago

The events of May 1968 in France have produced such a plethora of literature reflecting efforts to grasp its reality that the proverbial elephant is unidimensional by comparison. Observers from near and far, participants or not, approving or deploring, have tried to cram the period into their own frameworks of reference, more often with passion and *parti-pris* than objectivity. To this cacophony, Alfred Willener, a Swiss sociologist, adds a stimulating though no less committed view of the failure of a political "revolution" which, for him, has nevertheless produced "culture" in the sense of new mentalities and outlooks and a reinvented world view.

Willener, who had studied industrial and organizational transformation and the images of society held by different social classes in Europe, was conducting research on middle-level managers in Paris when the explosion of May permitted him to observe their attempts to participate actively at a moment when "participation" had become the order of the day. In addition, with the aid of a hastily assembled team of helpers, he has pulled together an assortment of social data, including eyewitness accounts by student participants, a small ($N = 77$) on-the-spot attitude survey of

students, and a tape-recorded discussion by a group of intellectuals on the subject of cultural politicization.

These materials serve as a preface to an analysis of the events in terms of sociological formulations which have come to prominence in France in recent years: situationism and actionism. Willener sees his own approach as stemming from Sartre's challenge to "Marxists (but not Marxism), and certain forms of structuralism with their tendency to mechanistic determinism" (p. 105), as elaborated by Alain Touraine and others. He aims to unite social situations and individual existence as the sources of value creation in order to explain the phenomenon of May through a hermeneutic effort which deploys all its aspects from different viewpoints (p. 283).

Rejecting Raymond Aron's assessment of May as merely expressive of negative action, Willener analyzes the "action-image" resulting from the subjective interpretation of events in which actors are engaged and which develop into new meanings. These, in turn, are projected on the future to "illustrate" new possibilities, perhaps for emulation. In the process of analysis he draws parallels among political movements (the Movement of March 22, the Dutch Provos) and a number of 20th-century artistic movements (Dada, surrealism, "free jazz," especially in its Black Power incarnation, and the Living Theater, which, he believes, have in common emphases on spontaneity, attempts to "overthrow the rhythm of life," the breakdown of hierarchical or structural barriers, all serving to promote freedom for liberation and self-development in the fusion of the separated spheres of performer and public, imagination and action, art and life. The utopian "instant" political elements of May, he suggests, have their counterparts in the "Freedom Now" number of a jazz group and the "Paradise Now" of the Living Theater.

Unfortunately a number of questionable assumptions, assertions, and omissions reflect biases underlying the analysis. The artistic movements selected support his thesis, but contrary trends or movements difficult to pinpoint politically are left aside. Thus he stresses the Communist involvement of many surrealists (who were eventually rejected by the party), the Black Power militancy of some exponents of "free jazz," and the anti-bourgeois character of the Living Theater. He does not, however, mention offshoots of Dada which merged into fascism, or the de-differentiating, antirational, holistic ideology of the Stefan George circle, some of whose followers (though not by any means all) found happiness in Nazism.

Willener asserts that the political "revolution" of May failed to bring about "an immediate change in society, or an immediate solution of all the problems raised," a failure which "cannot be attributed to a basic weakness, but to one of tactics" (p. 189). The leaders had no intention of institutionalizing the revolution, since this would have destroyed its spontaneity and permanently provocative character. In other words, routinization of the charismatic aspects of the movement would have made it as sterile, boring, and alienating as the capitalist society depicted in Godard's film *Weekend*. This suggests that Willener shares viewpoints

entertained by members of the Movement of March 22 and Dadaists alike: politics and art are "simple" things in which all is possible (pp. 202-3). Yet he does not point to the structural components of society which make immediate changes unlikely, nor to the likelihood that even if structures were transformed, certain social problems might resist resolution because of their inherent complexity.

He is on firmer ground when he suggests that the "revolution" left a "subterranean" or "cultural residue" among students and intellectuals, which is likely to provide models of behavior for future revolutionary events. This observation (made pejoratively by Marx, who perceived echoes of earlier revolutions as transformed into parody in later ones) merits greater consideration, since the events may represent the creation and modification of cultural content, not to speak of the "general revolution in sensibility" which André Breton postulated as the aim of surrealism (p. 211).

One can agree with Willener that analytic understanding of "unrepressed sociological phenomena" is in its infancy (p. 286). His engrossing work, slightly enlarged in an almost flawless translation (p. 285, line 11, mis-translates *sans* as "with", and the choice of "I" for *nous* in a team effort is debatable), suggests a cultural model which should prove an indispensable starting point for students of collective behavior and dynamic social situations.

Social Status in the City. By Richard P. Coleman and Bernice L. Neugarten. San Francisco: Jossey-Bass Inc., 1971. Pp. xiv+320. \$9.75.

Edward O. Laumann

University of Michigan

If I had been asked to review this book in 1960, I would no doubt have welcomed it, although even then with some major reservations. But to wait another 11 years to see a study so clearly rooted in the methodology of the late forties and early fifties and so unresponsive to the excellent criticisms directed against the "Warner approach" to the study of community status systems that were beginning to accumulate even at the time the study was in the field (1952-55) leads me, from the vantage point of 1971, to be much more critical of what was done methodologically and to question some of its substantive results and interpretations as well. Critics of the Warner tradition have often questioned its assumption that small-town America could be treated as an accurate microcosm of modern urban America upon which to base inferences about social life in the cities. Very much in response to this well-justified criticism, a group of scholars at the University of Chicago, including W. Lloyd Warner, Robert J. Havighurst, and the authors, undertook a major field study of the Kansas City, Missouri, metropolitan area (with a population of some 850,000 in the early 1950s) in order to adapt "the methods

used earlier in studying small communities to the problems of studying the large city and in evaluating the extent to which a large city could be accurately described as a hierarchical status structure" (p. ix).

The book is organized into three major parts. The first is devoted to describing how the researchers studied Kansas City, identifying the dimensions of status perceived by Kansas Citians and developing their multidimensional Index of Urban Status (IUS) used there, a modified version of which is proposed as a general method for measuring social status in large urban settings. The second part is addressed to ethnographic descriptions of the five major social classes and the 13 substrata into which they are divided, while the final short section is given over to some speculations about the possible existence of a nationwide status system during the period 1925-60 and about the nature of various apparent changes in the American class structure in more recent years.

One might have some reservations about Kansas City as being ideal for their stated purposes, since it is not particularly "representative" of large American cities. That is, it has atypically low proportions of foreign stock, Negroes, and Catholics and very limited ethnic heterogeneity, and its economic and industrial base is rather more diversified and less unionized than is customary in Northern and Eastern cities. But I am rather inclined to agree with their view that these differences are probably not so crucial as to lead to totally erroneous conclusions about urban class systems. It is, however, important to keep in mind that Kansas City is more socially homogeneous with respect to ascriptive and religious composition than most large cities east of the Mississippi, and that this might facilitate greater consensus about the dimensions of the class-status system than would be found elsewhere.

From my point of view, a much more critical problem is the description and rationale of the multidimensional Index of Urban Status itself and the authors' general failure to incorporate in any serious way the recent advances in statistical and methodological techniques in building and evaluating such an index. (This actually reflects a more general problem throughout the book, in which relatively little work done outside the Warner tradition or since 1965 is cited, let alone taken at all seriously.) Starting with Warner's definition of the nature of social class (pp. 5-6), which emphasized *bounded* intimate interaction systems among social equals, eight dimensions (namely, occupation of male head of household, total family income [later replaced by occupation of female head of household or employed wife], neighborhood of residence, quality of housing, education of male head, education of female head or wife, church affiliation, and community associations [including the factor of ethnic identity]) are identified as being of crucial status-conferring significance to Kansas Citians as revealed by various informal and formal data-collecting devices. A seven-point scale for each dimension is devised such that, so we are to believe, score values are the same for all dimensions (that is, 1 is predictive of upper-class status, 2 is predictive of upper-middle core, and so on). A family's "status profile" on these eight dimensions is then

examined to determine subjectively (i.e., guided by one's unspecified intuitions reflective of his understanding of a given city's class structure as the natives see it) or objectively (i.e., averaging the scores) to which of the 13 strata the family belongs. Social class placement is presumably the criterion variable, akin to the earlier Warner Index of Evaluated Participation. We are informed, with not too convincing or clearly specified procedures or data, that these social classes are separated from one another by differential patterns of intimate association, although the real "breaks" are to be most clearly observed between the five major class groupings. We are further informed that, although the occupational scale itself has a correlation of .89 with the criterion variable—almost surely an overstatement of the degree of correlation, since the criterion variable is not estimated independently of the occupational information—and the other seven scales have correlations ranging only between .65 and .85 with social class placement, all eight must be used for optimal placement (p. 83). An intercorrelation matrix for the eight predictor scales is not given, nor is there any statistical evaluation of the independent contribution of these scales to justify the potential user's going to all the trouble of calibrating these eight dimensions for another city. The authors repeatedly stress the inadequacy of occupation status by itself as a single criterion of class placement; yet their own evidence suggests that it is the best single indicator, and their use of the other dimensions seems ad hoc and overly subjective at best. In short, utilization of more adequate scaling techniques and criteria would have greatly facilitated evaluation of the IUS. (On the basis of recent work on occupations, I think that the Occupation Status Scale (pp. 83–89) might prove quite useful and justifiable as a simple, rough and ready occupational code when one is especially concerned with occupational status differences per se.)

One of the hallmarks of the Warner tradition, which this book clearly manifests, has been its followers' clear, lucid style of writing, that is enjoyable to read as well as intellectually stimulating. Nowhere is this feature more evident than in the second part of the book, which provides stimulating ethnographies of the five major classes and their subdivisions, filled with perceptive insights and suggestive interpretations well worth following up in future studies. Although some of the content is rather dated (e.g., concerning certain features of the life styles of various classes that have probably changed since the 1950s), it is certainly worth reading and could be usefully assigned in an undergraduate introductory or stratification course.

Chapter 11, on "Social Mobility in Kansas City," deserves comment from several points of view. On the one hand, we are presented an interesting and suggestive set of speculations about the course of social mobility in adult life and differential rates of mobility among the classes. Unfortunately, the data base upon which these speculations rest is too small to sustain the authors' rather subtle interpretations. But what is more disappointing is that they again fail to take advantage of the time lag in publication to incorporate recent studies of mobility that have the

merit of much larger sample bases and more adequate analytic strategies and that, at least in some cases, provide substantiating evidence for some of the propositions advanced and, in others (e.g., p. 228 on the upper middle class as the site of the greatest turnover from generation to generation), some sobering qualifications and question marks.

The third part, devoted to assessing the existence of a nationwide status system, would have been greatly strengthened if it had explicitly confronted the commentators, like R. Nisbet, O. D. Duncan, G. Lenski, R. W. Hodge, and myself, who, among others, argue for a multidimensional, status continuum model of urban stratification systems or some variant thereof. The authors prefer, however, to base their entire extrapolation on eight community studies gathered almost exclusively within the Warner tradition, studies that have serious methodological weaknesses. It is only at this late point (see pp. 264, 277) in the text that it is stressed that "classes" are constructs of the social scientist and not the population studied. But even in validating the relevance of the social scientist's construct of class, I, for one, do not find satisfactory evidence presented in the book that the classes identified are in fact bounded interaction systems. The final chapter, while again filled with speculations about the changing nature of the American "class structure" in the 1960s, is almost totally devoid of citation or discussion of the relevant current literature.

To end this review on a more positive note, I have always found much food for thought—for example, researchable ideas—in the ethnographies growing out of the Warner tradition. They are always perceptive and sensitive to details often ignored by more methodologically rigorous investigators. This book is no exception. While I would have enjoyed this old wine even more in an aseptically clean bottle, it was still a good vintage.

Abstracts of Articles in This Issue

Insiders and Outsiders

Robert K. Merton

The social relevance of perspective established in the sociology of knowledge becomes evident during times of great social change and conflict. Conflict makes for a total functionalizing of thought which is interpreted only in terms of its alleged social, economic, political, or psychological sources and functions. Deepened social conflict today renews the relevance of an old problem in the sociology of knowledge: socially patterned differentials in access to new knowledge. As groups and collectivities become more self-conscious and solidary under conditions of social polarization, their members tend to claim unique or privileged access to certain kinds of knowledge. This can be described as the doctrine of the Insider, which includes the correlative claim that the Outsider has a structurally imposed incapacity for access to such knowledge. Outsider doctrine involves complementary claims of access to knowledge grounded on the assumption of socially based detachment. The rationale of the Insider doctrine is examined, with special reference to the advocates of a "black social science," a case taken as prototypical for other Insider doctrines based on sex, age, religion, nationality, etc. Structural analysis in terms of status sets indicates that Insider and Outsider doctrines based on affiliation with a single collectivity or occupancy of a single status are necessarily unstable and inadequate. The paper concludes by examining the distinctive interactive roles of Insiders and Outsiders that involve interchange, tradeoffs, and syntheses in the formation of social knowledge.

Radical Politics and Sociological Research

Howard S. Becker and Irving Louis Horowitz

Sociological work producing results that are "true" in not being falsifiable by available evidence and "true to the world" in encompassing the major components of the phenomena it studies is likely to have a radical thrust. Such work unmasks the conventional stories with which institutional functionaries and social leaders hide the inequities and failures of the organizations for which they are responsible. In so doing, it serves to increase the possibilities for freedom and equality in a society. The radical thrust of good sociological work reveals itself in its choice of causes for analysis, since the designation of causes serves to assign blame for undesirable events and to suggest what might be attacked in order to prevent them from occurring.

The Politics of American Sociologists

Seymour Martin Lipset and Everett Carl Ladd, Jr.

Various critiques of American sociology, most recently that of Alvin Gouldner, argue that the domain assumptions of the field stemming

from the functionalist approach of Talcott Parsons have imposed an essentially conservative system maintenance set of concerns on the field. Gouldner suggests that Parsons's approach reflects a lifelong opposition to socialism. In fact, Parsons's personal history belies these contentions. Further, a variety of survey studies, including a major unpublished one by Gouldner, indicate that sociologists as a group have been the most left-disposed field in academe, an occupation which is to the left of other strata. Within sociology, the "achievers," those at the most prestigious schools, who have published most, and have the most research funds, are to the left of others in the field. This pattern reflects a general characteristic of intellectual life in which the most successful people hold more unorthodox socially critical views, behavior which may stem from a link between creativity and heterodoxy. The fact that sociology is to the left of other fields may be inherent in the subject material it deals with, its distrust of reason, and its role as a debunker.

Professionalization of Sociology

Morris Janowitz

Sociology has developed as an academic profession since its origin. This paper seeks to analyze the efforts of sociologists to create a practice specialty and to identify the intellectual, organizational, and professional barriers to an "applied sociology." Alternatively, the role of the sociologist as a "staff" professional is examined.

On Gouldner's *Crisis*

John R. Rhoads

This is a critical analysis of Gouldner's *The Coming Crisis of Western Sociology*. It exposes some of the author's misinterpretations of the historical development of social theory and presents evidence drawn largely from the original works of the theorists. It disputes Gouldner's principal conclusion that there is a crisis in academic sociology insofar as this conclusion rests on his faulty reconstruction of social theory. Some contentions this critique opposes to those presented by Gouldner are: sociological positivism took a broader perspective than that toward the utilitarianism of the middle class; the functional theory of classical sociology appeared elsewhere than in contexts characterized by the supremacy of the middle class over the aristocracy; Parsons's theory is not an antideterministic defense of capitalism and does recognize the autonomy of the individual.

Political Judgments and the Social Perceptions

Vernon K. Dibble

Content analyses of 102 reports submitted to the Verein fuer Sozialpolitik during the 1880s and 1890s show that those authors who were

more explicit in their value judgments were also more likely to perceive social relationships rather than aggregates of people. Among the professional officials, however, a different dynamic was at work. They were more likely to look to the state for the implementation of their policy recommendations. And authors who looked to the state were more likely to perceive relations rather than aggregates. These findings speak to certain wider issues in the history of sociology and the sociology of knowledge, including the transition of sociology away from its original value-laden stance, and the necessity for sociologists of knowledge to use quite different approaches toward the understanding of cognitions and of evaluative styles.

Early Marxist Thought

Lewis A. Coser

This paper investigates Kautskyan Marxism, Bernsteinian revisionism, the teachings of Rosa Luxemburg and her co-thinkers, Russian Menshevism and Bolshevism, the political writings of Georg Lukacs, and those of Antonio Gramsci in terms of the societal milieu in which they originated, the social location of their authors, and the audiences and publics to which they addressed themselves. The revolutionary and activist Marxism first developed by Marx and Engels in the late 1840s found an echo among 20th-century intellectuals in those rimland areas of Europe which still resembled in essential respects the conditions prevailing in the heartland of Europe half a century earlier. In the main centers of the European working class, on the other hand, positivistic evolutionary Marxism, adumbrated by Engels in the last period of his life and further developed by Kautsky, carried the day and was later largely replaced by Fabian or Bernsteinian gradualism. Social existence determined social consciousness. The receptivity to variant Marxist doctrine was largely conditioned by the values and attitudes of the men and women whose concrete social and historical existence was mirrored in their world view, whether as producers or as consumers of ideas.

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Format of References in Text: All references to monographs, articles, and statistical sources are to be identified at the appropriate point in the text by last name of author, year of publication, and pagination where appropriate, all within parentheses. Footnotes are to be used only for substantive observations. Specify subsequent citations of the same source in the same way as the first one; do not use "ibid.," "op. cit.," or "loc. cit."

1. When author's name is in the text: Duncan (1959). When author's name is not in the text: (Gouldner 1963).
2. Pagination follows year of publication: (Lipset 1964, pp. 61–65).
3. With dual authorship, give both names; for more than three, use "et al." For institutional authorship, supply minimum identification from the beginning of the complete citation: (U.S. Bureau of Census 1963, p. 117).
4. With more than one reference to an author, distinguish them by use of letters (a, b) attached to the year of publication: (1965a).
5. Enclose a series of references within a single pair of parentheses, separated by semicolons.

Format of References: List all items alphabetically by author and, within author, by year of publication, in an appendix, titled "References." Use no abbreviations. Examples follow:

- Davis, K. 1963a. "The Theory of Change and Response in Modern Demographic History." *Population Index* 29 (October): 345–66.
- . 1963b. "Social Demography." In *The Behavioral Sciences Today*, edited by Bernard Berelson. New York: Basic Books.
- Goode, W. J. 1967. "The Protection of the Inept." *American Sociological Review* 32 (February): 5–19.
- Moore, Wilbert E., and Arnold S. Feldman. 1960. *Labor Commitment and Social Change in Developing Areas*. New York: Social Science Research Council.
- Sanford, Nevitt, ed. 1962. *The American College*. New York: Wiley.

The Positive Functions of Poverty¹

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Mertonian functional analysis is applied to explain the persistence of poverty, and fifteen functions which poverty and the poor perform for the rest of American society, particularly the affluent, are identified and described. Functional alternatives which would substitute for these functions and make poverty unnecessary are suggested, but the most important alternatives are themselves dysfunctional for the affluent, since they require some redistribution of income and power. A functional analysis of poverty thus comes to many of the same conclusions as radical sociological analysis, demonstrating anew Merton's assertion that functionalism need not be conservative in ideological outlook or implication.

I

Over 20 years ago, Merton (1949, p. 71), analyzing the persistence of the urban political machine, wrote that because "we should ordinarily . . . expect persistent social patterns and social structures to perform positive functions which are at the time not adequately fulfilled by other existing patterns and structures . . . perhaps this publicly maligned organization is, under present conditions, satisfying basic latent functions." He pointed out how the machine provided central authority to get things done when a decentralized local government could not act, humanized the services of the impersonal bureaucracy for fearful citizens, offered concrete help (rather than law or justice) to the poor, and otherwise performed services needed or demanded by many people but considered unconventional or even illegal by formal public agencies.

This paper is not concerned with the political machine, however, but with poverty, a social phenomenon which is as maligned as and far more persistent than the machine. Consequently, there may be some merit in applying functional analysis to poverty, to ask whether it too has positive functions that explain its persistence. Since functional analysis has itself taken on a maligned status among some American sociologists, a

¹ Earlier versions of this paper were presented at a Vassar College conference on the war on poverty in 1964, at the 7th World Congress of Sociology in 1971, and in *Social Policy* 2 (July-August 1971): 20-24. The present paper will appear in a forthcoming book on poverty and stratification, edited by S. M. Lipset and S. M. Miller, for the American Academy of Arts and Sciences. I am indebted to Peter Marris, Robert K. Merton, and S. M. Miller for helpful comments on earlier drafts of this paper.

secondary purpose of this paper is to ask whether it is still a useful approach.²

II

Merton (1949, p. 50) defined functions as "those observed consequences which make for the adaptation or adjustment of a given system; and dysfunctions, those observed consequences which lessen the adaptation or adjustment of the system." This definition does not specify the nature or scope of the system, but elsewhere in his classic paper "Manifest and Latent Functions," Merton indicated that social system was not a synonym for society, and that systems vary in size, requiring a functional analysis "to consider a *range* of units for which the item (or social phenomenon H.G.) has designated consequences: individuals in diverse statuses, subgroups, the larger social system and cultural systems" (1949, p. 51).

In discussing the functions of poverty, I shall identify functions for *groups* and *aggregates*; specifically, interest groups, socioeconomic classes, and other population aggregates, for example, those with shared values or similar statuses. This definitional approach is based on the assumption that almost every social system—and of course every society—is composed of groups or aggregates with different interests and values, so that, as Merton put it (1949, p. 51), "items may be functional for some individuals and subgroups and dysfunctional for others." Indeed, frequently one group's functions are another group's dysfunctions.³ For example, the political machine analyzed by Merton was functional for the working class and business interests of the city but dysfunctional for many middle class and reform interests. Consequently, functions are defined as those observed consequences which are positive *as judged by the values of the group under analysis*; dysfunctions, as those which are negative by these values.⁴ Because functions benefit the group in question and dysfunctions

² The paper also has the latent function, as S. M. Miller has suggested, of contributing to the long debate over the functional analysis of social stratification presented by Davis and Moore (1945).

³ Probably one of the few instances in which a phenomenon has the same function for two groups with different interests is when the survival of the system in which both participate is at stake. Thus, a wage increase can be functional for labor and dysfunctional for management (and consumers), but if the wage increase endangers the firm's survival, it is dysfunctional for labor as well. This assumes, however, that the firm's survival is valued by the workers, which may not always be the case, for example, when jobs are available elsewhere.

⁴ Merton (1949, p. 50) originally described functions and dysfunctions in terms of encouraging or hindering adaptation or adjustment to a system, although subsequently he has written that "dysfunction refers to the particular inadequacies of a particular part of the system for a designated requirement" (1961, p. 732). Since adaptation and

hurt it, I shall also describe functions and dysfunctions in the language of economic planning and systems analysis as benefits and costs.⁵

Identifying functions and dysfunctions for groups and aggregates rather than systems reduces the possibility that what is functional for one group in a multigroup system will be seen as being functional for the whole system, making it more difficult, for example, to suggest that a given phenomenon is functional for a corporation or political regime when it may in fact only be functional for their officers or leaders. Also, this approach precludes reaching a priori conclusions about two other important empirical questions raised by Merton (1949, pp. 32-36), whether any phenomenon is ever functional or dysfunctional for an entire society, and, if functional, whether it is therefore indispensable to that society.

In a modern heterogeneous society, few phenomena are functional or dysfunctional for the society as a whole, and most result in benefits to some groups and costs to others. Given the level of differentiation in modern society, I am even skeptical whether one can empirically identify a social system called society. Society exists, of course, but it is closer to being a very large aggregate, and when sociologists talk about society as a system, they often really mean the nation, a system which, among other things, sets up boundaries and other distinguishing characteristics between societal aggregates.

I would also argue that no social phenomenon is indispensable; it may be too powerful or too highly valued to be eliminated, but in most instances, one can suggest what Merton calls "functional alternatives" or equivalents for a social phenomena, that is, other social patterns or policies which achieve the same functions but avoid the dysfunctions.

III

The conventional view of American poverty is so dedicated to identifying the dysfunctions of poverty, both for the poor and the nation, that at

adjustment to a system can have conservative ideological implications, Merton's later formulation and my own definitional approach make it easier to use functional analysis as an ideologically neutral or at least ideologically variable method, insofar as the researcher can decide for himself whether he supports the values of the group under analysis.

⁵ It should be noted, however, that there are no absolute benefits and costs just as there are no absolute functions and dysfunctions; not only are one group's benefits often another group's costs, but every group defines benefits by its own manifest and latent values, and a social scientist or planner who has determined that certain phenomena provide beneficial consequences for a group may find that the group thinks otherwise. For example, during the 1960s, advocates of racial integration discovered that a significant portion of the black community no longer considered it a benefit but saw it rather as a policy to assimilate blacks into white society and to decimate the political power of the black community.

first glance it seems inconceivable to suggest that poverty could be functional for anyone. Of course, the slum lord and the loan shark are widely known to profit from the existence of poverty; but they are popularly viewed as evil men, and their activities are, at least in part, dysfunctional for the poor. However, what is less often recognized, at least in the conventional wisdom, is that poverty also makes possible the existence or expansion of "respectable" professions and occupations, for example, penology, criminology, social work, and public health. More recently, the poor have provided jobs for professional and paraprofessional "poverty warriors," as well as journalists and social scientists, this author included, who have supplied the information demanded when public curiosity about the poor developed in the 1960s.

Clearly, then, poverty and the poor may well serve a number of functions for many nonpoor groups in American society, and I shall describe 15 sets of such functions—economic, social, cultural, and political—that seem to me most significant.

First, the existence of poverty makes sure that "dirty work" is done. Every economy has such work: physically dirty or dangerous, temporary, dead-end and underpaid, undignified, and menial jobs. These jobs can be filled by paying higher wages than for "clean" work, or by requiring people who have no other choice to do the dirty work and at low wages. In America, poverty functions to provide a low-wage labor pool that is willing—or, rather, unable to be unwilling—to perform dirty work at low cost. Indeed, this function is so important that in some Southern states, welfare payments have been cut off during the summer months when the poor are needed to work in the fields. Moreover, the debate about welfare—and about proposed substitutes such as the negative income tax and the Family Assistance Plan—has emphasized the impact of income grants on work incentive, with opponents often arguing that such grants would reduce the incentive of—actually, the pressure on—the poor to carry out the needed dirty work if the wages therefore are no larger than the income grant. Furthermore, many economic activities which involve dirty work depend heavily on the poor; restaurants, hospitals, parts of the garment industry, and industrial agriculture, among others, could not persist in their present form without their dependence on the substandard wages which they pay to their employees.

Second, the poor subsidize, directly and indirectly, many activities that benefit the affluent.⁶ For one thing, they have long supported both

⁶ Of course, the poor do not actually subsidize the affluent. Rather, by being forced to work for low wages, they enable the affluent to use the money saved in this fashion for other purposes. The concept of subsidy used here thus assumes belief in a "just wage."

the consumption and investment activities of the private economy by virtue of the low wages which they receive. This was openly recognized at the beginning of the Industrial Revolution, when a French writer quoted by T. H. Marshall (forthcoming, p. 7) pointed out that "to assure and maintain the prosperities of our industries, it is necessary that the workers should never acquire wealth." Examples of this kind of subsidization abound even today; for example, domestics subsidize the upper middle and upper classes, making life easier for their employers and freeing affluent women for a variety of professional, cultural, civic, or social activities. In addition, as Barry Schwartz pointed out (personal communication), the low income of the poor enables the rich to divert a higher proportion of their income to savings and investment, and thus to fuel economic growth. This, in turn, can produce higher incomes for everybody, including the poor, although it does not necessarily improve the position of the poor in the socioeconomic hierarchy, since the benefits of economic growth are also distributed unequally.

At the same time, the poor subsidize the governmental economy. Because local property and sales taxes and the ungraduated income taxes levied by many states are regressive, the poor pay a higher percentage of their income in taxes than the rest of the population, thus subsidizing the many state and local governmental programs that serve more affluent taxpayers.⁷ In addition, the poor support medical innovation as patients in teaching and research hospitals, and as guinea pigs in medical experiments, subsidizing the more affluent patients who alone can afford these innovations once they are incorporated into medical practice.

Third, poverty creates jobs for a number of occupations and professions which serve the poor, or shield the rest of the population from them. As already noted, penology would be miniscule without the poor, as would the police, since the poor provide the majority of their "clients." Other activities which flourish because of the existence of poverty are the numbers game, the sale of heroin and cheap wines and liquors, pentecostal ministers, faith healers, prostitutes, pawn shops, and the peacetime army, which recruits its enlisted men mainly from among the poor.

Fourth, the poor buy goods which others do not want and thus prolong their economic usefulness, such as day-old bread, fruit and vegetables which would otherwise have to be thrown out, second-hand clothes, and deteriorating automobiles and buildings. They also provide incomes for

⁷ Pechman (1969) and Herriott and Miller (1971) found that the poor pay a higher proportion of their income in taxes than any other part of the population: 50% among people earning \$2,000 or less according to the latter study.

doctors, lawyers; teachers, and others who are too old, poorly trained, or incompetent to attract more affluent clients.

In addition, the poor perform a number of social and cultural functions:

Fifth, the poor can be identified and punished as alleged or real deviants in order to uphold the legitimacy of dominant norms (Macarov 1970, pp. 31–33). The defenders of the desirability of hard work, thrift, honesty, and monogamy need people who can be accused of being lazy, spendthrift, dishonest, and promiscuous to justify these norms; and as Erikson (1964) and others following Durkheim have pointed out, the norms themselves are best legitimated by discovering violations.

Whether the poor actually violate these norms more than affluent people is still open to question. The working poor work harder and longer than high-status jobholders, and poor housewives must do more housework to keep their slum apartments clean than their middle-class peers in standard housing. The proportion of cheaters among welfare recipients is quite low and considerably lower than among income taxpayers.⁸ Violent crime is higher among the poor, but the affluent commit a variety of white-collar crimes, and several studies of self-reported delinquency have concluded that middle-class youngsters are sometimes as delinquent as the poor. However, the poor are more likely to be caught when participating in deviant acts and, once caught, to be punished more often than middle-class transgressors. Moreover, they lack the political and cultural power to correct the stereotypes that affluent people hold of them, and thus continue to be thought of as lazy, spendthrift, etc., whatever the empirical evidence, by those who need living proof that deviance does not pay.⁹ The actually or allegedly deviant poor have traditionally been described as undeserving and, in more recent terminology, culturally deprived or pathological.

Sixth, another group of poor, described as deserving because they are disabled or suffering from bad luck, provide the rest of the population with different emotional satisfactions; they evoke compassion, pity, and charity, thus allowing those who help them to feel that they are altruistic, moral, and practicing the Judeo-Christian ethic. The deserving poor also enable others to feel fortunate for being spared the deprivations that come with poverty.¹⁰

⁸ Most official investigations of welfare cheating have concluded that less than 5% of recipients are on the rolls illegally, while it has been estimated that about a third of the population cheats in filing income tax returns.

⁹ Although this paper deals with the functions of poverty for other groups, poverty has often been described as a motivating or character-building device for the poor themselves; and economic conservatives have argued that by generating the incentive to work, poverty encourages the poor to escape poverty. For an argument that work incentive is more enhanced by income than lack of it, see Gans (1971, p. 96).

¹⁰ One psychiatrist (Chernus 1967) has even proposed the fantastic hypothesis that

Seventh, as a converse of the fifth function described previously, the poor offer affluent people vicarious participation in the uninhibited sexual, alcoholic, and narcotic behavior in which many poor people are alleged to indulge, and which, being freed from the constraints of affluence and respectability, they are often thought to enjoy more than the middle classes. One of the popular beliefs about welfare recipients is that many are on a permanent sex-filled vacation. Although it may be true that the poor are more given to uninhibited behavior, studies by Rainwater (1970) and other observers of the lower class indicate that such behavior is as often motivated by despair as by lack of inhibition, and that it results less in pleasure than in a compulsive escape from grim reality. However, whether the poor actually have more sex and enjoy it more than affluent people is irrelevant; as long as the latter believe it to be so, they can share it vicariously and perhaps enviously when instances are reported in fictional, journalistic, or sociological and anthropological formats.

Eighth, poverty helps to guarantee the status of those who are not poor. In a stratified society, where social mobility is an especially important goal and class boundaries are fuzzy, people need to know quite urgently where they stand. As a result, the poor function as a reliable and relatively permanent measuring rod for status comparison, particularly for the working class, which must find and maintain status distinctions between itself and the poor, much as the aristocracy must find ways of distinguishing itself from the *nouveau riche*.

Ninth, the poor also assist in the upward mobility of the nonpoor, for, as Goode has pointed out (1967, p. 5), "the privileged . . . try systematically to prevent the talent of the less privileged from being recognized or developed." By being denied educational opportunities or being stereotyped as stupid or unteachable, the poor thus enable others to obtain the better jobs. Also, an unknown number of people have moved themselves or their children up in the socioeconomic hierarchy through the incomes earned from the provision of goods and services in the slums: by becoming policemen and teachers, owning "Mom and Pop" stores, or working in the various rackets that flourish in the slums.

In fact, members of almost every immigrant group have financed their upward mobility by providing retail goods and services, housing, entertainment, gambling, narcotics, etc., to later arrivals in America (or in the city), most recently to blacks, Mexicans, and Puerto Ricans. Other Americans, of both European and native origin, have financed their entry into the upper middle and upper classes by owning or managing the illegal institutions that serve the poor, as well as the legal but not respectable ones, such as slum housing.

the rich and the poor are engaged in a sadomasochistic relationship, the latter being supported financially by the former so that they can gratify their sadistic needs.

Tenth, just as the poor contribute to the economic viability of a number of businesses and professions (see function 3 above), they also add to the social viability of noneconomic groups. For one thing, they help to keep the aristocracy busy, thus justifying its continued existence. "Society" uses the poor as clients of settlement houses and charity benefits; indeed, it must have the poor to practice its public-mindedness so as to demonstrate its superiority over the *nouveaux riches* who devote themselves to conspicuous consumption. The poor play a similar function for philanthropic enterprises at other levels of the socioeconomic hierarchy, including the mass of middle-class civic organizations and women's clubs engaged in volunteer work and fundraising in almost every American community. Doing good among the poor has traditionally helped the church to find a method of expressing religious sentiments in action; in recent years, militant church activity among and for the poor has enabled the church to hold on to its more liberal and radical members who might otherwise have dropped out of organized religion altogether.

Eleventh, the poor perform several cultural functions. They have played an unsung role in the creation of "civilization," having supplied the construction labor for many of the monuments which are often identified as the noblest expressions and examples of civilization, for example, the Egyptian pyramids, Greek temples, and medieval churches.¹¹ Moreover, they have helped to create a goodly share of the surplus capital that funds the artists and intellectuals who make culture, and particularly "high" culture, possible in the first place.

Twelfth, the "low" culture created for or by the poor is often adopted by the more affluent. The rich collect artifacts from extinct folk cultures (although not only from poor ones), and almost all Americans listen to the jazz, blues, spirituals, and country music which originated among the Southern poor—as well as rock, which was derived from similar sources. The protest of the poor sometimes becomes literature; in 1970, for example, poetry written by ghetto children became popular in sophisticated literary circles. The poor also serve as culture heroes and literary subjects, particularly, of course, for the Left, but the hobo, cowboy, hipster, and the mythical prostitute with a heart of gold have performed this function for a variety of groups.

Finally, the poor carry out a number of important political functions:

Thirteenth, the poor serve as symbolic constituencies and opponents for several political groups. For example, parts of the revolutionary Left could not exist without the poor, particularly now that the working class can no longer be perceived as the vanguard of the revolution. Conversely,

¹¹ Although this is not a contemporary function of poverty in America, it should be noted that today these monuments serve to attract and gratify American tourists.

political groups of conservative bent need the "welfare chiselers" and others who "live off the taxpayer's hard-earned money" in order to justify their demands for reductions in welfare payments and tax relief. Moreover, the role of the poor in upholding dominant norms (see function 5 above) also has a significant political function. An economy based on the ideology of laissez faire requires a deprived population which is allegedly unwilling to work; not only does the alleged moral inferiority of the poor reduce the moral pressure on the present political economy to eliminate poverty, but redistributive alternatives can be made to look quite unattractive if those who will benefit from them most can be described as lazy, spendthrift, dishonest, and promiscuous. Thus, conservatives and classical liberals would find it difficult to justify many of their political beliefs without the poor; but then so would modern liberals and socialists who seek to eliminate poverty.

Fourteenth, the poor, being powerless, can be made to absorb the economic and political costs of change and growth in American society. During the 19th century, they did the backbreaking work that built the cities; today, they are pushed out of their neighborhoods to make room for "progress." Urban renewal projects to hold middle-class taxpayers and stores in the city and expressways to enable suburbanites to commute downtown have typically been located in poor neighborhoods, since no other group will allow itself to be displaced. For much the same reason, urban universities, hospitals, and civic centers also expand into land occupied by the poor. The major costs of the industrialization of agriculture in America have been borne by the poor, who are pushed off the land without recompense, just as in earlier centuries in Europe, they bore the brunt of the transformation of agrarian societies into industrial ones. The poor have also paid a large share of the human cost of the growth of American power overseas, for they have provided many of the foot soldiers for Vietnam and other wars.

Fifteenth, the poor have played an important role in shaping the American political process; because they vote and participate less than other groups, the political system has often been free to ignore them. This has not only made American politics more centrist than would otherwise be the case, but it has also added to the stability of the political process. If the 15% of the population below the federal "poverty line" participated fully in the political process, they would almost certainly demand better jobs and higher incomes, which would require income redistribution and would thus generate further political conflict between the haves and the have-nots. Moreover, when the poor do participate, they often provide the Democrats with a captive constituency, for they can rarely support Republicans, lack parties of their own, and thus have no other place to go politically. This, in turn, has enabled the Democrats to count

on the votes of the poor, allowing the party to be more responsive to voters who might otherwise switch to the Republicans, in recent years, for example, the white working class.

IV

I have described fifteen of the more important functions which the poor carry out in American society, enough to support the functionalist thesis that poverty survives in part because it is useful to a number of groups in society. This analysis is not intended to suggest that because it is functional, poverty *should* persist, or that it *must* persist. Whether it should persist is a normative question; whether it must, an analytic and empirical one, but the answer to both depends in part on whether the dysfunctions of poverty outweigh the functions. Obviously, poverty has many dysfunctions, mainly for the poor themselves but also for the more affluent. For example, their social order is upset by the pathology, crime, political protest, and disruption emanating from the poor, and the income of the affluent is affected by the taxes that must be levied to protect their social order. Whether the dysfunctions outweigh the functions is a question that clearly deserves study.

It is, however, possible to suggest alternatives for many of the functions of the poor. Thus, society's dirty work (function 1) could be done without poverty, some by automating it, the rest by paying the workers who do it decent wages, which would help considerably to cleanse that kind of work. Nor is it necessary for the poor to subsidize the activities they support through their low-wage jobs (function 2), for, like dirty work, many of these activities are essential enough to persist even if wages were raised. In both instances, however, costs would be driven up, resulting in higher prices to the customers and clients of dirty work and subsidized activity, with obvious dysfunctional consequences for more affluent people.

Alternative roles for the professionals who flourish because of the poor (function 3) are easy to suggest. Social workers could counsel the affluent, as most prefer to do anyway, and the police could devote themselves to traffic and organized crime. Fewer penologists would be employable, however, and pentecostal religion would probably not survive without the poor. Nor would parts of the second- and third-hand market (function 4), although even affluent people sometimes buy used goods. Other roles would have to be found for badly trained or incompetent professionals now relegated to serving the poor, and someone else would have to pay their salaries.

Alternatives for the deviance-connected social functions (functions 5-7) can be found more easily and cheaply than for the economic functions.

Other groups are already available to serve as deviants to uphold traditional morality, for example, entertainers, hippies, and most recently, adolescents in general. These same groups are also available as alleged or real orgiasts to provide vicarious participation in sexual fantasies. The blind and disabled function as objects of pity and charity, and the poor may therefore not even be needed for functions 5-7.

The status and mobility functions of the poor (functions 8 and 9) are far more difficult to substitute, however. In a hierarchical society, some people must be defined as inferior to everyone else with respect to a variety of attributes, and the poor perform this function more adequately than others. They could, however, perform it without being as poverty-stricken as they are, and one can conceive of a stratification system in which the people below the federal "poverty line" would receive 75% of the median income rather than 40% or less, as is now the case—even though they would still be last in the pecking order.¹² Needless to say, such a reduction of economic inequality would also require income redistribution. Given the opposition to income redistribution among more affluent people, however, it seems unlikely that the status functions of poverty can be replaced, and they—together with the economic functions of the poor, which are equally expensive to replace—may turn out to be the major obstacles to the elimination of poverty.

The role of the poor in the upward mobility of other groups could be maintained without their being so low in income. However, if their incomes were raised above subsistence levels, they would begin to generate capital so that their own entrepreneurs could supply them with goods and services, thus competing with and perhaps rejecting "outside" suppliers. Indeed, this is already happening in a number of ghettos, where blacks are replacing white storeowners.

Similarly, if the poor were more affluent, they would make less willing clients for upper- and middle-class philanthropic and religious groups (function 10), although as long as they are economically and otherwise unequal, this function need not disappear altogether. Moreover, some would still use the settlement houses and other philanthropic institutions to pursue individual upward mobility, as they do now.

The cultural functions (11 and 12) may not need to be replaced. In America, the labor unions have rarely allowed the poor to help build cultural monuments anyway, and there is sufficient surplus capital from other sources to subsidize the unprofitable components of high culture.

¹² In 1971, the median family income in the United States was about \$10,000, and the federal poverty line for a family of four was set at just about \$4,000. Of course, most of the poor were earning less than 40% of the median, and about a third of them, less than 20% of the median.

Similarly, other deviant groups are available to innovate in popular culture and supply new culture heroes, for example, the hippies and members of other counter-cultures.

Some of the political functions of the poor would, however, be as difficult to replace as their economic and status functions. Although the poor could probably continue to serve as symbolic constituencies and opponents (function 13) if their incomes were raised while they remained unequal in other respects, increases in income are generally accompanied by increases in power as well. Consequently, once they were no longer so poor, people would be likely to resist paying the costs of growth and change (function 14); and it is difficult to find alternative groups who can be displaced for urban renewal and technological "progress." Of course, it is possible to design city-rebuilding and highway projects which properly reimburse the displaced people, but such projects would then become considerably more expensive, thus raising the price for those now benefiting from urban renewal and expressways. Alternatively, many might never be built, thus reducing the comfort and convenience of those beneficiaries. Similarly, if the poor were subjected to less economic pressure, they would probably be less willing to serve in the army, except at considerably higher pay, in which case war would become yet more costly and thus less popular politically. Alternatively, more servicemen would have to be recruited from the middle and upper classes, but in that case war would also become less popular.

The political stabilizing and "centering" role of the poor (function 15) probably cannot be substituted for at all, since no other group is willing to be disenfranchised or likely enough to remain apathetic so as to reduce the fragility of the political system. Moreover, if the poor were given higher incomes, they would probably become more active politically, thus adding their demands for more to those of other groups already putting pressure on the political allocators of resources. The poor might continue to remain loyal to the Democratic party, but like other moderate-income voters, they could also be attracted to the Republicans or to third parties. While improving the economic status of the presently poor would not necessarily drive the political system far to the left, it would enlarge the constituencies now demanding higher wages and more public funds. It is of course possible to add new powerless groups who do not vote or otherwise participate to the political mix and can thus serve as "ballast" in the polity, for example, by encouraging the import of new poor immigrants from Europe and elsewhere, except that the labor unions are probably strong enough to veto such a policy.

In sum, then, several of the most important functions of the poor cannot be replaced with alternatives, while some could be replaced, but almost

always only at higher costs to other people, particularly more affluent ones. Consequently, *a functional analysis must conclude that poverty persists not only because it satisfies a number of functions but also because many of the functional alternatives to poverty would be quite dysfunctional for the more affluent members of society.*¹³

V

I noted earlier that functional analysis had itself become a maligned phenomenon and that a secondary purpose of this paper was to demonstrate its continued usefulness. One reason for its presently low status is political; insofar as an analysis of functions, particularly latent functions, seems to justify what ought to be condemned, it appears to lend itself to the support of conservative ideological positions, although it can also have radical implications when it subverts the conventional wisdom. Still, as Merton has pointed out (1949, p. 43; 1961, pp. 736-37), functional analysis per se is ideologically neutral, and "like other forms of sociological analysis, it can be infused with any of a wide range of sociological values" (1949, p. 40). This infusion depends, of course, on the purposes—and even the functions—of the functional analysis, for as Wirth (1936, p. xvii) suggested long ago, "every assertion of a 'fact' about the social world touches the interests of some individual or group," and even if functional analyses are conceived and conducted in a neutral manner, they are rarely interpreted in an ideological vacuum.

In one sense, my analysis is, however, neutral; if one makes no judgment as to whether poverty ought to be eliminated—and if one can subsequently avoid being accused of acquiescing in poverty—then the analysis suggests only that poverty exists because it is useful to many groups in society.¹⁴ If one favors the elimination of poverty, however, then the analysis can have a variety of political implications, *depending in part on how completely it is carried out.*

If functional analysis only identifies the functions of social phenomena without mentioning their dysfunctions, then it may, intentionally or otherwise, agree with or support holders of conservative values. Thus, to say that the poor perform many functions for the rich might be interpreted or used to justify poverty, just as Davis and Moore's argument

¹³ Or as Stein (1971, p. 171) puts it: "If the non-poor make the rules . . . antipoverty efforts will only be made up to the point where the needs of the non-poor are satisfied, rather than the needs of the poor."

¹⁴ Of course, even in this case the analysis need not be purely neutral, but can be put to important policy uses, for example, by indicating more effectively than moral attacks on poverty the exact nature of the obstacles that must be overcome if poverty is to be eliminated. See also Merton (1961, pp. 709-12).

(1945) that social stratification is functional because it provides society with highly trained professionals could be taken to justify inequality.

Actually, the Davis and Moore analysis was conservative because it was incomplete; it did not identify the dysfunctions of inequality and failed to suggest functional alternatives, as Tumin (1953) and Schwartz (1955) have pointed out.¹⁵ Once a functional analysis is made more complete by the addition of functional alternatives, however, it can take on a liberal and reform cast, because the alternatives often provide ameliorative policies that do not require any drastic change in the existing social order.

Even so, to make functional analysis complete requires yet another step, an examination of the functional alternatives themselves. My analysis suggests that the alternatives for poverty are themselves dysfunctional for the affluent population, and it ultimately comes to a conclusion which is not very different from that of radical sociologists. To wit: *that social phenomena which are functional for affluent groups and dysfunctional for poor ones persist; that when the elimination of such phenomena through functional alternatives generates dysfunctions for the affluent, they will continue to persist; and that phenomena like poverty can be eliminated only when they either become sufficiently dysfunctional for the affluent or when the poor can obtain enough power to change the system of social stratification.*¹⁶

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¹⁵ Functional analysis can, of course, be conservative in value or have conservative implications for a number of other reasons, principally in its overt or covert comparison of the advantages of functions and disadvantages of dysfunctions, or in its attitudes toward the groups that are benefiting and paying the costs. Thus, a conservatively inclined policy researcher could conclude that the dysfunctions of poverty far outnumber the functions, but still decide that the needs of the poor are simply not as important or worthy as those of other groups, or of the country as a whole.

¹⁶ On the possibility of radical functional analysis, see Merton (1949, pp. 40-43) and Gouldner (1970, p. 443). One difference between my analysis and the prevailing radical view is that most of the functions I have described are latent, whereas many radicals treat them as manifest: recognized and intended by an unjust economic system to oppress the poor. Practically speaking, however, this difference may be unimportant, for if unintended and unrecognized functions were recognized, many affluent people might then decide that they ought to be intended as well, so as to forestall a more expensive antipoverty effort that might be dysfunctional for the affluent.

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A Note on the Rapid Rise of Mass Bengali Nationalism in East Pakistan

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The social and economic conditions for the emergence of East Pakistan as a distinct nation (Bangladesh) go back almost to 1947 when Pakistan itself was created. Did a sense of separate Bengali political identity likewise exist at the level of mass consciousness long before the open military repression of 1971? This note takes advantage of a survey datum gathered in 1964 to provide a clue that the answer is: no.

Between December 1970 and December 1971, Bengali nationalism in what was once East Pakistan manifested itself in many forms. The nearly total victory of the Awami League at the end of 1970, the subsequent discipline of the urban population in response to Sheik Mujibur's call, the increasingly organized guerrilla resistance to the West Pakistan army, all testify to the fact that by the end of 1971 vast numbers of East Bengalis had deserted the concept of "Pakistan" (or felt it had deserted them) and did indeed think, however vaguely, of Bangladesh—Bengal as their country. We cannot know for certain how wide and deep this feeling ran by the end of 1970 or even the end of 1971, but impressionistic accounts suggest that it must have penetrated all but the most remote rural areas by the latter date. If so, the question this brief research note raises is whether a conflict between attachment to Bengal and attachment to Pakistan was felt at the level of mass public opinion earlier in the 1960s or even in the 1950s.

The basic conditions leading to Bengali nationalism in East Pakistan are well known. East and West Pakistan were from the beginning separated not only geographically, but also in almost every aspect of culture and appearance one can imagine, other than religion. Moreover, West Pakistanis and other non-Bengalis dominated industry, the military establishment, much of the central government, and the bureaucracy from the beginning. Within a few years after independence, some economists in East Pakistan were already talking about the disparity in allocation of development funds between the two wings, and this source of antagonism continued to rise until the end, even though the disparity itself may have decreased by some indices in the later sixties. By the beginning of the 1960s, if not earlier, the conditions for Bengali disillusionment with the concept of Pakistan were already present, and at least a few political

leaders and intellectuals were thinking in terms of relative autonomy, if not independence.

Our question then is how deeply had this feeling penetrated public opinion in East Pakistan? Or looked at another way, given the local attachment and identification of the "man in the street" and the "man in the field" in East Bengal, where did his political identification lie in 1947? in 1950? in 1960? in 1968? We have no systematic answer to this question over time, but we do have a single relevant "snapshot" of mass opinion on this issue dating from early 1964. It is obviously incomplete even for that one period, yet it provides useful objective data for that single point in time and by implication for the years preceding 1964.

As part of a large cross-national project, I administered an interview survey to 1,001 urban factory workers, full-time rural cultivators, and certain other basically similar groups in East Pakistan from December 1963 until April 1964. The interviews were carried out in Bengali by a trained Bengali interviewing staff, using a questionnaire that had been carefully designed and translated over the preceding year. The study has been reported elsewhere more fully, and the sample is described in the Appendix of this paper.¹ The data we concentrate on here are based on what can be considered a reasonably representative sample of factory workers from 46 factories in the three major industrial cities of Dacca, Chittagong, and Khulna, and a somewhat less representative sample of cultivators from the districts of Barisal, Comilla, and Noakhali. All respondents were Bengali Muslim males, born in East Pakistan, and all were within the age range of 18-32 years. The factory workers had all grown up in Barisal, Comilla, or Noakhali—as had most industrial workers in East Pakistan—but had now been employed from three to 12 years as factory workers. The cultivators were purposely chosen from these same districts to allow a direct rural-urban comparison.

The survey was not directed toward political issues, and indeed stayed away from such issues on the whole. But midway in the quite lengthy interview, the following forced choice question was posed:

Do you consider yourself first and foremost a:²

¹ Several articles based on this survey are cited below, and a volume on the full cross-national project is being prepared by Alex Inkeles. There is also available an informal "Field Director's Report" (unpublished) describing the procedures used in East Pakistan to recruit a staff, translate the questionnaire, etc. I am indebted to Professor Inkeles, and also to Edward J. Ryan who, if my memory serves me right, devised the final (English) form of the particular question under scrutiny here.

² A transliteration of the actual Bengali version of the question reads:

Apani nijke pradhānata ki mane karen?

1. Pākistāni? nāki?
2. Bāngālī? nāki?
3. . . . Er bāsindā? nāki?
4. . . . Er bāsindā?

1. Pakistani,
2. A Bengali,
3. A man from [Insert R's District of Origin], or
4. A man from [Insert R's Village of Origin].

Results are given in table 1 for the entire sample of 1,001 and for the main factory worker and cultivator categories, broken down by literacy.³ There is little difference in the broad conclusions that would be drawn from any of the samples, although interesting differences emerge when occupation and literacy are used in analysis. Before proceeding to this second step, let us examine the overall results.

TABLE 1

PERCENTAGE CHOOSING EACH POLITICAL IDENTITY FOR TOTAL SAMPLE AND BY OCCUPATION AND LITERACY

| | TOTAL SAMPLE | CULTIVATORS | | FACTORY WORKERS | |
|-------------------------|-----------------|-----------------|---------------|-----------------|---------------|
| | | Illit- erate | Liter- ate | Illit- erate | Liter- ate |
| Pakistani | 48 | 36 | 58 | 45 | 62 |
| Bengali | 11 | 6 | 1 | 15 | 12 |
| Man of [district] | 17 | 14 | 14 | 14 | 12 |
| Man of [village] | 25 | 45 | 27 | 26 | 14 |
| Total | 101 | 101 | 100 | 100 | 100 |
| N | (1,001) | (103) | (74) | (179) | (205) |

The question under discussion ~~is~~ originally included not in an attempt to study Bengali nationalism, but rather with an interest in the level of abstractness of political identification held by the ordinary East Pakistani. The four alternatives presented range from a completely local identification (one's village), through the much larger but still relatively local unit of the administrative district,⁴ through the basic linguistic-cultural-provincial unit, and finally through the abstract purely political citizenship that had been invented in 1947. As expected, a considerable part of the sample retained a largely local identification (42% answering with the name of either their village or their district), but a surprisingly large proportion (48%) preferred to call themselves Pakistanis. For our present purpose, however, the most interesting point is the relatively small percentage of respondents who identified themselves as first and foremost

³ The literacy classification is based on a short test administered as part of the interview, as described in Schuman, Inkeles, and Smith (1967). The literacy measure is highly related to self-reported years of schooling.

⁴ A discussion of the importance of the district as an ethnic unit in East Pakistan appears in Schuman (1966b).

Bengalis. If we consider only the two more abstract levels of identification, men were more than four times as likely to say Pakistani as to say Bengali. Even if one thinks of the district as reflecting a somewhat similar level of identification as Bengal, more men considered themselves Pakistani than gave the provincial and district levels combined. We take this to mean that in 1964, those men who thought at all in terms of an abstract political identification accepted as valid and natural their primary citizenship as Pakistanis. Not that there was necessarily any opposition to claims of being a Pakistani rather than being a Bengali. On the contrary, it seems likely that no conflict at all was perceived between the two; instead, one's Bengali identification was simply included in the more general and at least equally desirable identification as a Pakistani.

Some further light can be thrown on these responses using more open-ended qualitative responses. As with all other forced-choice questions in the survey, a random subsample of about 50 respondents were asked immediately after answering the question to explain what they meant by their answer. A nondirective probe was used, such as, "Could you tell me a little more about that?" The probe was used both to make certain that a respondent understood the question and to obtain some insight into his answer.⁵ Most of these qualitative responses are not too informative for this question, since they essentially repeat the original choice in slightly more words, for example, "This is my birthplace," or, "I am from that district, I took my birth there, took my education there, and I am reared up there"—both of which could be used to explain almost any choice. But a few of the responses have more substantive import as shown below:

Answered (Pakistani):

I love Pakistan and we got Pakistan as a kingdom.

Because my own village is part of Pakistan.

I am a Muslim.

I was Bengali, now this Bengal has become independent and I have become Pakistani.

I am the son of a Muslim so I am a Pakistani.

Answered (Bengali):

I speak and understand the Bengali language well and do not know any other.

I read, write, and speak Bengali.

Bengal is my birth country. Pakistan is new.

All East Pakistanis are Bengalis and all in Barisal are also Bengali.

From these few responses, several things are clear. There is little sign of real conflict between being a Bengali and being a Pakistani. The "Pakistani" responses sometimes refer to religion, which makes obvious sense

⁵ The technique as developed for use in East Pakistan is described in Schuman (1966a).

since the original concept of Pakistan represented one's identity as a Muslim rather than as a Hindu. The "Bengali" responses mention language, again a very obvious and sensible association. But there are only two responses suggesting that respondents feel they must consciously choose between somewhat incompatible identifications, and in neither case does the choice seem very intense.

Turning to the rest of table 1, both literacy and occupation appear to have effects on political identity, but of a different nature. Literacy accounts for a statistically significant ($P < .02$) and fairly large shift (of about 20%) toward "Pakistani" among cultivators and factory workers alike. The most obvious interpretation is that these men "learned" their political identity in school and through the newspapers, both of which would have had much to say in the years preceding 1964 about the creation and development of Pakistan. It should be noted that the men interviewed were between the ages of 18 and 32, which together with the fact of literacy means that the younger of them had attended school after the establishment of Pakistan. However, there is no noticeable association between age and saying "Pakistani," and thus the effects of literacy must be attributed to the more general knowledge that comes from reading and related activities. The fact that there is essentially no difference between cultivators and factory workers for the "Pakistani" response indicates that urban living as such did not increase this more national identification.

The response "Bengali" reveals no effect of rising literacy, but there is a trend for factory workers to give that answer more often than do cultivators. The trend is not significant and is slight in size, so that interpretations can hardly be ventured with much confidence. One might note, however, that to be a cultivator in rural Bengal is to be a fish in the water—everyone else is a Bengali and so little notice may be taken of one's linguistic identification. Factory workers, however, came quickly into contact with a variety of non-Bengali owners and in many factories with Bihari workers as well. In such cases they may have become more aware of their own distinctive language and culture—more aware of being a "Bengali."

However, our most general finding for all categories is the paucity of "Bengali" responses. Basically, those men who thought politically beyond their local boundaries in 1964 largely identified with the nation of Pakistan. It is probable that they did this because Pakistan represented something good to them—a Muslim state—while it did not yet represent anything bad, that is, they were untouched by intellectual criticisms of the central government and had no negative personal experience with West Pakistanis. We did not investigate the latter point, but we did ask two questions that bear indirectly on the issue of whether Pakistan represented essentially

a protection against Hindus. Asked whether "Hindus" do mostly good, mostly harm, or neither, 30% said harm (8% good, the rest neither good nor harm); asked whether Pakistan and India could ever live in peace, 31% said no. Thus there was clearly considerable distrust of Hindus among Bengali Muslims in East Pakistan in 1964—which it may be remembered was a year in which severe communal rioting occurred in both East Pakistan and eastern India—and this certainly would have strengthened identification with the concept of Pakistan.⁶

We have one final bit of relevant data. A self-administered form of the questionnaire was given in March 1964 to 204 Bengali students in an intermediate technical college in Dacca. The sample consisted of all members of the second-year class, except that an unknown number did not attend for one reason or another. We can make no claim as to the precise representativeness of the sample in terms of any population, but as college students in Dacca these were obviously a much more informed and sophisticated group than our regular interview sample. A mark of this is that only 2% gave their village, and none at all gave their district in answer to the political identification question. With respect to our focal distinction between "Pakistani" and "Bengali," 74% gave the former and 24% the latter, a ratio of three to one. The emphasis on "Bengali" was thus somewhat greater among these students than among the common man, and this may well reflect the fact that Bengali nationalism was already beginning to rise in the student population. Unfortunately we lack qualitative follow-up responses of the kind reported earlier, so that we have no direct evidence for this interpretation. In any case, what deserves more emphasis is the fact that a substantial majority of even these college students gave "Pakistani" as their primary political identification in 1964.

In conclusion, it seems clear that in 1964 there was little or no awareness by the ordinary man in East Pakistan of a conflict between his identity as a Bengali and his identity as a Pakistani. Moreover, those citizens who were able to think beyond their village and district boundaries were more likely to see their national identity as a Pakistani as natural. If this was true in 1964, it was doubtless also true in 1960 and perhaps in all of the earlier years back to 1947. Unlike most other new states founded after World War II, Pakistan had a clear ethnic and religious meaning to even its most unsophisticated citizens. As for the years beyond 1964, our data obviously cannot tell us at what point allegiance to Pakistan began to be seriously questioned by the ordinary man. Enough important events occurred between 1964 and 1970—war with India, publicized con-

⁶ However, distrust of Hindus was not significantly associated with the "Pakistani" response.

piracy trials, the fall of Ayub, the campaigning of the Awami League—so that any one or several of these in combination could have changed mass opinion. It is worth keeping in mind, however, that much of the excitement of politics usually takes place among a small set of highly educated individuals, and that events of great national magnitude can transpire without much involvement of the multitudes.⁷ This is especially true in a largely rural society such as East Pakistan, where in 1964 there was no television and only a limited radio audience (98.3% of our sample did *not* own a radio of any kind). It may well be that *none* of the events mentioned above or others much before 1971 penetrated the general populace very deeply. Mass opinion in what is now Bangladesh may have been loyal to the concept of Pakistan as the nation until the Pakistan national army turned on the people of Bengal.

APPENDIX: SAMPLING

The design of the overall cross-national project required a sampling procedure which is complex, difficult to describe adequately, and in retrospect unfortunate from the standpoint of generalization to a known theoretical population. Nevertheless, for the present purpose the sample is probably a reasonable approximation to the results that would have been obtained from a straight probability sample of the East Pakistan population, subject to qualifications noted below.

Factory Workers

A stratified sample of factories was drawn from a complete list of all factories in Dacca, Khulna, and Chittagong, including surrounding industrial areas such as Narayanganj. Thus almost all factories in East Pakistan were in the population. The strata used were size of labor force, as listed by government sources, and the technical modernity of the factory, as judged by knowledgeable raters. On the whole, we have an excellent sample of factories.

Within each factory, approximately 50 workers were drawn by as random a procedure as was possible given many practical difficulties. Then approximately 10 workers were selected on the basis of the following criteria: all were to be Bengali Muslim males; ages 18–32; born and reared in Barisal, Comilla, or Noakhali; migrants to the urban industrial labor force; with factory experience of three to 12 years. This seems like

⁷ See the discussion in Converse (1964) of the absence from a large sample of personal documents of any serious mention of the abolitionist movement in the years just preceding the Civil War.

a long list of screening criteria, but in fact most workers fit all the criteria easily, with the exception of some who were Hindus and some who were "Biharis." One other criterion was that half the selected workers have four to eight years of education, half three or fewer years—again a criterion easily met, although the final sample is *slightly* more educated on the average than the industrial labor force as a whole. Where there were more than enough screened workers who met all the necessary criteria, random selection was used so far as possible. Certain other types of workers were also selected for special purposes (e.g., new workers with less than six months experience), but they are not included in the main factory worker sample presented in table 1, though they are included in the total sample of 1,001.

Cultivators

A similar quota sampling procedure was used to select cultivators who had lived all their lives in the three districts mentioned above. The villages sampled were selected on a more haphazard basis, however, than the factories, and we can be less sure that we represent the three districts well. The educational criteria were harder to meet in rural areas, so that the rural sample is noticeably more educated than the rural population generally. However, since literacy is controlled in table 1, it is possible to see the effects of this overrepresentation of more educated cultivators. Whether the three districts included under "Cultivators" in table 1 also represent other rural areas of East Pakistan is less clear. We did select an additional sample from Mymensingh, and as reported elsewhere (Schuman 1966*b*) there are some between-district differences in attitudes. However, we do not find them to be very large or particularly meaningful on the item that is the main subject of this paper.

Other Subsamples

Included in our total sample of 1,001 are several other small special samples, such as several lower-class urban nonindustrial worker samples (e.g., rickshaw drivers) and a sample of Comilla Cooperative cultivators. Since these are not our main focus of analysis, they will not be described in detail here. None of these subsamples show appreciable differences in political identification from the main cultivator and factory worker samples. The college students referred to in this paper constituted an entirely separate sample, obtained by self-administered questionnaires rather than face-to-face interviewing; these students are *not* included in our total sample of 1,001.

Nonresponse and Other Problems

Nonresponse was virtually absent as a sampling problem in this survey (except for the college students). The stratification of the factories results in a sample that gives equal representation to each factory and therefore undersamples larger factories. However, factory size is not related to political identification, nor is factory location in any large or interpretable sense.

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Social Antecedents of Adult Psychological Functioning

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The variables of age, father's education, rural upbringing, religion, and region in which one was raised are related to the complexity of the child-rearing environment. Having been reared in a complex, multifaceted environment results in a relatively high level of intellectual functioning, a rejection of external constraints, and a subjectivism stressing concern for the quality of one's inner life. The latter two trends are components of the process of individuation which seems to characterize modern life.

This paper attempts to establish causal relationships between certain aspects of the individual's early environment and his adult orientations, values, and psychological functioning. It focuses on variables determined by the individual's birth rather than on variables, such as adult social class, that may be functions of his own achievement. The guiding conception is that an individual raised in a complex multifaceted environment that does not subject him to many normative proscriptions becomes accustomed to having considerable freedom in determining his behavior. Consequently, as an adult such an individual is likely to be intolerant of external constraints, to be able to cope intellectually with complex and ambiguous situations, and to have a strong concern with and an awareness of his inner life.

Such results of being raised in a complex multifaceted environment are based on the definition of such an environment as one in which the individual (1) is called on to make decisions on the basis of a wide variety of factors, (2) is exposed to numerous alternate models of behavior, and (3) can see as plausible a wide range of goals. Such an environment should affect the individual's psychological functioning, particularly when there are relatively few effective normative proscriptions limiting either the goals he may choose or the means he can use to achieve these goals. Under such circumstances, he should come to see himself as a relatively autonomous being and consequently should develop an intolerance for external constraints on his behavior. Second, to the extent that the pattern of reinforcements within such a complex, ambiguously structured environment rewards efficacious cognitive organization, the individual should be motivated to develop his intellectual capacities as fully as possible. Finally, an individual raised in such an environment should

tend to develop a form of subjectivism based on the belief that what takes place within himself is of primary importance, such a belief deriving both from the feeling that the locus of control is and should be within oneself and from the importance given to mental processes by the emphasis placed on effective cognitive functioning.

This formulation is similar to those of two other investigations, one by Kohn and Schooler (1969), the other by Inkeles (1966, 1969). The conclusion of the former study was that occupational conditions that require the exercise of self-direction increase (1) a worker's intellectual flexibility, (2) the value he places on self-direction in situations outside of his job, and (3) the likelihood that he will see the world as an essentially benign place upon which his actions can have some effect.

The importance of environmental complexity and freedom from external control in determining the individual's attitudes and psychological functioning is also stressed by the investigations of modernization carried out by Inkeles and his associates. This series of studies was designed to determine both the conditions which affect rates of modernization, and the psychological qualities that characterize modern man. Inkeles, in a general prospectus, notes: "The change in the external condition of modern man is well known and widely documented. . . . It may be summarized by reference to a series of key terms: urbanization, education, mass communication, industrialization, politicization" (1966, p. 139).

Describing the ways that such an environment might mold the individual, Inkeles states: "To perform effectively in contemporary society, one must acquire a series of qualities" (1966, p. 280). Among these are "certain levels of skill in the manipulation of language and other symbol systems, skills in interpersonal relations which permit negotiation, insure protection of one's interests . . . ; motives to achieve, to master, to persevere . . . a mind which does not insist on excessively premature closure, is tolerant of diversity and has some components of flexibility" (1966, pp. 280-81).

Although Inkeles's concept of modernity was formulated in the study of developing nations, it can also be used in the context of American society to throw light on the reasons for and the nature of the effects of the types of environmental conditions with which we are concerned. His description of modern conditions fits our definition of a complex, multifaceted environment in that such modern conditions increase the complexity of the decisions the individual is required to make and the number of alternate courses of action open to him.

METHODS

The data used in this paper were collected as part of the study conducted by Kohn and Schooler (1969) on the social-psychological conse-

quences of occupational experience. Interviews were conducted with 3,101 men representative of all men throughout the United States employed in civilian occupations. These interviews were conducted for us by the National Opinion Research Center in spring and summer 1964 (see Sudman and Feldman 1965, for a general description of the sampling methods). About half the interview questions were directed to job, occupation, and career; the remainder, to the background information, values, and orientations—with which the present paper deals.

Of the men originally chosen to be in the study, 76% gave reasonably complete interviews, the median interview taking two and one-half hours. The overwhelming majority of nonrespondents were either unavailable or insufficiently interested to give what they were told would be a very long interview.

This paper focuses on the effects of five variables: age, father's education, rurality of childhood, region of the country in which raised, and religious background. All these variables share four characteristics: (1) they are determined by the circumstances of an individual's birth and are hence antecedent to and not affected by his adult orientations, values, and functioning; (2) they serve to locate the individual and his family of origin in the general social context of American society; (3) they are related to degree of modernity; and (4) they can be directly related to the degree of complexity of the environment and the degree of freedom the individual is likely to have experienced while growing up (see below).

Age

It is a truism, but a useful one, that younger people have been raised in a more modern society than were the older ones. In their formative years, each generation is being increasingly exposed to a wider variety of ways of doing things through the expansion of mass media and through increased geographic mobility (Williams 1970, pp. 15–16). These same phenomena may also be serving to expand aspirations by making a wider variety of goals appear increasingly feasible.

There is also evidence that there has been a change in childrearing values and behavior. If, as Inkeles states, "concern about ultimate playing of social roles is the decisive element in the child-rearing behavior of most parents" (1966, p. 279), it would be expected that contemporary parents would attempt to socialize their children to develop the self-directedness, intellectual flexibility, and interpersonal skills necessary to perform effectively in contemporary society. Some confirmation of such a trend in childrearing behavior is provided by Bronfenbrenner (1958). After assessing the available evidence, he concludes that there has been an increase in flexibility in infant care, an increase in the emphasis placed

LINEAR WEIGHTINGS USED FOR ANTECEDENT VARIABLES

| Variable | Weighting |
|--|-----------|
| 1. Age: | |
| 16-20 | 1 |
| 21-25 | 2 |
| 26-30 | 3 |
| 31-35 | 4 |
| 36-40 | 5 |
| 41-45 | 6 |
| 46-50 | 7 |
| 51-55 | 8 |
| 56-60 | 9 |
| 61-65 | 10 |
| 66 and over | 12 |
| 2. Father's education: | |
| Postgraduate | 0 |
| College graduate | 1 |
| Some college | 2 |
| High school graduate | 3 |
| Some high school (to 11 years) | 4 |
| Grade school graduate (8 years) | 5 |
| Some grade school (to 7 years) | 6 |
| 3. Rurality in childhood: lived: | |
| Mostly city | 1 |
| Mostly village or small town | 2 |
| Mostly on farm | 3 |
| 4. Region in which reared: | |
| Mountain and Pacific states | 1 |
| Great Lakes and Great Plains | 2 |
| New England and Middle Atlantic | 3 |
| Southwest (almost entirely Texas) | 4 |
| Border states and Southeast | 5 |
| 5. Religion in which reared: | |
| Jewish | 1 |
| Old established (Episcopalian, Congregationalist, Presbyterian) | 2 |
| Lutheran | 3 |
| Catholic | 4 |
| Methodist | 5 |
| Baptist and transitional sects | 6 |
| Fundamentalist | 7 |

NOTE.—Weightings are ranked within categories according to hypothesized multifacetedness: from most to least.

on self-direction by the child, and a greater tolerance by parents of the child's impulses and desires.

As a consequence, it would be expected that today's younger people would show more of the effects of growing up in a complex environment than older ones. They should be less tolerant of external constraints on their behavior and more concerned and involved with their internal lives, and should function at a more efficient intellectual level.

Father's Education

In the present study, father's education is the best available index of social class of origin. There are several reasons to believe that those brought up in a middle-class environment have more freedom and experience with self-direction than individuals from working-class origins. The results of a series of studies indicate that the parental values of the middle class stress self-directedness, while those of the working class stress conformity to external standards (Kohn 1969; Kohn and Schooler 1969). Middle-class parents are probably also more likely to consciously expose their children to a wide variety of intellectual and cultural stimuli. And, certainly, the intellectual stimulation of vacations, trips, etc., is more likely to have occurred for those brought up in middle-class circumstances. Finally, the greater financial resources of a middle-class family give their child greater freedom to pursue his goals.

Besides its use as an index of social class of origin, father's education may also have a direct effect on the environment in which an individual was raised. The data from the Kohn and Schooler study indicate that when such attributes of social class as occupation and income are partialled out, education independently affects individuals: more educated men value self-direction both for themselves and their children, are more open-minded, and have a positive self-image. These trends would operate on a child's environment in the same way as would higher levels of social class, resulting in a richer environment and a greater emphasis on self-directedness.

Urban versus Rural Childhood

Although it is possible to debate whether an urban setting provides more complex nonsocial stimuli than a rural one, the urban scene does appear to be more multifaceted, demanding, and stimulating in terms of social complexity and cultural diversity. Inkeles, in discussing influences affecting the degree of modernization of individuals, states:

Many analysts of the problem propose the urban environment as the next [to education] most important input. The city is itself a powerful new experience. It encourages, and indeed to some degree obliges, the individual to adopt many new ways of life. By exposing men to a variety of ways of living, a wide range of opinions and ideas, increased mobility, more complex resources of all kinds, it accelerates the process of change. At the same time, in the city the prospect is greater that the individual will be relatively free from the obligations and constraints placed on him . . . by his extended kinship ties, . . . and the tight community of his neighbors. [1966, p. 147]

One would therefore expect those raised in urban areas to show more of the hypothesized effects of having grown up in a complex, multifaceted environment than those raised in rural settings.

Region of the Country in Which Raised

The regions of the United States can be ranked according to the complexity of the environments they provide and the levels of existing proscriptions constraining individual behavior. The dimension underlying this ranking of regions is the degree to which they moved from the essentially "manor"-based agrarian economy that predominated in the South—at least in the first half of the 20th century—to a modern economy where the demand for skilled workers has resulted in higher wages and greater expenditures for education.

The meaningfulness of this scheme of ranking regions of the country can be seen when one compares the consistency with which they can be ranked on their geographical proximity to the Old South, on the one hand, and on the average weekly earnings among production workers in manufacturing industries (1965) and the per capita expenditures of state and local governments for education (1966), on the other (U.S. Bureau of the Census 1967). Surprisingly, all of these rankings go consistently from South to Southwest, to Middle Atlantic and New England, to the Great Lakes and Great Plains, and to the Mountain and Pacific States. The modern industrial condition that underlies this ranking of states probably exposes the individual to a wider variety of ways of doing things, and probably places fewer proscriptions on his behavior than a "feudalistic" system in which the interactions between individuals are carefully regulated by both legal rules and clearly articulated informal codes of behavior.

Religion

Although in the past the variable of religion has generally been dealt with in terms of a Catholic/Protestant/Jewish trichotomy, much of the recent work in this area (Stark and Glock 1968) suggests that more meaningful results can be obtained if Protestantism is not considered as a homogeneous entity. In this paper, religions are ranked from fundamentalism to modernism. The basic position characterizing fundamentalism has been described as "an acceptance of religious revelation, apart from science and fallible human reason; a belief in the absolute accuracy and unchangeable truth of the original divinely inspired Bible; belief in a distinct supernatural order; acceptance of Jesus as the Christ who brings salvation; belief in resurrection and an afterlife in Heaven or Hell"

(Williams 1970, p. 400). On the other hand, "the modernist or 'liberal' movements deviate from the [fundamentalist] position just sketched in their attempts to reconcile their beliefs with the findings of science and of history and to apply Christian doctrines to modern social problems. The broad inclination of modernism toward ethics rather than salvation and toward rationalism rather than traditional beliefs has apparently appealed to the relatively secularized elements of Protestantism" (Williams 1970, p. 401).

Specifically, this ranking of religions is based on (1) the literalness and unquestioningness with which practitioners interpret the Bible as the direct word of God and (2) the degree to which full membership involves an act of adult commitment, such as adult baptism, contrasted with more liberal movements where membership often occurs by means of inheritance.¹

It is predicted that religious upbringing would affect the individual's adult attitudes and functioning because fundamentalist religions generally accustom people to think that desired goals, such as avoiding damnation, can be achieved through the acceptance of a specified set of beliefs and proscriptions on thought and behavior, that questioning is dangerous, and that deviation is sinful. Furthermore, those religions in which full membership is dependent upon ritualized voluntary adult commitment may well be more binding on adult behavior than are those in which membership tends to be inherited. In any case, this ranking of religions probably reflects the degree to which the religion of one's formative years limits one's freedom in thought and action.

Dependent Variables

Three classes of effects result from one's having been raised in a complex, multifaceted environment: (1) intolerance of external constraints, (2) subjectivism marked by a strong awareness of and concern with the quality of one's internal life, and (3) maximal development of the individual's intellectual capacity. All of the dependent variables measuring these effects are derived from a series of orthogonal principal-component factor analyses.²

1. Intolerance of external constraints would cause the individual to reject authority, to feel free to act in a nontraditional way, and to see

¹ The religions from the liberal pole to the center—Jewish, old established (Episcopalian, Congregationalist, Presbyterian), Lutheran, and Catholic—were all established or semi-official religions.

² The factor analyses performed were of the orthogonal principal-component type rotated to simple structure through the varimax procedure and based on the computer program presented by Clyde, Cramer, and Sherin (1966).

himself as responsible for his own fate. The variables used to investigate such attitudes are factors from a factor analysis of 57 questions about the individual's self-conceptions and social orientations.³ *Authoritarian conservatism* is the most important of these. It emphasizes obedience to authority of both leaders and parents and an intolerance of nonconformity. *Stance toward change* measures men's receptiveness or resistance to innovation and change. *Pragmatic morality* involves the rejection of objective moral standards which call for the individual to go beyond obeying the letter of the law; instead, it is directed toward getting the individual what he wants while he keeps out of trouble with authority. *Attribution of responsibility* measures men's sense of being controlled by outside forces as opposed to a feeling of having some control over their own fate.

2. Subjectivism is most readily measured by the values and goals of the individual, specifically those which reflect his overriding concern for the quality of his internal life. Factor analysis of the values an individual holds for himself produces two factors, both of which are related to subjectivism.⁴ The first factor—*self-evaluating values*—reflects a preference for values that derive from the individual's evaluation of his own psychological functioning: an interest in how and why things happen, his feeling that he uses good sense and sound judgment, the ability to face facts squarely, and a rejection of values that emphasize socially acceptable behavior—respectability, truthfulness, and success. The second factor—*self-oriented values*—contrasts the individualistically oriented values of self-reliance and responsibility with the externally oriented values of getting along with people and the ability to do many things well.

The factors which describe the standards by which individuals judge their jobs may also reflect differences in the emphasis they place on the quality of their inner lives.⁵ One of these factors, the *valuation of intrinsic qualities of the job*, focuses on how much opportunity the job provides for using one's abilities, how interesting the work is, and how much opportunity the job offers to help people. The second factor focuses on the *disvaluation of the extrinsic benefits of the job*, such as hours of work, how tiring the work is, and the importance of not being under too much pressure. This primary emphasis on the quality of one's internal life is additionally reflected in a factor—*life goal: seeks pleasure for self*—

³ For the derivation of these indices and their factor loadings, see Kohn (1969, pp. 265–69) or Kohn and Schooler (1969, pp. 666–69). For a detailed description of the interview schedule, see Kohn (1969).

⁴ For a fuller description of the derivation of these indices, see Kohn and Schooler (1969, p. 664), where the self-evaluating factor is identified as self-direction/conformity and the self-oriented factor, as self-direction/competence.

⁵ For a fuller description of the derivation of these indices and their factor loadings, see Kohn and Schooler (1969, p. 666).

which derives from a factor analysis of the goals the individual wants to attain by the age of 70.⁶ This score indicates a preference for having had many pleasurable experiences rather than having done something for the benefit of others or gaining truth and understanding.

3. Intellectual functioning is measured through three variables. Two of these—*ideational* and *perceptual flexibility*—are based on the factor analyses of several diverse measures of intellectual performance. *Ideational flexibility* is determined by those variables in the factor analyses that require weighing both sides of an economic or social issue and by the interviewer's evaluation of the respondent's intelligence. *Perceptual flexibility* is determined by a test of the ability to differentiate figure from ground in complex color designs (Witkin et al. 1962), and a test of the ability to draw a recognizable human figure whose parts fit into a meaningful whole (Goodenough 1926).⁷ A third variable, based on a factor analysis of questions about leisure-time activities, focuses on the intellectual demand-iness of those activities. The relevant factor contrasts spending large amounts of leisure time watching TV and reading popular magazines with engaging in such intellectually active pursuits as going to museums and plays, reading books, and working on hobbies.

⁶ Respondent was given the following list of statements that he might make when reviewing his life at age 70: (1) I have had many pleasurable experiences. (2) I have been loved. (3) I have had respect and recognition. (4) I have gained truth and understanding. (5) I have appreciated the beautiful things in life. (6) I have done something for the benefit of others. (7) I have gained great wealth. (8) I have lived a moral life. He was then asked to pick the three most and single most, three least and single least important of them. A low rank on an item means that it was highly valued; a high rank means that it was disvalued. The factor loadings over .250 were: wants pleasure, —.74; wants truth, .66; wants to benefit others, .53; wants a moral life, .26. Thus, people with high scores on this factor disvalue truth, benefiting others, and morality as life goals, and value pleasure for themselves.

⁷ Specifically, we asked: "Suppose you wanted to open a hamburger stand and there were two locations available. What questions would you consider in deciding which of the two locations offers a better business opportunity?" and "What are all the arguments you can think of for and against allowing cigarette commercials on TV? First, can you think of arguments *for* allowing cigarette commercials on TV? And can you think of arguments *against* allowing cigarette commercials on TV?" The perceptual test consists of a portion of Witkin's (Witkin et al. 1962) Embedded Figures Test, selected by Witkin. The Figure-Drawing Test (see Witkin et al. 1962, pp. 117–29) consists simply of asking the respondent to draw a figure of a man on a standard-size card with a standard pencil. Respondents are reassured that artistic ability is not required. The meaningful coherence of the figure is what we appraise. Ideational flexibility is based on the interviewer's appraisal of the respondent's intelligence (0.69), the cigarette-commercials problem (0.61), the respondent's "agree" score (—0.56), the hamburger-stand problem (0.54), and the Embedded Figures Test (0.52). Perceptual flexibility is based primarily on the Draw-A-Man Test (summary score, —0.91; Goodenough estimate of intelligence, —0.91). and the Embedded Figures Test (—0.43). For the derivation of these indices and their factor loadings, see Kohn (1971, pp. 464–65).

RESULTS

The data indicate that being raised in a complex multifaceted environment does, in fact, produce intolerance of external constraints, efficient intellectual functioning, and an increase in subjectivism (table 1).⁸ In the

TABLE 1
RELATIONSHIP BETWEEN ANTECEDENT AND DEPENDENT VARIABLES;
SIGNIFICANT LINEAR η 'S: ALL IN PROPER DIRECTION

| | FATHER'S | | | | |
|--|--|-----------|----------|------------|-------------------|
| | AGE | EDUCATION | RURALITY | REGION | RELIGION |
| | The more complex multifaceted end of the dimension is: | | | | |
| | Youth | High | Urban | West Coast | Nonfundamentalist |
| Rejection of external constraints: | | | | | |
| Not authoritarian-conservative | .22** | .24** | .22** | .20** | .20** |
| Attribution of responsibility: to self ... | .07** | .09** | .06** | .05* | .04 |
| Pragmatic morality | .14** | ... | ... | ... | .05 |
| Stance toward change (receptive) | .16** | .10** | .11** | .12** | .07** |
| Canonical ^a | .31** | .27** | .25** | .24** | .22** |
| Intellectual functioning: | | | | | |
| Intellectually demanding use of leisure time | .05* | .28** | .22** | .19** | .24** |
| Perceptual flexibility | .27** | .12** | .20** | .18** | .06* |
| Ideational flexibility | .14** | .27** | .20** | .20** | .20** |
| Canonical | .31** | .35** | .31** | .29** | .27** |
| Subjectivism: | | | | | |
| Seeks pleasure for self | ... | ... | ... | .08** | .08** |
| Values intrinsic job qualities | ... | .08** | ... | ... | ... |
| Disvalues extrinsic job qualities | .13** | .23** | .16** | .22** | .21** |
| Self-evaluating values | .07** | .14** | .09** | .06 | .10** |
| Self-oriented values | .05* | .10** | .09** | .09** | ... |
| Canonical | .15** | .28** | .19** | .25** | .24** |

NOTE.—All listed η 's and canonical correlations are significant at least at $P < .05$. Here η is calculated by taking the square root of the ratio of (a) either linear or nonlinear "between groups" sum of the squared deviations from the mean, to (b) the total sum of squared deviations from the mean. It is directly analogous to the product-moment correlation coefficient, in that it represents the square root of the proportion of the variation in the dependent variable that can be attributed to the independent variable. When one deals with the linear component of the independent variable, η is identical with the product-moment correlation coefficient, except that its sign is always positive.

^a The canonical correlation is a multiple correlation of one or a set of independent variables to a set of dependent variables. More precisely, it is the maximum correlation between linear functions of the two sets of variables.

* $P < .01$.

** $P < .001$.

⁸ The analyses reported in this paper are based on the computer program for multivariate analysis of variance initially developed by Clyde, Cramer, and Sherin (1966) and further developed by Cramer.

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analysis of the relationships between the antecedent variables and adult attitudes reflecting the rejection of external constraints on behavior, multivariate analysis of variance reveals that each canonical correlation between the set of dependent variables indexing rejection of external constraints and each of the five antecedent variables indexing environmental complexity is significant. Examination of the constituent univariate analyses indicates that 17 out of 20 analyses which link the four dependent variables with the five independent variables are also significant and in the expected direction. Thus, authoritarianism, fatalism, and resistance to change are related to old age, having been born in the South, having had a rural childhood, having been raised with a fundamentalist religious background, and having had a father with a low level of education, while pragmatic morality is related only to being younger and having a nonfundamentalist religious background.⁹

Similarly, strong evidence is found for the relationship between the antecedent variables and present intellectual functioning. All five of the canonical correlations of the relevant multivariate analyses are significant, and all 15 of the analyses relating the dependent variables of perceptual flexibility, ideational flexibility, and the intellectually demanding use of leisure time to the five antecedent variables are significant in the predicted direction. Being young, having a well-educated father, being born far from the South, being raised in an urban environment, and being raised in a home with a nonfundamentalist religion result in generally higher intellectual performance at both the perceptual and ideational level and in the intellectually demanding use of leisure time. Thus, here too, as in the case of the dependent variables representing the rejection of external constraints, the predicted effect of having been raised in a multifaceted complex environment—in this case the full development of the individual's intellectual capacity—has been borne out.

A relationship between being raised in a complex environment and greater subjectivism is also confirmed. All of the relevant multivariate canonical correlations are significant. In terms of the individual analyses of variance, the possession of self-judging values is appropriately related to each of the five antecedent variables, as is the devaluing of extrinsic job qualities. Self-oriented values are significantly and appropriately related to each of the antecedent variables except religion. Seeing pleasure as a life goal is related to age and region, while valuing intrinsic aspects of the job

⁹ The only three nonsignificant relationships are between pragmatic morality and region, rurality, and father's education. Among those respondents who had children between three and 15 years of age, the poles of the dimensions of region, rurality, religion, and father's education indicative of having been raised in a complex environment were also significantly related to valuing self-direction rather than conformity to externally imposed rules in their children's behavior.

is related only to father's education. Thus, although not every hypothesized trend proved significant, the predicted relationships between those poles of the antecedent variables indicative of being raised in a complex, multifaceted environment and the occurrence of a greater degree of self-concern and awareness are borne out.

All in all, the results strongly suggest that background characteristics indicative of a complex childhood environment—being young, having a well-educated father, being raised in an urban environment, in a liberal religion, and in a region far from the South—result in efficient intellectual functioning, intolerance of external constraints, and a subjective orientation.

ALTERNATE EXPLANATIONS

Although the above results are congruent with our hypotheses, other explanations are possible. It would be useful to examine these alternate explanations and to present evidence which, if it does not entirely rule out such alternatives, does make them seem unlikely.

Alternative 1: the findings are due to the arbitrary linearization and weighting of the social background variables and as such merely represent capitalization on chance.—It is obvious that the rankings of the categories of the background variables, although based upon empirical differences in those variables, do not represent the only way in which they can be ordered. Aside from the possibility that these results are merely due to the arbitrary ranking and weighting of the categories, it is also possible that for a given background variable there may be other dimensions than the one used here and that these other dimensions may explain a significant part of its variance.

Besides pointing to the cohesiveness of the results obtained as evidence of the centrality of the dimensionalizations used, it is also possible to assess directly the degree to which the present dimensionalization of a particular background variable explains the variance which that variable causes among the dependent variables. This can be done by comparing the proportion of the variance accounted for by the linear trends (i.e., by the rankings and equality of intervals assumed by the present dimensionalizations) with that accounted for by nonlinear trends (i.e., the variance which violates our model). Such an examination reveals that where significant nonlinearity occurs it is almost always subsidiary to a more powerful linear trend. In each of the multivariate analyses the linear canonicals are greater than the nonlinear ones, while in the 65 univariate analyses performed there are only two cases in which there are significant nonlinear effects in the absence of significant linear effects. Nor is that nonlinearity which does exist a function of the "artificial" linearization of such variables as region

and religion. The nonlinear canonicals for age, the most obviously linear of the antecedent variables, and the only one which can be considered a true ratio scale, are just about twice as large as those for region and of about the same magnitude as those for religion.

Much of the nonlinearity which does occur is not the result of a violation of the predicted ordinal relationship between the categories of the antecedent variables. It occurs, rather, because the intervals between adjacent categories are not exactly equal. Thus, although there were 54 cases in which there were significant linear trends, there were only 11 cases in which there was a violation of the predicted ordinal relationships, that is, in which a category was out of the predicted order. Furthermore, six of these violations occurred when age was the independent variable, and these were primarily due to the youngest respondents—those under 25. Since working-class individuals would be overrepresented among those in this age group who worked full time, it is not surprising that when the effects of social class are covaried, the nonlinear component of the age variance is generally reduced and is significant in only three analyses. Since the five cases in which there was a significant nonlinear effect among the other independent variables do not represent major violations¹⁰ of the theoretical and empirical rationale underlying their dimensionalization, the significant nonlinear trends which do occur should not becloud the general consistency with which the present dimensionalization of the background variables has successfully predicted both the directionality and the ranking of the dependent variables.

Alternative 2: all of the results are merely due to social class differences.—Almost all of the predicted effects of being raised in a complex multifaceted environment are the same as those characteristics which distinguished middle-class from working-class men in the Kohn and Schooler study (1969). Furthermore, since one of the background variables—father's education—does, in fact, directly index social class, albeit not of the individual as an adult but rather of the environment in which he was raised, and since there is a correlation between this variable and some of the other background variables, it could be argued that the above results are due mainly to father's education. However, when the effects of social class of origin as measured by father's education are removed, all of the canonicals remain significant and none is reduced more than 25%. Thus,

¹⁰ Even the five violations of the ordinal trends which did occur were not substantial, involving at most a displacement of two levels from expected ordinal position. In terms of these expectations, those from the Southwest, who were mostly Texans, saw themselves as being more in control of their own fate; sons of college graduates were less receptive to innovation; Catholics were more fatalistic; both Catholics and fundamentalists were more prone to a legalistic pragmatic morality; and those from old established religions were less receptive to change than would be expected on the basis of their position on the relevant dimensions.

although social class of origin may explain some of the effects of the four other background variables, the evidence strongly suggests that the effects of these variables, though similar to those of social class of origin, are independent of it.

Despite the fact that father's education by itself explains little of the effects of the other background variables, examination of its interaction with these other background variables reveals a very interesting pattern. All of the significant interactions are with age. Thus, there were significant interactions between father's education and age for authoritarianism, intellectually demanding use of leisure time, disvaluing extrinsic job benefits, and holding self-evaluating values (table 2). Furthermore, all of these interactions were in the same direction: father's education has a greater effect among younger than among older respondents. The results indicate that the difference between those from middle-class and those from working-class origins is greater among the young than the old.

TABLE 2
SIGNIFICANT η 's OF THE RELATIONSHIP BETWEEN ANTECEDENT AND DEPENDENT
VARIABLES CONTROLLING ON FATHER'S EDUCATION

| | Age | Interaction: Father's Education × Age | Rurality | Region | Religion |
|--|-------|--|----------|--------|----------|
| External constraints: | | | | | |
| Not authoritarian-conservative | .20** | .06* | .20* | .15** | .20** |
| Attribution of responsibility: to self ... | .05 | ... | ... | ... | .07* |
| Pragmatic morality | .17** | ... | ... | ... | .07* |
| Stance toward change: | | | | | |
| receptive | .15** | ... | .07** | .10** | .15** |
| Canonical | .30** | .08* | .22** | .19** | .27** |
| Intellectual functioning: | | | | | |
| Intellectually demanding use of leisure time | ... | .05 | .17** | .14** | .19** |
| Perceptual flexibility | .26** | ... | .20** | .16** | ... |
| Ideational flexibility | .09** | ... | .15** | .12** | .15** |
| Canonical | .29** | .06 | .28** | .22** | .22** |
| Subjectivism: | | | | | |
| Seeks pleasure for self | ... | ... | ... | .09** | .08** |
| Values intrinsic job qualities | .04 | ... | ... | ... | ... |
| Disvalues extrinsic job qualities | .09** | ... | .11** | .19** | .20** |
| Self-evaluating values | .05 | .05 | .08** | ... | .09** |
| Self-oriented values | ... | ... | .07** | .08** | ... |
| Canonical | .12** | .07 | .16** | .23** | .23** |

NOTE.—See footnotes to table 1.

* $P < .01$.

** $P < .001$.

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Despite the general similarity between the young and those coming from middle-class backgrounds in self-directedness, intellectual interest, and concern for the quality of their inner lives, the differences between the social classes in these characteristics appear to be increasing with time.

A second possibility raised by the general similarity between the effects of the background variable and the effects of social class is that the background variables affect the dependent variables primarily by determining the individual's social class as an adult. However, table 3, which shows the

TABLE 3
SIGNIFICANT η 'S OF THE RELATIONSHIP BETWEEN ANTECEDENT AND DEPENDENT
VARIABLES; LINEAR η : COVARYING SOCIAL CLASS

| | Age | Father's Education | Rurality | Region | Religion |
|--|-------|-----------------------|----------|--------|----------|
| Rejection of external constraints: | | | | | |
| Not authoritarian-conservative | .21** | .12** | .12** | .14** | .11** |
| Attribution of responsibility: to self ... | .06* | ... | ... | ... | ... |
| Pragmatic morality | .15** | ... | .08** | ... | .10** |
| Stance toward change: | | | | | |
| receptive | .14** | .07* | .08** | .10** | .04* |
| Canonical | .30** | .15** | .17** | .18** | .15** |
| Intellectual functioning: | | | | | |
| Intellectually demanding use of leisure time | ... | .16** | .13** | .13** | .13** |
| Perceptual flexibility | .26** | .06* | .17** | .16** | ... |
| Ideational flexibility | .12** | .12** | .07** | .12** | .09** |
| Canonical | .31** | .19** | .22** | .22** | .15** |
| Subjectivism: | | | | | |
| Seeks pleasure for self | ... | ... | ... | .09** | .10** |
| Values intrinsic job qualities | .04 | ... | ... | .05 | .05 |
| Disvalues extrinsic job qualities | .11** | .10** | .16** | .17** | .14** |
| Self-evaluating values | .06* | .09** | .10** | ... | ... |
| Self-oriented values | .04 | .06* | .08** | .07* | ... |
| Canonical | .13** | .14** | .20** | .21** | .18** |

NOTE.—See footnotes to table 1.

* $P < .01$.

** $P < .001$.

relationship between the background and dependent variables when adult social class is controlled, indicates that a fairly strong and consistent pattern of relationships remains even when all of the joint variance is assigned to social class. In any case, this latter procedure represents an overly strict method of assessing the strength of the background variables under consideration. For one thing, it is possible that the consequences of being raised in a complex, multifaceted environment are functional for

achieving middle-class status and not merely the consequences of such status. Second, leaving aside the possibility that more intelligent, self-directed, and self-aware individuals may well be more likely to become middle class, there is no reason to assign the effects of the variance shared by the background variables and social class to social class alone, since the background variables are temporally prior to present social class and hence, if not the cause of present social class, certainly are not caused by it.¹¹

Alternative 3: some of the antecedent variables may not have had independent effects, their apparent effects being due to the joint variance they share with other background variables.—Just as we have considered the possibility that the other childhood background variables might merely reflect father's education, so each of the background variables including father's education may merely reflect one or some combination of the others. In order to insure that the background variables do, in fact, have independent effects and that the findings are not spuriously due to interrelationships among them, all five background variables were used as independent variables in a series of multiple regression analyses predicting each of the study's dependent variables. Under such conditions the standardized β weight for a given independent variable indicates "how much change in the dependent variable is produced by a standardized change in one of the independent variables when the others are controlled" (Blalock 1960, p. 345). As can be seen from the significant β weights in table 4, 40 of the 49 analyses which were significant in table 1 remain significant, and two that were not significant become significant.¹² Thus,

¹¹ The Hollingshead index of social position used in the above analyses as a measure of social class is based on a weighted combination of education and occupational position, and so can perhaps be seen as an oversimplified index of complex phenomena. The two aspects of social class most frequently mentioned as being overlooked by the Hollingshead index are prestige and income. In terms of the former, although the Hollingshead index was originally validated only against expert opinion, within the present sample it is correlated at .89 with Duncan's system (1961), which was based on weighting the categories of the U.S. Census classification to conform to the judgments held by society at large of occupational prestige. In order to test the possibility that income was an important determinant of the psychological characteristics under consideration, income was covaried both by itself and together with social class. By itself income had a much weaker effect than did social class in reducing the magnitude of the effects of the background variables, and when it was covaried with social class, it did not add substantially to the power of the latter variables.

¹² The two relationships which become significant when all of the other background variables are controlled are that young people are more likely to seek pleasure for themselves and that those with more highly educated fathers are less likely to hold a pragmatic morality. The former finding follows our hypothesis exactly; the latter is congruent with the discussion of the comparison of the effects of being raised in a complex multifaceted environment and being a middle-class adult presented in the later section on Individuation, Alienation, and Morality. The multiple regression program employed here is that by Nie, Bent, and Hull (1970, chap. 15).

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TABLE 4

SIGNIFICANT β WEIGHTS WHEN ALL BACKGROUND VARIABLES ARE IN THE EQUATION:
POSITIVE β WEIGHT INDICATES HYPOTHESIZED RELATIONSHIPS

| | Age | Father's Education | Rurality | Region | Religion |
|--|------|-----------------------|----------|--------|----------|
| Rejection of external constraints: | | | | | |
| Not authoritarian-conservative | .19 | .14 | .10 | .11 | .11 |
| Attribution of responsibility: to self ... | .05 | .07 | ... | ... | ... |
| Pragmatic morality | .15 | -.06* | ... | ... | .07 |
| Stance toward change: receptive | .15 | .05 | .05 | .09 | ... |
| Intellectual functioning: | | | | | |
| Intellectually demanding use of leisure time | ... | .22 | .12 | .07 | .14 |
| Perceptual flexibility | .25 | ... | .13 | .15 | ... |
| Ideational flexibility | .10 | .20 | .09 | .10 | .10 |
| Subjectivism: | | | | | |
| Seeks pleasure for self | .04* | ... | ... | .08 | .05 |
| Values intrinsic job qualities | ... | .08 | ... | ... | ... |
| Disvalues extrinsic job qualities | .11 | .16 | .04 | .14 | .12 |
| Self-evaluating values | .05 | .11 | .05 | ... | .07 |
| Self-oriented values | ... | .06 | .06 | .06 | ... |

* See n. 12 to text.

the evidence overwhelmingly suggests that each of the background variables has a noticeable independent effect upon the dependent variables under consideration.

Alternative 4: the age effects are due to differences in life-cycle stages.—

The present description of the way in which age has its effects is obviously not the only possible one. A cohort of individuals born in the same year may be similar both because of the many historically determined similarities in the environments in which they grew up and because they are at similar developmental points in their life cycles. Thus in our data, collected in 1964, the 24-year-old respondents differed from the 64-year-old ones both by having grown up during the era of the radio, automobile, and airplane, and by being in relatively early stages of their familial and occupational careers.

This paper's explanation of the effects of age strongly emphasizes the "historical" effects of age and deemphasizes the "life-cycle" effects. In the absence of longitudinal data it would be foolhardy to deny the possibility that differences in life cycle stage are an important cause of the differences between our older and younger respondents. However, several lines of evidence strongly suggest that at least a substantial portion of the age

effect is in fact due to similarities in environmental circumstances brought about by historical and technological change.

The pattern of the effects of age is remarkably similar to that of the other background variables. It would, of course, be possible to overlook the striking coherence of the results and to explain the effect of each of these variables with separate hypotheses and without recourse to the single unifying hypothetical construct of a *complex multifaceted environment* with its dependence on the historical aspects of age. Such explanations would not necessarily be wrong, but they would not be parsimonious.

Relevant empirical data also exist from a Japanese study that examines the similarity of the effects of being young and having a high level of education over a 15-year period. The results strongly suggest that, at least in that country, orientations very similar to the ones with which we are concerned are the results of the historical rather than the life-cycle component of age. Such evidence is found in Hayashi and Nishihira's (Hayashi et al. 1970, pp. 189-90, 213-14) time series and cohort analysis of the *National Character of the Japanese People Survey* that was started in 1953 and subsequently repeated at five-year intervals. Their interest centered on all of those items in the 1953 survey that showed significant differences both between the young and the old and between the higher and lower educational strata. During the 15-year period from 1953 to 1968 those views which were significantly more prevalent among the young and the highly educated in 1953 became more and more prevalent; those views held by the older and less educated became less and less prevalent.

The central direction of such changes also fits the hypothesis of the present paper. As can be seen from table 5, which deals only with changes in aspirations, the new aspirations are individualistically self-directive and subjectivistic, while the older ones tend to involve meeting the requirements of others or of an objective moral code. "According to the cohort analysis there can be no doubt that this clean contrast is the product of two simultaneous occurrences: The appearance during this period of a younger generation agreeing with the new views; and numerous conversions to the new view of older people" (Tominaga 1971, p. 18). Summarizing the findings of the whole questionnaire, Tominaga states: "A content comparison between new views and old views produces a general description of the former as individualistic and rationalistic, and of the latter as collectivistic and traditionalistic. . . . In every example the pattern is similar: Over the past 15 years, new views have gained while old views have declined."

INDIVIDUATION, ALIENATION, AND MORALITY

The rejection of external constraints and the growth of subjectivism which result from being raised in a complex multifaceted environment can be

Antecedents of Psychological Functioning

TABLE 5

JAPANESE LIFE-STYLE ASPIRATIONS: 1953-68

| | PERCENTAGE | | | |
|--|------------|-------|---------|---------|
| | 1953 | 1958 | 1963 | 1968 |
| Aspirations chosen significantly more frequently by young and well-educated in 1953: | | | | |
| To work hard and get rich | 15 | 17 | 17 | 17 |
| To live according to one's own taste, without aiming for either money or fame | 21 | 27 | 30 | 32 |
| To live from day to day in a carefree way without undue worries | 11 | 18 | 19 | 20 |
| Totals: New | 47 | 62 | 66 | 69 |
| Aspirations chosen significantly more frequently by older and less well-educated in 1953: | | | | |
| To study diligently and become famous | 6 | 3 | 4 | 3 |
| To live cleanly and honestly in all respects, while opposing the wrongs in the world | 29 | 23 | 18 | 17 |
| To devote oneself entirely to the cause of society, disregarding one's own interests | 10 | 6 | 6 | 6 |
| Totals: Old | 45 | 32 | 28 | 26 |
| Others and don't know | 8 | 6 | 6 | 5 |
| Total percentage | 100 | 100 | 100 | 100 |
| N | (2,254) | (920) | (2,698) | (3,033) |

SOURCE.—Arranged from data presented by Tominaga (1971) from Hayashi et al. (1970).

seen as part of a process of individuation in which the individual becomes the unit of central importance both in the selection of means and in the evaluation of ends. It should not be surprising that antecedent variables which place the individual in relatively unstructured situations where his relationships with larger social groupings are at best ambiguous and where he has been exposed to a wide variety of goals and to many alternate means of attaining them should produce such a result. Under such circumstances the individual's fate may not be tied to the fates of the groups to which he belongs; others' expectations of how he should fulfill his roles may play a much smaller part in shaping his behavior.

The effects of this individuation process can be described on several psychological levels. In terms of learning theory, the individual is reinforced—rewarded or punished—by the consequences of an act to himself rather than by its effect on social groups of which he is a member. In terms of ego psychology, the individual identifies with himself rather than with

some larger collective. In Lewinian terms, the individual can be seen as structuring his life space by drawing tight rather than permeable ego boundaries between himself and relevant others.

The linking of an increase in individuation with the growth of subjectivism and the rejection of external constraints on behavior resonates with evidence of other current social trends. Although the data on which this paper is based were gathered in 1964, it is not surprising that at present "mind-expanding drugs" are more popular among the young, those raised in middle-class, West Coast, and nontraditionally religious settings (Blum 1969), while the emphasis on law and order and allegiance to traditional national values is stronger among the older, more rural, more Southern, and more fundamentally religious segments of the population.

Our data also provide evidence that with the passage of time the differences between the social classes in the degree of individuation is increasing. As we saw when we examined the interaction between age and father's education, despite the general similarity between the young and those coming from middle-class backgrounds, the differences between those from different social backgrounds in rejection of external constraints and subjectivism seem to be increasing with time.

The similarity between individuation as we have described it and the concept of alienation is obviously close—close enough to invite and probably demand comparison. Such a comparison is complicated by the fact that the term "alienation" has been used in many different ways. For our purposes these usages provide very useful ways of classifying the similarities and differences between individuation and alienation as they are affected by our five antecedent variables. In a well-known paper Seeman (1959) has specified five different usages: (1) powerlessness—"the expectancy . . . by the individual that his own behavior cannot determine . . . the outcome . . . he seeks" (p. 784); (2) meaninglessness—"the individual's minimal standards for clarity in decision making are not met" (p. 786); (3) normlessness—"a high expectancy that socially unapproved behaviors are required to achieve given goals" (p. 788); (4) value isolation—"low reward value to goals or beliefs typically highly placed in the . . . society" (p. 789); (5) self-estrangement—"the inability of the individual to find self-rewarding the activities that engage him" (p. 790).

An examination of table 6 suggests that the five meanings commonly given to the term "alienation" can be broken into two empirically and theoretically meaningful clusters. One set of three meanings—powerlessness, meaninglessness, and self-estrangement—can be related to seeing oneself in a negative light, as being impotent, ignorant, and worthless. These three are, if anything, decreased in magnitude by the antecedent variables that increase individualism. On the other hand, those aspects of alienation which are concerned with sharing norms with others—normless-

Antecedents of Psychological Functioning

TABLE 6

SIGNIFICANT CORRELATIONS BETWEEN COMPONENTS OF ALIENATION
AND BACKGROUND CHARACTERISTICS

| Aspects of Alienation* | Young | Urban Upbringing | Non- Southern Upbringing | Nonfunda- mentalistic Religion | High Father's Education |
|---|-------|---------------------|--------------------------------|--------------------------------------|-------------------------------|
| Powerlessness: attribution of responsibility (controlled by outside forces) | -.07 | -.06 | -.05 | -.05 | -.09 |
| Meaninglessness: world is not very understandable | ... | ... | -.07 | ... | ... |
| Self-estrangement: self- deprecation | -.06 | -.12 | ... | ... | ... |
| Self-confidence | ... | .07 | ... | ... | .06 |
| Value isolation: idea conformity | ... | .05 | .06 | ... | .07 |
| Normlessness: pragmatic morality | .13 | ... | ... | ... | .06 |

* All of the variables are taken from the factor analysis of the 57 items measuring orientations. The factors attribution of responsibility and pragmatic morality have been described earlier (see n. 3 to text). The factor score self-deprecation measures the self-critical half of self-esteem—the degree to which men disparage themselves; self-confidence measures the degree to which men are confident of their own capacities; idea conformity measures the degree to which men believe their ideas mirror those of the social entities to which they belong. The factor loadings and derivation of these factors can be found in Kohn (1969, pp. 265–69) and Kohn and Schooler (1969, pp. 666–69). The factor score meaninglessness is measured by degree of agreement on a five-point scale; meaninglessness is indexed by response, on a five-point scale of frequency, to the question: "How often do you feel that the world just isn't very understandable?"

ness and value isolation—are, if anything, increased by the antecedent variables which indicate that the individual had been raised in a complex multifaceted society. The process of individuation thus runs counter to those aspects of alienation which make the individual seem inconsequential, and parallel to those aspects which serve to differentiate him from others by his lack of allegiance to commonly held norms.

The finding that being raised in a complex multifaceted environment increases the likelihood that the individual will reject commonly held norms also helps explain the differences between the effects of being raised in such an environment and the effects of adult social class. Although having been raised in a complex early environment and being a middle-class adult both result in relatively good intellectual functioning, feelings of efficacy, and the placing of a high value on self-direction, these two experiences differ somewhat in their effect on the two dependent variables which are most closely related to the individual's moral stance: pragmatic morality and a life goal of pleasure for self.

Both stances are more prevalent among the young, among those raised in religions on the "liberal" end of the continuum, and, when the effects of social class are controlled, among those raised in an urban setting. Having as a life goal pleasure for oneself also characterizes those raised in

states located far from the South. Thus, these characteristics are generally more prevalent among those who were raised in a complex multifaceted environment.

On the other hand, both of these characteristics are more common among working-class than among middle-class individuals. The correlation between pragmatic morality and low social class is .174 ($P < .001$) and between low social class and pleasure for self as a life goal, .077 ($P < .02$). According to both the other results of this analysis and the general tenor of the Kohn and Schooler findings, being at the poles of the antecedent variables characterized by a greater richness of stimuli and relative freedom of action generally have the same results as being middle class.

It should be remembered, though, that the analysis of social class differences in the Kohn and Schooler paper did not indicate that middle-class individuals differ from working-class individuals by being more altruistic. Rather, middle-class individuals differed from those in the working class by greatly preferring that they and their children act on the basis of internalized values rather than in conformity to externally imposed rules. What the middle class valued was not a greater concern for others but the sense of self-direction, the feeling that the locus of control should be within the individual.

Thus, the antecedent variables under discussion have a broader effect than does adult social class. They carry the process of individuation further—perhaps even to the point of selfishness, and certainly to a disregard of certain commonly held norms—through the emphasis they place on the self. The individual's increasing concern over the nature of his internal life and increasing desire for self-direction seem to override concern for the welfare of others or such "objectified" principles as truth and understanding or a morality that extends beyond the letter of the law.

DISCUSSION

Our description of a complex multifaceted environment closely parallels Inkeles's description of modernization. The two concepts are congruent enough and the conditions they describe similar enough that it seems likely that they would have the same effects—both resulting in an increase in individuation. Thus, it is not surprising that, by the 18th century, social theorists in Western society clearly saw that modernity meant an increase in the individual's freedom to pursue his own happiness (Gay 1966, 1969). Philosophers and economists emphasized the importance of protecting the individual's freedom of thought and action from control by the absolute monarchies of the Counter-Reformation or by the remnants of the medieval theocratic-feudal system. Against this optimism the 19th century saw the rise in importance of the concept of alienation, which, as we have seen,

by the middle of the 20th century implied both a lack of allegiance to the norms and values of society and concern over the inability to control one's own destiny. However, our data suggest that although the processes of modernization and individuation may sometimes result in the rejection of society's norms and values, such processes tend to increase the individual's sense of his own importance and potency in relation to others. The question then arises as to why the increase in concern over alienation by social theorists. Some of this increase is obviously due to a concern over the negative effects on both society and the individual of the general decrease in allegiance to traditional value systems. However, another cause of concern over alienation, particularly when it is defined in terms of the powerlessness of the individual, may have come about because the processes making for individuation may have increased the general desire for self-direction more than the possibility of its actual attainment, resulting in a greater disparity between aspiration and reality than existed heretofore.

Our data suggest another, and even more disconcerting, incongruity, not between the demands of the individual and the possibilities of the situation, but rather between the demands of the situation and the possibilities of the individual. Our findings give us reason to believe that increased intellectual flexibility is one of the hallmarks of those raised in the increasingly complex conditions characterizing the modern world. Nothing in our data, however, tells us anything about the relative magnitude of the two: the increase in modern man's intellectual flexibility and the increase in the complexity of the modern world in which he finds himself.

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Ethnic and Environmental Influences on Mental Abilities

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The extent to which ethnic group differences in intellectual test performance can be attributed to environmental forces remains unresolved. In this study, the relationship between a refined measure of the learning environment of the home and the levels and profiles of a set of mental ability scores of children from five Canadian ethnic groups was examined. After accounting for the "independent" influence of the environment, significant ethnic group differences persisted in the verbal, number, and reasoning ability scores, and in the profiles of the scores. Only for spatial ability were the ethnic group differences absorbed by the environmental differences.

The controversies generated by the work of Coleman et al. (1966), Jensen (1969), and Eysenck (1971) have indicated the inadequacies of the research investigating the interrelationships among the constructs of ethnicity, environment, and intellectual ability. Much of the research has been limited by inadequate measures of both the environment and student performance. Environment has generally been defined in terms of social status characteristics such as the occupation of the father and the education of the parents or family structure variables such as family size and crowding ratio of the home. Similarly, global measures of intelligence have tended to be used, to compare the intellectual performance of ethnic groups. The use of these gross indicators of the environment and of intellectual ability has obscured important differences that exist among ethnic groups.

To overcome some of the shortcomings of previous examinations of ethnic group differences in intellectual ability, this study investigated the relationship between a refined measure of the learning environment of the home and ethnic group differences in the levels and profiles of a set of mental ability test scores. The following questions were examined: (1) What is the relationship between ethnic group membership and mental ability test scores? (2) What is the relationship between ethnic group membership and environmental force scores? (3) What is the relationship between the environmental force scores and the mental ability test scores? (4) To what extent is the learning environment of the home related to ethnic group differences in mental ability test scores?

METHOD

Mental Abilities

The levels and profiles of four mental abilities were examined: verbal, number, spatial, and reasoning. These abilities were operationalized by the scores on the relevant SRA Primary Mental Abilities subtests (1962 rev. ed.).

Environment

The environment of an individual is composed of a complex network of forces; a subset of the total network of forces is related to each human characteristic.¹ Therefore, for verbal, number, spatial, and reasoning ability, it was proposed that subenvironments or subsets of environmental forces could be identified which would be related to each of the mental abilities.

The union of the four subenvironments was defined as the learning environment. Such an environment may be present in the home, the community, and the school. Of these environments, the home produces the first and perhaps the most subtle influence on the mental ability development of the child. As a result, the learning environment of the home was chosen as the focus of the study.

A set of eight environmental forces was identified from a review of relevant theoretical and empirical literature (Strodtbeck 1958; Rosen 1959; Bing 1963; Dave 1963; Wolf 1964; Plowden et al. 1967; Vernon 1969; Weiss 1969). These forces were labeled: (1) press for achievement, (2) press for activeness, (3) press for intellectuality, (4) press for independence, (5) press for English, (6) press for ethlanguage,² (7) mother dominance, and (8) father dominance. Each of the environmental forces was defined by a set of environmental characteristics assumed to be the behavioral manifestations of the environmental forces.

The environmental characteristics provided the framework for the construction of a new environmental measure. The instrument, in the form of a semistructured home interview schedule, was used to elicit responses from both mothers and fathers. For each item in the questionnaire a set of alternate responses was supplied to the interviewer. In addition, an

¹ See Bloom (1964), who suggests that "somewhere between the total environment and the specific interaction or single experience is the view of the environment as a set of persisting forces which affect a particular characteristic." This concept of the environment has been developed in a number of unpublished doctoral dissertations from the University of Chicago, including Dave (1963), Wolf (1964), and Weiss (1969).

² Ethlanguage refers to any language other than English which is spoken in the home.

Influences on Mental Abilities

THE ENVIRONMENTAL FORCES AND THEIR RELATED ENVIRONMENTAL CHARACTERISTICS USED IN THE INTERVIEW SCHEDULE

| Environmental Forces | Environmental Characteristics* |
|------------------------------------|--|
| 1. Press for achievement | 1a) Parental expectations for the education of the child 1b) Social press† 1c) Parent's own aspirations 1d) Preparation and planning for child's education 1e) Knowledge of child's educational progress 1f) Valuing educational accomplishments 1g) Parental interest in school |
| 2. Press for activeness‡ | 2a) Extent and content of indoor activities 2b) Extent and content of outdoor activities 2c) Extent and the purpose of the use of TV and other media |
| 3. Press for intellectuality§ | 3a) Number of thought-provoking activities engaged in by children 3b) Opportunities made available for thought-provoking discussions and thinking 3c) Use of books, periodicals, and other literature |
| 4. Press for independence | 4a) Freedom and encouragement to explore the environment 4b) Stress on early independence |
| 5. Press for English | 5a) Language (English) usage and reinforcement in the home 5b) Opportunities available for language (English) usage in the home |
| 6. Press for ethlanguage | 6a) Ethlanguage usage and reinforcement in the home 6b) Opportunities available for ethlanguage usage in the home |
| 7. Father dominance | 7a) Father's involvement in child's activities 7b) Father's role in family decision making |
| 8. Mother dominance | 8a) Mother's involvement in child's activities 8b) Mother's role in family decision making |

* The environmental characteristics were selected from the studies by Bing (1963), Dave (1963), Wolf (1964), Vernon (1969), and Weiss (1969).

† Social press refers to the press for achievement which might be supplied by persons such as brothers, sisters, grandparents, other relatives, neighbors, and visitors to the home.

‡ This force stresses the interaction which takes place between members of a family when they are involved in educational activities. The nature and quality of the activities of a family are likely to determine the quality and variety of experiences to which a child is exposed.

§ Press for intellectuality refers to the challenging nature of the environment which is provided for the child. The challenge may be provided, for example, in the form of thought-provoking experiences presented through toys, games, hobbies, and discussions.

|| This characteristic refers to the ages at which parents expect their sons to accomplish activities by themselves.

"other answer" space was provided so that the interviewer could record a response which was not covered by those supplied.

A six-point rating scale was developed in order to score each item in the schedule. The score for each of the environmental characteristics was obtained by summing the scores on the relevant environmental items. The score for each of the environmental forces was obtained by summing the scores on the relevant environmental characteristics.

As well as obtaining a measure of the intensity of the present learning

environment operating in the home, the schedule attempted to gain a measure of the cumulative nature of the learning environment. For example, as well as asking, "What kind of job would you like him to have when he grows up?" the schedule also included a question to estimate how long these expectations had been held. Three preliminary tests of the schedule were made before the final questionnaire was adopted. Families from each of the ethnic groups were included in the pilot testing of the instrument. The final questionnaire took approximately two hours to administer.

The Sample

Families from five ethnic groups residing in Canada were included in the study: Canadian Indians, French Canadians, Jews, southern Italians, and white Anglo-Saxon Protestants.

Approximately 100 11-year-old boys from each ethnic group were tested, using first the California Test of Mental Maturity and then the SRA Primary Mental Abilities test. The first test situation was used: (1) to establish examiner-examinee rapport, (2) to ensure that all students were able to understand the test instructions spoken or written in English, and (3) to establish as far as possible uniform test-taking conditions. In the second test situation, given within a week of the first meeting, the SRA Primary Mental Abilities tests were administered. Only the scores from these latter tests were used in the study.³

Each boy in the sample was Canadian-born and was attending a school in which English was the predominant language of instruction. In the Jewish schools, a Hebrew studies program was also followed, and in the schools for the French Canadians a strong French studies curriculum was offered.

Where possible, two parallel pools of 40 families were formed within each ethnic group. The purpose of the substitute pool was to provide a set of alternate families which could be used in the study if families from the first pool did not agree to participate. Within these pools, the families were assigned to two categories, one classified as middle class and the other as lower class, with the intention of obtaining 20 middle-class and 20 lower-class families in each pool.⁴ The social-class classification was based on an equally weighted combination of the occupation of the head of the household and a rating of his (or her) education.

³ The mental ability tests were administered in the ordinary classes which the children attended. In an effort to achieve maximum uniformity in the test-taking conditions, all the testing was conducted by the writer.

⁴ The "ethnic families" were located by a visit from the writer and a member of the ethnic group. During the preliminary contact, a time was established when both parents would be home and be willing to participate in a two-hour interview session.

In the case of the southern Italians and Canadian Indians, it was not possible to form completely parallel middle-class pools, and in the case of the Jewish families it was not possible to duplicate the lower-class group. The final sample within each ethnic group contained 37 families, 18 classified as middle class and 19 as lower class. Therefore, the results in the study are based upon 185 ethnic families, 37 coming from each of the five ethnic groups.

RESULTS

Stage I

Ethnic group differences in mental abilities.—It was fundamental to the remainder of the study to determine whether the ethnic groups differed in the profiles and levels of the four mental abilities.

Profiles of mental ability test scores.—The shapes of the profiles of the mental ability test scores for each ethnic group are presented in figure 1. The scores on each of the mental abilities were converted to standard scores, calculated over the total sample of boys, with a mean of 50 and a standard deviation of 10.

The results of a multivariate analysis-of-variance technique, which involved the computation of Wilk's λ criterion, indicated that the ethnic group differences in the profiles of mental ability scores were statistically significant (Rulon and Brooks 1968; Cooley and Lohnes 1962). This result indicated that the ethnic groups occupied different regions of the mental ability space.

The shape of the profile of the mental ability test scores for the Jewish children is similar in shape to the profile obtained by Lesser, Fifer, and Clark (1967) in an examination of six- and seven-year-old Jewish children in New York. It was also found that these Jewish children were characterized by high verbal and number ability scores and lower spatial and reasoning ability scores.

The results of the study by Coleman (1966) of sixth-grade American Indian children are compatible with the findings of the present study regarding the Canadian Indian (Iroquois) children. Coleman also found that the reasoning ability scores of Indian children were higher than the verbal ability scores, which in turn were higher than the number ability scores. In an investigation of children in western Canada by Vernon (1969), it was found that the spatial ability test scores of the Cluny Indian children exceeded their number and verbal ability test scores.

Therefore the study replicated, in part, a number of previous studies which have examined the profiles of mental abilities of different ethnic

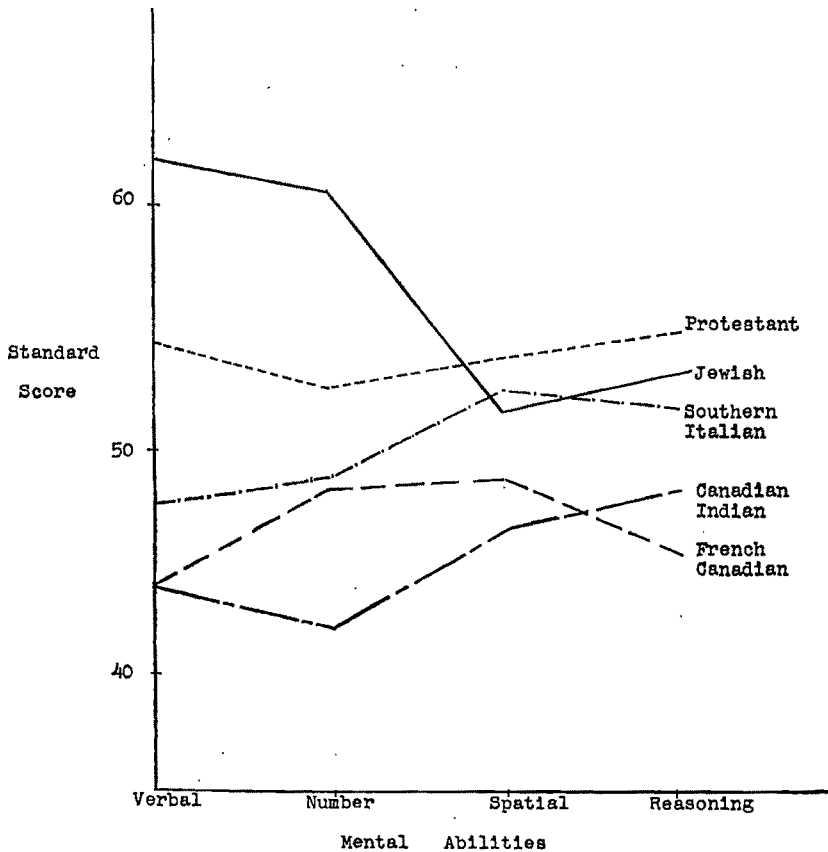


FIG. 1.—Profiles of mental ability test scores for each ethnic group

groups. The replication gives some confidence for the validity of the present results.

Levels of mental ability test scores.— When differences between the ethnic groups in the levels of each of the mental abilities were examined, it was found that: (1) the mean verbal ability score of the Jewish boys was significantly higher than the verbal ability score of the Protestant boys. The verbal ability level of the Jewish and Protestant boys was significantly higher than the verbal ability levels of boys from the other three groups. These latter groups did not differ significantly in their scores. (2) The mean number ability score of the Jewish boys was significantly higher than the mean number ability scores of boys from the other four groups. The Protestant group differed significantly from the French Canadian and Canadian Indian boys, but not from the southern Italian boys. The French Canadian and southern Italian groups, which did not differ significantly from each other, had significantly higher scores than

the Canadian Indians. (3) The mean spatial ability score of the Protestant boys was significantly higher than the mean score of the Canadian Indian and French Canadian boys. The mean score of the southern Italian boys was significantly higher than the mean score of the Indian boys. The Jewish group did not differ significantly from the other groups. (4) The mean reasoning ability scores of the Jewish and Protestant boys were significantly higher than the mean scores for the Canadian Indian and French Canadian groups. The mean score for the southern Italian group was significantly higher than the mean score for the French Canadian group. The results, presented in this first stage of the analysis, indicated that the ethnic groups differed significantly in both the profiles and levels of the mental ability test scores.

Stage II

What is the relationship between ethnic group membership and environmental force scores? Before examining the second research question, the reliability coefficient of each of the environmental scales was estimated by evaluating coefficient α . The reliability coefficients which are presented in table 1 were considered to be of an acceptable level.

TABLE 1
RELIABILITY COEFFICIENTS OF THE ENVIRONMENTAL SCALES

| Scale | Reliability Coefficient | Items (N) | SD of Scores |
|---------------------------------|-------------------------|-----------|--------------|
| Press for achievement | .94 | (50) | 35.18 |
| Press for intellectuality | .88 | (18) | 17.05 |
| Press for activeness | .80 | (25) | 11.29 |
| Press for independence | .71 | (16) | 8.72 |
| Press for English | .93 | (20) | 17.83 |
| Press for ethlanguage | .90 | (15) | 14.4 |
| Father dominance | .67 | (22) | 9.22 |
| Mother dominance | .66 | (22) | 10.33 |

Profiles of environmental forces.—The shapes of the profiles of the environmental force scores for each ethnic group are presented in figure 2. The scores on each of the environmental forces were converted to standard scores, calculated over the total sample of boys, with a mean of 50 and a standard deviation of 10. The results of a multivariate analysis of variance indicated that the group differences in the profiles of the scores were statistically significant, which meant that the ethnic groups occupied different regions in the test space for the environmental forces.

Levels of scores of the environmental forces.—A multiple regression

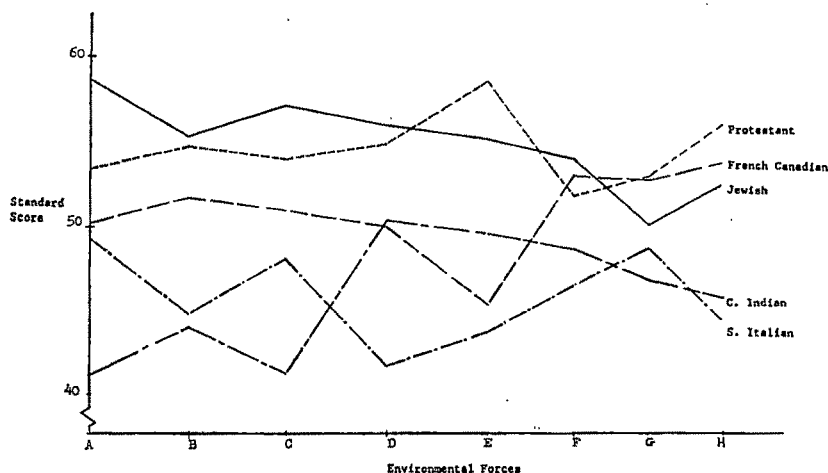


FIG. 2.—Profiles of scores of the environmental forces for each ethnic group. *A* = press for achievement, *B* = press for activeness, *C* = press for intellectuality, *D* = press for independence, *E* = press for English, *F* = mother dominance, *G* = father dominance, *H* = press for ethlanguage.

model was used to examine the relationship between ethnic group membership, and the level of each of the environmental forces. The analysis indicated that except for the parental dominance forces, ethnicity accounted for a moderate to a large percentage of the variance in the environmental force scores. Thus the examination of the first two questions indicated that ethnicity was related to both the levels and profiles of a set of mental ability test scores and to a set of environmental force scores.

Stage III

What is the relationship between the learning environment of the home and the mental ability test scores? The first analysis of this question involved an examination of the zero-order correlations between the scores of the four mental ability tests and the scores of the environmental forces. The zero-order correlations, which are presented in table 2, indicated that the parental dominance forces had either low or negligible relationships with the mental abilities. To investigate the relationship between the two parental forces and the other environmental forces, a principal-component analysis of the eight environmental forces was conducted. The unrotated factor-loading matrix of the interrelations among the forces is presented in table 3. Only those factors with an eigenvalue greater than unity have been included. The third factor had an eigenvalue of 0.65.

The results in table 3 indicated that all of the environmental forces loaded strongly on the first factor, which was labeled the "learning-

TABLE 2

INTERRELATIONSHIPS BETWEEN THE MENTAL ABILITY TEST SCORES
AND SCORES OF THE ENVIRONMENTAL FORCES

| ENVIRONMENTAL FORCE | ABILITY | | | |
|---------------------------------|---------|--------|---------|-----------|
| | Verbal | Number | Spatial | Reasoning |
| Press for achievement | .66** | .66** | .28** | .39** |
| Press for activeness | .52** | .41** | .22** | .26** |
| Press for intellectuality | .61** | .53** | .26** | .31** |
| Press for independence | .42** | .34** | .10 | .23** |
| Press for English | .50** | .27** | .18** | .28** |
| Press for ethlanguage | .35** | .24** | .09 | .19** |
| Father dominance | .16* | .10 | .09 | .11 |
| Mother dominance | .21** | .16* | .04 | .10 |

* $P < .05$.
** $P < .01$.

environment-of-the-home" factor. The second factor, which was characterized by equal but opposite weightings on the parental dominance forces, was labeled the "father-dominance-and-mother-dominance contrast" factor. When interrelationships between scores on the two factors and the mental ability scores were examined, it was found that the scores on the learning-environment-of-the-home factor were significantly related to scores on each of the mental abilities. None of the relationships between the scores on the second factor and the scores on the mental ability tests reached statistical significance.

Because of (1) the exploratory nature of the study in identifying sub-environments for mental abilities and (2) the presence of a general factor, it was decided to use the eight environmental forces as the subenvironment for each mental ability. The relationship between the learning environment

TABLE 3

UNROTATED FACTOR-LOADING MATRIX OF THE EIGHT ENVIRONMENTAL FORCES

| ENVIRONMENTAL FORCE | FACTORS | | h^2 |
|---|---------|-------|-------|
| | I | II | |
| Press for achievement | .83 | .06 | .69 |
| Press for activeness | .90 | .08 | .82 |
| Press for intellectuality | .91 | .01 | .83 |
| Press for independence | .64 | -.25 | .47 |
| Press for English | .76 | -.10 | .58 |
| Press for ethlanguage | .75 | .10 | .57 |
| Father dominance | .41 | .84 | .88 |
| Mother dominance | .40 | .84 | .87 |
| Eigenvalues | 4.196 | 1.533 | ... |
| Percentage of variance accounted for | 52.4 | 19.2 | ... |
| Cumulative percentage of total variance | 52.4 | 71.6 | ... |

of the home and each mental ability was examined by computing the multiple correlation between the eight environmental forces and each mental ability. The results of the analysis, which are presented in table 4, indicated that when the environmental forces were combined into a set of predictors, they accounted for a large percentage of the variance in the verbal and number ability scores, and a low to moderate percentage of the variance in the spatial and reasoning ability scores.

TABLE 4
MULTIPLE CORRELATIONS OF EACH OF THE MENTAL ABILITY SCORES
WITH THE EIGHT ENVIRONMENT FORCES

| Ability | Multiple Correlation | Corrected Multiple Correlation ^a (R_c) | Percentage of Total Variance (R_c^2) |
|-----------------|-------------------------|--|---|
| Verbal | .72*** | .71*** | 50.4*** |
| Number | .72*** | .71*** | 50.4*** |
| Spatial | .32** | .26* | 6.7* |
| Reasoning | .43*** | .40*** | 16.0*** |

^a Corrected to allow for cumulative errors in multiple R , and for small sample size (Nunnally 1967). The correction was applied in all tables involving multiple correlation.

* $P < .05$.
** $P < .01$.
*** $P < .001$.

Thus the investigations of the zero-order and multiple correlations between the environment and mental abilities indicated that, in general, the environmental scales had moderate to high concurrent validity in relation to verbal and number abilities, and low to moderate concurrent validity for spatial and reasoning abilities.

Stage IV

Ethnic group membership, environment, and mental abilities.—To what extent is the learning environment of the home related to ethnic group differences in mental ability scores? A multiple regression model was used to examine the interrelationship among the constructs of ethnicity, environment, and the level of mental abilities. The amount of variance in the mental ability scores that could be attributed to the influence of ethnic group membership was determined, after accounting for the variance that could be attributed to the environment and to the covariation of ethnic group membership and environment. In the regression model the ethnic group membership data formed a set of mutually exclusive categories. The scores on the environmental forces formed a set of predictor variables, and the mental ability test scores formed the criterion vectors.

The results of the investigation, which are presented in table 5, indicated:

TABLE 5

RELATIONSHIP BETWEEN INDEPENDENT INFLUENCE OF ETHNICITY
AND MENTAL ABILITIES

| Criterion | Predictor Variables | Corrected Multiple Correlation (R_c) | Amount of Total Variance (R_c^2) |
|-------------------------|-----------------------------|--|--------------------------------------|
| Verbal ability | Ethnic group membership | .65*** | 45.0*** |
| | A = environment + ethnicity | .78*** | 61.0*** |
| | B = environment | .71*** | 50.0*** |
| | A - B = ethnicity | ... | 11.0*** |
| Number ability | Ethnic group membership | .58*** | 34.0*** |
| | A = environment + ethnicity | .73*** | 53.0*** |
| | B = environment | .71*** | 50*** |
| | A - B = ethnicity | ... | 3.0* |
| Spatial ability | Ethnic group membership | .21* | 4.4* |
| | A = environment + ethnicity | .31 | 10.0 |
| | B = environment | .26* | 6.7* |
| | A - B = ethnicity | ... | N.S. |
| Reasoning ability | Ethnic group membership | .28** | 8.0** |
| | A = environment + ethnicity | .47*** | 22.0*** |
| | B = environment | .40*** | 16.0*** |
| | A - B = ethnicity | ... | 6.0* |

* $P < .05$.** $P < .01$.*** $P < .001$.

(1) For verbal ability, ethnic group membership originally accounted for 45% of the variance in the test scores. After the variance that could be attributed to the environment and to the covariation of the environment and ethnic group membership was allowed for, the "independent" contribution of ethnicity was 11%. (2) For number ability, ethnic group membership originally accounted for 34% of the variance in the scores. The "independent" contribution of ethnicity was 3%. (3) For spatial ability, ethnic group membership did not make a significant "independent" contribution to the variance. (4) For reasoning ability, ethnic group membership originally accounted for 8% of the variance in the scores. The "independent" contribution of ethnicity was 6%.

Thus the results indicated that the variance in the mental ability scores that could be attributed to the influence of ethnicity was reduced after the influence of the environment had been accounted for. However, because of the intercorrelation between ethnicity and environment, the results as presented in table 5, attribute a possible inflated importance to the environment variable. Therefore the contribution of environment to the variation

in the ability scores was calculated after allowing for the contribution of ethnicity and the covariation of ethnicity and environment.

The analysis (table 6) indicates that the contribution of environment was reduced from (1) 50% to 16% in relation to verbal ability, (2) 50% to 19% for number ability, (3) 6.7% to 5.6% for spatial ability, and

TABLE 6
RELATIONSHIP BETWEEN INDEPENDENT INFLUENCE OF
ENVIRONMENT AND MENTAL ABILITIES

| Criterion | Predictor Variables | Corrected Multiple Correlation (R_c) | Amount of Total Variance (R_c^2) |
|-------------------------|-----------------------------|--|--------------------------------------|
| Verbal ability | Environment | .71*** | 50.0*** |
| | A = ethnicity + environment | .78*** | 61.0*** |
| | B = ethnicity | .65*** | 45.0*** |
| | A - B = environment | ... | 16.0*** |
| Number ability | Environment | .71*** | 50.0*** |
| | A = ethnicity + environment | .73*** | 53.0*** |
| | B = ethnicity | .58*** | 34.0*** |
| | A - B = environment | ... | 19.0*** |
| Spatial ability | Environment | .26* | 6.7* |
| | A = ethnicity + environment | .31 ^a | 10.0 ^a |
| | B = ethnicity | .21* | 4.4* |
| | A - B = environment | ... | 5.6* |
| Reasoning ability | Environment | .40*** | 16.0*** |
| | A = ethnicity + environment | .47*** | 22.0*** |
| | B = ethnicity | .28** | 8.0** |
| | A - B = environment | ... | 14.0** |

^a Not significant.

* $P < .05$.

** $P < .01$.

*** $P < .001$.

(4) 16% to 14% for reasoning ability. In order that the relative importance of ethnicity, environment, and the joint contribution of the two variables might be compared, the results from tables 5 and 6 have been summarized in table 7. The results indicated that after accounting for the "independent" influence of the environment, significant ethnic group differences remained in verbal, number, and reasoning ability scores. Only for spatial ability scores were the ethnic group differences absorbed by the environmental differences.

Profiles of mental ability scores.—A multivariate analysis-of-variance technique with multiple covariance adjustments (Rulon and Brooks 1968; Cooley and Lohnes 1962) was used to examine whether the effect of ethnic

TABLE 7
RELATIVE CONTRIBUTION OF ENVIRONMENT AND ETHNICITY
IN RELATION TO THE MENTAL ABILITIES

| CRITERION | VARIANCE (%) ATTRIBUTABLE TO CONTRIBUTION OF: | | |
|-----------------|---|-----------|--|
| | Environment | Ethnicity | Covariation of Environment and Ethnicity |
| Verbal | 16.0 | 11.0 | 34.0 |
| Number | 19.0 | 3.0 | 31.0 |
| Spatial | 5.6 | N.S. | N.S. |
| Reasoning | 14.0 | 6.0 | N.S. |

group membership upon the profiles of mental ability scores would be significantly reduced if the effect of the environmental forces on the profiles of mental ability scores was statistically controlled.

The analysis indicated that, after statistically controlling for the influence of the scores of the eight environmental forces, the ethnic group differences in the profiles of mental ability scores were reduced.

CONCLUSION

The Canadian social structure is characterized by a disproportionate representation of ethnic groups in the various levels of the socioeconomic hierarchy. Census data indicate that the ethnic groups classified as British and Jewish are overrepresented in the upper levels of the hierarchy and underrepresented in the lower levels. The French Canadian and the southern Italian groups exhibit a reverse pattern. The Canadian Indian group is largely concentrated in the lower levels of the socioeconomic hierarchy.

When such differences among ethnic groups persist, it is necessary to determine, as Jensen (1969) suggests, "To what extent can such inequalities be attributed to unfairness in society's multiple selection processes? . . . And to what extent are the inequalities attributable to really relevant selection criteria?" By examining the verbal, number, spatial, and reasoning ability scores of boys from five ethnic groups, this study, in part, investigates the relevance of mental ability performances as selection criteria. It was found that: (1) the ethnic groups differed significantly in both the levels and profiles of a set of mental ability test scores, (2) the ethnic groups differed significantly in both the levels and profiles of a set of environmental force scores, and (3) the environment scores accounted for a large percentage of the variance in the verbal and number scores and a low to moderate percentage of the variance in the spatial and reasoning ability scores.

When the relationships between the environment and the ethnic group differences in mental abilities were examined, it was found that only for spatial ability scores were the ethnic group differences absorbed by the environmental differences. For verbal and number abilities, there was also a considerable proportion of the variance shared between ethnicity and environment.

The present study has gone beyond much of the previous research, which has concentrated on the examination of ethnic group differences in global indicators of both the environment and intellectual ability. Also, from the present study, and from those by Dave (1963), Wolf (1964), Plowden et al. (1967), and Vernon (1969), a common set of environmental forces can be identified which have been shown to account for a large percentage of the variance in different measures of the cognitive performance of children from different cultural settings. This subenvironment contains the forces that press for (1) achievement, (2) activeness, (3) intellectuality, and (4) language.

The ex post facto nature of the study and its correlational design restrict, of course, the inferences which can be made about causation among the variables. The presence of the large joint environment and ethnicity component, in the analysis of the verbal and number abilities, may reflect the finding that children who are members of different ethnic groups have a greater than chance likelihood of being surrounded by distinctive patterns of learning environmental forces, which in turn are related to distinctive ethnic patterns of mental abilities. Such an interpretation raises the question of whether ethnic group profiles of environmental forces might in themselves be relevant selection criteria.

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Social Position and Self-Evaluation: The Relative Importance of Race¹

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Evidence and argument designed to demonstrate that blacks suffer higher levels of personal disorganization than whites has not seriously attempted to assess the relative effects of racial status upon psychological state. Here two samples of blacks and whites are used to evaluate the relative effects of racial status on a measure of self-esteem and on a measure of psychological symptoms of stress. In competition with sexual status, city, age, education, marital status, and work-force participation, racial status has minimal effects upon these measures of affective state employing a multiple classification analysis. City and work-force participation have a greater effect upon whites than upon blacks; marital status has a greater effect upon blacks than upon whites; and education has strong effects upon both racial groups. Caution is therefore suggested for researchers who employ racial differences as an explanation for behavioral differences between racial groups.

In a recent paper in this *Journal*, McCarthy and Yancey (1971) challenged the view that blacks manifest more symptoms of psychological stress and lower self-esteem than do white Americans. In arguing this position, they pointed out that much of the evidence cited to support this view lacked minimal controls for background characteristics thought to be important in comparing blacks and whites on any behavioral or attitudinal dimension. Social psychologists have repeatedly shown these background characteristics to be related to dimensions of psychological well-being. The research reported here evaluates the relative effects of race upon both symptoms of psychological stress and self-esteem (hereafter to be referred to as affective state), while considering other relevant background characteristics. It further evaluates the pattern of relationships between background characteristics and these two dimensions of affective state separately for blacks and whites.

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INTRODUCTION

A major concern of many social scientists, particularly since the beginning of the civil rights movement, has been the explanation of behavioral, and especially achievement, differences between blacks and whites in American society. A dominant perspective among these scholars is that prejudice and discrimination have caused disorganization in the life and personality of blacks. Such disorganization is seen as an important impediment to entering the "mainstream" of American life, particularly the pursuit and achievement of social and economic benefits. Silberman (1964, p. 12) summarizes this view when he says:

The Negro will be unable to take his place in the mainstream of American life until he stops despising himself and his fellows. The Negro will be unable to compete on equal terms until he has been able to purge from his mind all sense of white superiority and black inferiority—until he really believes, with all his being, that he is a free man, and acts accordingly.

Thus, it is the fact of race per se, made salient and operative through personal and group histories of prejudice and discrimination in American life, that is one cause of the putative psychological disorganization of blacks. Low levels of occupational and educational achievement, then, are to be accounted for by the intervening variable of negative self-evaluation.

On the other hand, some authors confound race with its obvious socioeconomic correlates in developing explanations along these lines. In particular, race and socioeconomic status are frequently employed in concert, their independent effects remaining theoretically and empirically inexplicit. Katz's (1968, p. 58) discussion of some of his own research illustrates this problem:

The earlier paper suggested that in *lower-class Negro* homes children's language and problem-solving efforts were not adequately encouraged and reinforced so that children failed to acquire the internal mechanisms that were requisite for autonomous achievement striving—namely, realistic standards of self-evaluation and the capacity for intrinsic mediation of satisfaction through self-approval of successful performance. In *white middle-class* children, on the other hand, internalization of the achievement motive presumably is relatively well advanced at the time of entering school. Therefore, for *disadvantaged* students but not for their more *affluent* age peers, the development of the will to learn should depend heavily upon the behavior of social models in the classroom—that is, upon the extent to which teachers and fellow students exhibit suitable standards of performance and reward individual accomplishment with genuine approval and respect. Moreover, in predominantly *Negro schools* where low attainment levels prevail, most pupils should be largely incapable in the absence of external cues of realistic self-appraisal or intrinsic mediation of achievement satisfaction. [Emphasis added]

It is not clear whether Katz intends to invoke race or class (or some more complex combination of the two) as the principal explanatory concept here. The emphasis seems to be upon socioeconomic status, although the focus in the discussion from which it is drawn is clearly upon race; race and class are correlated in the United States, but they are hardly substitutable constructs. Their empirical correlation makes it all the more important to keep them theoretically and operationally separate.

Apart from the confounding of race and socioeconomic status in such arguments, there is a tendency to ignore other possible determinants of psychological disorganization. This leads in many cases to empirical studies which relate the variable of race to the psychological variables under study, while neglecting to make minimal controls for other variables whose relationship to the dependent variables have been established in other research contexts. Prefatory to presenting our evidence, some of the literature which relates not only race and socioeconomic status but a variety of other background characteristics to affective state is reviewed.

There are two important ways in which social scientists have addressed the problem of the effects social position or social category (normally termed "background characteristics") have upon attitudes, values, and ultimately, self-evaluation. The first perspective focuses upon the different experiences which members of different social groups have and attitudes, values, and self-evaluation that relate directly to these experiences. The second perspective focuses on the relationship between groups. In particular it argues that behavior, attitudes, and self-evaluation stem from the *relative* position of a group and the evaluation accorded it by members of other groups. Cartwright (1950, p. 440) states this view when he says: "The groups to which a person belongs serve as primary determiners of his self-esteem. To a considerable extent, *personal* feelings of worth depend on social evaluation of the groups with which a person is identified. Self-hatred and feelings of worthlessness tend to arise from membership in underprivileged or outcast groups."

Generally, arguments from each perspective are made to predict group differences in self-evaluation for any particular social position, be it age, education, sex, marital status, or race. Evaluating the relative importance of group differences in self-worth, as is the intent here, does not allow the investigator to choose between these perspectives for any single dimension of social position found to be of importance. However, the relative, as well as the absolute, effect of each dimension of social position is of utmost importance in determining whether further research is likely to be fruitful in untangling these two sources of differences in self-worth.

For theoretical and evidential reasons, racial category has been thought to be of great importance in explaining affective state (see McCarthy and Yancey 1971). Exploring literature outside that concerned with blacks,

however, uncovers other dimensions of social position also thought to be important in explaining affective states. The focus upon a single dimension of social position leads to conclusions of statistically significant differences along that social dimension, but rarely focuses upon the relative importance of that dimension in explaining affective state. Literature to be discussed below suggests we should look simultaneously at the effects of the following dimensions of social position on affective state: region (city of residence), sex, marital status, age, social and economic status (education), work-force participation, and race.²

The only other dimension of social position excluded which we would have preferred to include is that of religious affiliation. Rosenberg (1965) has shown this dimension to be related to self-esteem among adolescents. Unfortunately, we do not have information on the religious affiliation of our respondents.

We have chosen these dimensions of social position based upon our reading of literature relating each to affective state. Below we will briefly summarize how that literature related the dimension to affective state. For each (with the exception of marital status), it has been suggested by other researchers and theorists that the relationship between the dimension and affective state should be somewhat different for blacks and whites. Where appropriate, that argument will also be briefly summarized.

SOCIAL POSITION AND AFFECTIVE STATE

Race

Both general arguments have been employed to predict lower feelings of self-worth among blacks³ (see McCarthy and Yancey 1971). It is argued that blacks must adopt a submissive posture by virtue of membership in

² The reader may complain that we have excluded both occupation and income from our set of dimensions. We have done so for what we believe are good reasons, although they are pragmatic, relating to the data at hand, rather than theoretical. The exclusionary rationale will be explained in detail below.

³ We have chosen here to focus upon the adult respondents and are, to some degree, emphasizing the importance of adult life experiences on self-evaluation. This contradicts the emphasis on life-history and childhood experience on the self-esteem of adults which is typical of several studies in the area (Coopersmith 1967; Baughman and Dahlstrom 1968). Some evidence supports our bias that self-evaluation is changeable in adult life rather than being permanently set during childhood. The reaction of unemployed workers seems to support this view where the shift from employment to unemployment was shown to result in seriously decreased feelings of self-worth (Bakke 1940; Zawadski and Lazarsfeld 1935). Similarly, the reactions of "ethnic villagers" to urban renewal and relocation suggest that self-evaluations do change following adult life experiences (Fried 1963). Only longitudinal studies can establish the limits of changeability in affective states and they remain to be carried out systematically.

the nonwhite social category. Submissive behavior is said to result, through a series of postulated psychological processes, in a derogatory view of self. It is also argued that interaction with whites who demean and other blacks who have learned from the dominant whites to demean similarly produces a derogatory view of self. No matter which version of these theoretical processes is posited, the conclusion has predicted greater psychological difficulty and lesser feelings of self-worth on the part of black Americans.

Region of Residence (City)

Past discussion of the relationship between race and affective state tends to be based theoretically and/or empirically upon Southern patterns of race relations. Given that there are regional differences in relations between blacks and whites, particularly in the overtness of discrimination by whites, it follows that blacks living in the South should manifest higher rates of psychosomatic symptoms of stress and lower rates of self-esteem than blacks living in the North.

Regarding whites, some observers, such as Cash (1941), have argued that Southerners feel inferior to their Northern white countrymen. On the other hand, Killian (1970) has recently argued that there are rather strong feelings of regional pride on the part of white Southerners.

Dohrenwend (1966), summarizing a series of studies reporting racial comparisons, suggests that there may be an interaction between race and region in explaining psychological states. He notes that the majority of studies which show whites to exhibit higher rates of disturbance were done with Southern respondents while those showing blacks to have higher rates of disturbance used Northern respondents. Such argument and evidence produces contradictory predictions while they agree in attaching importance to region of residence as an explanatory factor in affective state.

Sexual Status

Even though both sexes have received more education during the last several decades and a greater proportion of females are in the labor force, there has been a persistent decline in the relative status of women since 1940 (Knudsen 1969). In a recent paper, Gove and Tudor (1972) identify several propositions relating these structural changes to the psychological states of men and women. It is argued that the role set of women is more fragile than the role set of men. Housework is less gratifying than it once was. Even when married women work, they are in less satisfactory positions than married men. Finally, it is argued that role prescriptions for women are contradictory.

Evidence suggests that females receive lower evaluations than men by

both women and men (McKee and Sheriffs 1957, 1959). There is also evidence which suggests that American females exhibit higher rates of psychiatric disorder than men (Gurin, Veroff, and Feld 1960; Langner and Michael 1963).

While this argument and evidence suggests that men should have higher self-esteem and lower symptoms of stress than women, some literature suggests that among blacks the very opposite pattern is found. Moynihan (1965, p. 16) argues that the segregation of public facilities was directed more toward the black male than the black female, with a more serious negative effect upon him. Ladner (1971, p. 17-18) argues that slavery acted to "reinforce the subjugation of men to women." She writes: "The impact this had on black men is immeasurable and remnants of this effect can be found today, especially since many of the fundamental conditions are unchanged." This suggests that the expectation of a relationship between sexual status and affective state should be either muted or reversed for blacks.

Marital Status

Since Durkheim (1951) demonstrated the positive effect of marriage on the suicide rate, social scientists have assumed that the rate of psychic difficulties for unmarried adults was greater than for the married. This relationship is normally phrased in terms of the psychic benefits of the close association provided by marriage. Further, since most American adults are married, the statistically deviant category of unmarried is thought to be viewed with some suspicion unless it is easily explainable. The "Gay Liberation" movement has articulated the difficulties encountered by one type of unmarried male, providing at least impressionist evidence of the devaluation of this social status. Gove (1971) summarizes these arguments and their supporting evidence with respect to psychiatric disorder and concludes that the evidence offers qualified support for postulated differences. We have found no evidence or argument suggesting these should be a different relationship between affective state and marital status for blacks.

Chronological Age

A substantial literature suggests that a process of disengagement from society is an inevitable concomitant of aging (Cummings and Henry 1961; Cummings 1963; Henry 1964). Such increases in interpersonal isolation are thought to produce corresponding increases in self-preoccupation and levels of anomie (Shannas 1970, p. 183; Drake 1958; Reichard, Livson, and Peterson 1962).

The empirical evidence relating to the disengagement hypotheses is characterized by some ambiguity in results and tends to be restricted to samples composed only of relatively old respondents. Cavan, Burgess, and Goldhammer (1949) found that increased age was inversely associated with happiness, usefulness, zest, interest in life, and personal adjustment. Kutner et al. (1956) found that increasing age is related to a decline in morale, and more recently Shanas (1970, p. 188-89) found "that with increasing age old people apparently do become increasingly self-preoccupied. These findings substantiate the disengagement theory."

While we know of no argument concerning the relative effect of age upon affective state among blacks and whites, there is some reason to expect that there should be an interaction. The advent of "black is beautiful" as a theme in American black communities may be thought to have some effect upon the self-conception of blacks, especially young blacks. Also, if older blacks were socialized under an institutional system of race relations which resulted in lower self-esteem and higher rates of stress, then we should expect (at least to some degree) to find remnants of generational differences in socialization among our older black respondents. If this is the case, the relationship between age and affective state ought to be stronger among blacks than whites.

Socioeconomic Status

The relationship between affective state and socioeconomic status has been demonstrated in a number of different research settings. In particular, the Midtown Manhattan study whose scale of "psychosomatic symptoms" is utilized below reported a negative relationship—the higher the status, the lower the reported number of symptoms—for an adult's current social status, as well as for status of family of origin (Srole et al. 1962; see also, Phillips 1966; Dohrenwend 1966). Previous reviews of the research on race and affective state indicates that socioeconomic status is a key variable which must be controlled before racial comparisons are made (McCarthy and Yancey 1971).

There are two ways in which socioeconomic status has been related to the affective states of individuals. First, membership in a given social class is said to make it more or less likely that an individual will be exposed to certain life experiences. These experiences are said to directly affect self-evaluation. In particular, membership and socialization in the lower class is said to have negative effects on self-conception (Rainwater 1966; Coopersmith 1967; Banfield 1970). Second, social status has been related to affective state in the relative prestige accorded to various social positions. A concomitant of "high" material placement (regarding education, income, occupation) is the positive evaluation from others which

tends to follow. Similarly, positions of "low" material placement tend to bring relatively negative evaluations from others.

Applying these propositions to predictions concerning affective states for blacks and whites is not altogether straightforward. A number of writers suggest that being black exacerbates the negative effects of lower socioeconomic status (Dollard 1957; Kardiner and Ovesey 1951; Davis and Dollard 1940; Rohrer and Edmondson 1960; Pettigrew 1964; Rainwater 1966). Blacks of lower socioeconomic status, then, are thought to have lower self-esteem and higher levels of symptoms of psychosomatic stress than whites of the same status. Without consistent focus upon blacks of both middle and lower socioeconomic status, it is unclear whether this literature suggests the hypothesis that the relationship between socioeconomic status and affective state should be stronger among blacks than among whites, or that the relationship should be of equal magnitude within each group and that similar differences should be found at each level of socioeconomic status.

In its simplest form, the relative evaluation argument predicts relationships of similar magnitude with consistent differences (between races) at each level of socioeconomic status. McCarthy and Yancey (1971) have pointed out, however, that this simple application of the relative evaluation argument does not take into account the various mechanisms a man may employ to counter negative evaluations of his social group. It was suggested that (1) blacks are more likely than whites to find themselves in lower socioeconomic status categories, thus they are more likely to associate with other blacks in similar positions and to use them, rather than whites, as standards of comparison in developing affective states; (2) lower status blacks are more likely to be members of a distinctive subculture which provides alternative and achievable criteria of success; and (3) blacks at lower socioeconomic status levels are more likely to see their relatively disadvantaged position as a result of "system bias" and, therefore, are less likely than whites of the same status to interpret their position as a reflection of personal worth. If we assume that similar processes are less likely to hold for blacks of higher status, these arguments lead to the expectation that the relationship between socioeconomic status and affective state should be stronger among whites than among blacks.

Work-Force Participation

Even though occupational status is viewed by many social scientists as the key social status (Kahl and Davis 1955; Bierstedt 1963; Blau and Duncan 1967), and especially as a source of self-evaluation and identity (Rainwater 1965), a relatively large proportion of adult Americans possess no occupational position. This is particularly true among the poor, blacks,

women, and aged. Although occupational status may be a strong predictor of affective states for adults in the work force, persons out of the labor force do not have this major source of self-evaluation and identity. In addition to problems which may stem from the absence of occupation as evaluative criteria, unemployment frequently carries negative and stigmatizing consequences for those normally expected to be employed (Zawdski and Lazarsfeld 1935; Goffman 1961). Yet the effect of nonemployment, particularly in its stigmatizing consequences, should vary depending on one's other social positions. Particularly those persons who are not expected to be employed, for example, women and the elderly, should not be affected as strongly as those who are, for example, males and younger adults. With these relevant dimensions of social position controlled, we are led to hypothesize that work-force participation should be positively related to self-esteem and negatively related to psychosomatic symptoms of stress.

The arguments concerning the reinforcing effects of racial status upon socioeconomic status apply to the dimension of work-force participation, but the lack of clarity in prediction of within-group relationships remains. Similarly, the arguments concerning the muting effects of racial status upon self-worth at the lower socioeconomic level also apply here. We are led to predict as we did with socioeconomic status that work-force participation will be more strongly related to affective state for whites than among blacks.

METHODS

Data were generated by means of a questionnaire survey administered to black and white adults living in Nashville, Tennessee and Philadelphia, Pennsylvania during the summer of 1969. A total of 1,179 respondents were interviewed.⁴ Our sample design was dictated by two major considerations. First, we felt it was important to obtain an approximately equal number of whites (602) and blacks (577), since we were primarily interested in racial comparisons. We interviewed 362 blacks and 350 whites

⁴ Given the finding reported by Pettigrew (1964) that black respondents are influenced by the race of the interviewer, we attempted to match race of interviewer with race of respondent. Less than 5% of the white respondents were interviewed by black interviewers, but approximately 35% of the black respondents were interviewed by white interviewers. Fearing that this may have biased our black respondents, we utilized the race of interviewer variable as a predictor upon our measures of affective state (to be discussed below). The race of interviewer was not related to the affective state of our black respondents. We will report elsewhere our supporting finding that a measure of social desirability response (Crowne and Marlowe 1966) was not related to race of interviewer for our black respondents.

in Nashville, and 215 blacks and 252 whites in Philadelphia. Second, given the emphasis on the traditional literature on low socioeconomic status we felt it was important that we obtain (1) a relatively large number of lower-class respondents, (2) enough variation in social status to examine its effect, and (3) similar distributions of socioeconomic status among blacks and whites.

Employing 1960 census information and any information available on subsequent neighborhood change, residential areas in both cities were selected which were thought to hold lower-, working-, and middle-class blacks and whites. Within each residential area, blocks were randomly chosen. Specific dwellings were selected by systematically interviewing in every fifth dwelling unit. Within each dwelling unit, we attempted to interview the head of the household, but interviewed some second adult when it became clear that the household head was unavailable. The non-response rate, given three call-backs, was under 5% in each city. In both cities, we obtained the anticipated overrepresentation of lower- and working-class respondents and of black respondents.

Our interview schedule contained two measures of affective state—the 22-item scale of psychosomatic symptoms of stress (Langner 1962) and the self-esteem scale developed by Rosenberg (1965). Both of these scales refer to global characteristics. The Langner scale is designed to give a global psychiatric rating, and is normally scored “well” (three or fewer reported symptoms) or “impaired” (four or more reported symptoms). The results reported below are based upon this dichotomous scoring of the scale results, but the analysis was also run using the total scale score. The results are substantially the same using either the dichotomous scoring or the total scale score, but we elected to report the former since it corresponds more closely to other reports using the scale. The distribution of scores obtained in this sample was slightly higher than those reported in other studies, but given the representative nature of earlier samples compared with the disproportionate weighting toward the lower end of the socioeconomic dimension in this sample, such a result was expected. Table 1 shows the distribution of responses in this sample compared with the distribution of responses reported by Langner (1962).

Rosenberg's self-esteem scale is designed to measure an evaluative attitude toward the self as a total object. This scale is presented in a Likert format with no neutral category. Strongly positive responses were scored 4 and strongly negative responses were scored 1, with appropriate reserve scoring. This yielded a possible range of 10–40. The mean score was 29.82 with a standard deviation of 4.23. The zero-order correlation (η or r_{pbi}) between self-esteem and psychosomatic symptoms of stress is $-.269$ for this sample.

TABLE 1
 TWENTY-TWO ITEM SCREENING SCORE OF PSYCHOSOMATIC
 SYMPTOMS: MIDTOWN TOTAL (LANGNER 1962) AND
 PRESENT STUDY

| Screening Score | Midtown (%) | Nashville- Philadelphia (%) |
|---|----------------|-----------------------------------|
| 0 | 21.7 | 26.7 |
| 1 | 20.2 | 17.6 |
| 2 | 14.3 | 13.7 |
| 3 | 12.6 | 10.2 |
| 4 | 8.6 | 8.0 |
| 5 | 7.0 | 5.5 |
| 6 | 4.4 | 3.1 |
| 7 | 3.3 | 3.7 |
| 8 | 2.7 | 2.4 |
| 9 | 1.6 | 2.4 |
| 10 | 1.7 | 2.2 |
| 11 | 0.5 | 1.0 |
| 12 | 0.7 | 1.2 |
| 13 | 0.4 | 1.0 |
| 14 | 0.1 | 0.34 |
| 15 | 0.05 | 0.34 |
| 16 | 0.05 | 0.21 |
| 17 | 0.05 | 0.24 |
| 18 | 0.05 | 0.24 |
| 19 | 0.00 | 0.08 |
| 20-21 | 0.00 | 0.00 |
| Total | 100.00 | 100.15 |
| Percentage with scores of four or more | 31.2 | 31.8 |
| Average score | 2.83 | 3.04 |

Multiple-Classification Analysis

We believe that past work attempting to evaluate the effects of race upon affective state has not taken relevant dimensions of social position into account. We have chosen to use multiple-classification analysis (MCA) (Sonquist 1969; Andrews, Morgan, and Sonquist 1969) as an efficient statistical tool in our task of exploring the relationship between race and affective state with appropriate controls. Primarily this is because "MCA [multiple classification analysis] looks at predictors *simultaneously* and adjusts *each* predictor to take some account of its relationship with other predictor(s)" (Bachman 1970, p. 65). Since a number of dimensions of social position have been hypothesized to influence affective state along with racial category, we needed a technique which would assess relative effects; MCA is appropriate for nominal predictor variables such as blacks and whites as well.

Bachman (1970) has said that "MCA assumes that the effects of pre-

dictor variables are combined *additively*; that is, it assumes that there is no interaction among predictors." Other than racial interactions, our review of the relevant literature did not suggest theoretical reasons for expecting interactions among our predictor variables. The only appreciable interaction uncovered in these data was between race and city of residence in predicting self-esteem. We have dealt with this by partitioning the data by city and race in the analysis that follows.

Predictor Variables

The variables of city (Nashville or Philadelphia), sex, and race are self-explanatory. Marital status was scored married, never married, divorced, separated, or widowed. Work-force participation was scored as participating (working now either full time or part time) and nonparticipating. Education was categorized as 0-2 years of school, 3-7, 8, 9-11, 12, some college, and college graduate. Age was scored by decades—20-29 and so on. Occupation and income were ignored in the analysis for three reasons: (1) there was appreciable nonresponse for this sample on each dimension; (2) work-force participation and education were thought to be adequate indicators of socioeconomic status since the addition of either income or occupational position (including the nonresponse categories) did not appreciably increase the variance explained in the criterion variables; and (3) the relationship between the measures of affective state and occupational status and family income is similar to their relationship to educational status.

RESULTS

Tables 2 and 3 present the independent effects of six dimensions of social position on our two measures of affective state. We digress briefly to give some explanation of the figures presented in these and subsequent tables to help the reader interpret these results. The basic output from the multiple-classification analysis is, for each category of each predictor, an adjusted (for the other predictors) deviation of the mean value of the dependent variable from the grand mean of the dependent variable for the total sample. (These are, in effect, dummy variable regression coefficients.) The β 's given in these tables are the square root of the weighted average of the squares of these deviations divided by the standard deviation of the dependent variable. Thus β for any given predictor is a summary measure of its effects on the dependent variable adjusted for the effects of the five other predictors. The β 's cannot be given the usual standardized regression coefficient interpretation of the number of standard deviations change in the dependent variable per standard deviation change in the independent

TABLE 2

INDEPENDENT EFFECTS OF SIX DIMENSIONS OF SOCIAL POSITION ON PSYCHOSOMATIC SYMPTOMS OF STRESS, BY RACE, FOR NASHVILLE AND PHILADELPHIA RESPONDENTS^a

| DIMENSIONS OF SOCIAL POSITION | NASHVILLE | | | PHILADELPHIA | | |
|---|-----------|--------------------------------|-------|--------------|--------------------------------|-------|
| | β^b | Variance Added ^c | p^d | β^b | Variance Added ^c | p^d |
| Marital status (5) | .080 | ... | N.S. | .121 | .001 | N.S. |
| Work-force participation (2) | .201 | .022 | .001 | .235 | .029 | .001 |
| Sex (2) | .069 | ... | N.S. | .186 | .015 | .001 |
| Race (1) | .113 | .011 | .001 | .044 | ... | N.S. |
| Age (9) | .176 | .012 | .05 | .131 | ... | N.S. |
| Education (9) | .291 | .057 | .001 | .181 | .013 | N.S. |

NOTE.—Numbers in parentheses are degrees of freedom used by including this variable as a predictor. All predictors except race include "no information" category.

^a The proportion of total variance (reduced for degrees of freedom used in prediction) explained by six predictors is: Nashville = .146, and Philadelphia = .124.

^b The β coefficient is the standardized regression coefficient. It is a measure of the relative importance of predictor variables in this sample. See Andrews, Morgan, and Sonquist (1969: 1117-122).

^c "Variance Added" is determined by taking the difference between "shrunk" R^2 for the regression equation with all five predictors and the "shrunk" R^2 for the regression incorporating all the dimensions except the one whose impact is being assessed.

^d Probability value is from the F -ratio to test the statistical significance of (unshrunk) variance added by the variable whose impact is being assessed.

variable (since the predictors are categorical and have no meaningful standard deviation). They are, however, one appropriate measure of the relative (among the six predictors) importance of the predictors. (See Andrews et al. 1969, pp. 117-22).

The other set of figures given in these tables is labeled "Variance Added." This measure is the percentage point increase in total variance explained in the dependent variable by adding the particular independent

TABLE 3

INDEPENDENT EFFECTS OF SIX DIMENSIONS OF SOCIAL POSITION ON SELF-ESTEEM, SEPARATELY FOR NASHVILLE AND PHILADELPHIA RESPONDENTS BY RACE^a

| DIMENSIONS OF SOCIAL POSITION | NASHVILLE | | | PHILADELPHIA | | |
|----------------------------------|-----------|--------------------------------|-------|--------------|--------------------------------|-------|
| | β^b | Variance Added ^c | p^d | β^b | Variance Added ^c | p^d |
| Marital status | .139 | .010 | .05 | .148 | .009 | N.S. |
| Work-force participation . | .094 | ... | N.S. | .180 | .017 | .01 |
| Sex | .067 | .002 | N.S. | .079 | ... | N.S. |
| Race | .143 | .018 | .001 | .098 | .007 | .05 |
| Age | .154 | .012 | .05 | .261 | .032 | .01 |
| Education | .304 | .062 | .001 | .399 | .095 | .001 |

^a The proportion of total variance (reduced for degrees of freedom used in prediction) explained by six predictors is: Nashville = .147, Philadelphia = .183.

^b See ^b of table 2.

^c See ^c of table 2.

^d See ^d of table 2.

variable to a prediction equation containing the other five independent variables. "Variance Added," then, is the amount of variance in the dependent variable explained by the particular independent variable *over and above* that explained by all the other independent variables in that analysis. The "Variance Added" measure we report is computed from the squared multiple correlation coefficient corrected for degrees of freedom (see Blau and Duncan 1967, pp. 132-40, for a discussion of alternative strategies for partitioning variance using multiple-classification analysis).

We have not shown in these tables the actual adjusted deviations for each category of each predictor from the grand mean of the dependent variable. To have presented the data in such detail would have taken an inordinate amount of space. In order to give sufficient substantive detail for the interpretation of these results, we describe in text the categories of the predictor variables for which the adjusted means on the dependent variable were above the grand mean.

In table 2, the β 's suggest that, for the Nashville sample, educational attainment and work-force participation are the most important independent variables in predicting levels of psychosomatic symptoms of stress and that sex and marital status are the least important. The "Variance Added" measure supports this purely statistical result. That is, education and labor-force participation contribute the most unique variance when added to the other predictors, while neither sex nor marital status contributes a significant amount of variance when added to the other predictors.

At this point, we want to focus the reader's attention on the effects of race on the two measures of affective state. It is our intention to discuss only these effects for tables 2 and 3, since the pattern of results for race will structure how we present and discuss data in the rest of the paper.

It can be seen in these tables that, although the increment to explained variance due to race is small, its effects on self-esteem are significant for both Nashville and Philadelphia respondents. Further, its effects on levels of symptoms of stress are significant for Nashville. Not shown in these tables, however, is that in both cases of significant effects the impact of race is *opposite* that predicted by the traditional arguments which posit that blacks have higher levels of psychological disorganization than whites. In addition, the effect of race on self-esteem for Philadelphia respondents, although not significant, favors whites. The adjusted mean self-esteem values are as follows: Philadelphia whites = 31.32, Philadelphia blacks = 30.33, Nashville whites = 28.64, Nashville blacks = 29.62. Thus, it can be seen that there is an interaction involving city, race, and self-esteem; the effects of race on self-esteem are different for Nashville respondents than for Philadelphia respondents. Further, a comparison of the sets of adjusted means reveals that there is a "city" effect, in the sense that

Philadelphia respondents have higher average self-esteem than Nashville respondents.⁵ It may be noted that these results support Dohrenwend's (1966) observation that region (in our case city) may structure the character of relations between the races so that the impact of race on psychological state (at least on self-esteem) will be different for different settings. This appears to be true in our comparison of findings in Nashville and Philadelphia.

The results for symptoms of stress are not so complex as those for self-esteem, but they do merit some discussion relative to the traditional arguments that blacks incur higher levels of psychological disorganization than whites. For both Nashville and Philadelphia respondents, blacks have *lower* levels of psychosomatic symptoms of stress than whites (although the differences are not statistically significant for Philadelphia). The adjusted mean scores are: Philadelphia whites = 1.29, Philadelphia blacks = 1.16, Nashville whites = 1.41, Nashville blacks = 1.30. (The reader is reminded that the higher the score, the higher the symptom rate.) Again there is an additive region (or city) effect favoring the Philadelphia respondents.

Given the interaction involving the effects of race on self-esteem by city, we are led to the strategy of presenting the rest of the data separately for blacks and whites for each city. It is not meaningful to try to summarize the effects of race with a single parameter when its effects differ by city.

Looking first at the effects of marital status on the two measures of affective state in tables 4 and 5, we can see that their magnitudes are consistently low. In only one of the eight analyses ($2 \text{ cities} \times 2 \text{ races} \times 2 \text{ dependent variables}$)—the effects of marital status on self-esteem for Nashville Negroes—is the amount of variance added significant. Nevertheless, a consistent pattern is revealed in the adjusted mean scores for the dependent variables within categories of the marital status predictor. The pattern is that currently married respondents have higher levels of self-esteem and lower rates of symptoms of stress than other respondents. There is a tendency for the effects of marital status to be greater for blacks than for whites. This holds true in each instance, except for the effects of marital status on symptoms of stress among Philadelphia respondents.

Our expectations regarding the pattern of results for sexual status were

⁵ It has recently been shown (Schuman and Gruenberg 1970) that city of residence has an important impact upon racial, urban, and other attitudes of both blacks and whites, after the effects of other background characteristics are taken into account. Although the attitude studies by Schuman and Gruenberg have no necessary theoretical relationship upon the affective state under investigation here, their findings are striking since they ignored southern cities in their analysis. Though we have interpreted the city variable primarily in terms of its regional component, Schuman and Gruenberg's findings suggest caution in such an interpretation since they uncovered effects of similar magnitude for cities within regions.

TABLE 4

INDEPENDENT EFFECTS OF FIVE DIMENSIONS OF SOCIAL POSITION ON PSYCHOSOMATIC SYMPTOMS OF STRESS, BY RACE, FOR NASHVILLE AND PHILADELPHIA RESPONDENTS^a

| DIMENSIONS OF SOCIAL POSITION | WHITE RESPONDENTS | | | BLACK RESPONDENTS | | |
|----------------------------------|-------------------|--------------------------------|-------|-------------------|--------------------------------|-------|
| | β^b | Variance Added ^c | P^d | β^b | Variance Added ^c | P^d |
| Nashville: | | | | | | |
| Marital status | .054 | ... | N.S. | .170 | .007 | N.S. |
| Work-force participation | .249 | .028 | .01 | .159 | .012 | .05 |
| Sex | .039 | ... | N.S. | .096 | ... | N.S. |
| Age | .236 | .011 | N.S. | .206 | .025 | .05 |
| Education | .311 | .049 | N.S. | .276 | .038 | .01 |
| Philadelphia: | | | | | | |
| Marital status | .173 | .010 | N.S. | .127 | ... | N.S. |
| Work-force participation | .376 | .070 | .001 | .109 | ... | N.S. |
| Sex | .171 | .007 | N.S. | .218 | .020 | .05 |
| Age | .246 | .012 | N.S. | .159 | ... | N.S. |
| Education | .252 | .028 | N.S. | .189 | ... | N.S. |

^a The proportion of total variance (reduced for degrees of freedom used in prediction) explained by five predictors is: Nashville whites = .103, Nashville blacks = .175, Philadelphia whites = .179, Philadelphia blacks = .060.

^b See ^b in table 2.

^c See ^c in table 2.

^d See ^d in table 2.

TABLE 5

INDEPENDENT EFFECTS OF FIVE DIMENSIONS OF SOCIAL POSITION ON SELF-ESTEEM SEPARATELY FOR NASHVILLE AND PHILADELPHIA RESPONDENTS BY RACE^a

| DIMENSIONS OF SOCIAL POSITION | WHITE RESPONDENTS | | | BLACK RESPONDENTS | | |
|----------------------------------|-------------------|--------------------------------|-------|-------------------|--------------------------------|-------|
| | β^b | Variance Added ^c | P^d | β^b | Variance Added ^c | P^d |
| Nashville: | | | | | | |
| Marital status | .078 | ... | N.S. | .201 | .020 | .05 |
| Work-force participation | .251 | .022 | .05 | .050 | ... | N.S. |
| Sex | .023 | ... | N.S. | .083 | .002 | N.S. |
| Age | .180 | .003 | N.S. | .207 | .021 | .05 |
| Education | .249 | .026 | .05 | .347 | .078 | .001 |
| Philadelphia: | | | | | | |
| Marital status | .144 | .002 | N.S. | .189 | .016 | N.S. |
| Work-force participation | .245 | .027 | .05 | .121 | .002 | N.S. |
| Sex | .093 | ... | N.S. | .113 | .005 | N.S. |
| Age | .386 | .064 | .01 | .216 | .007 | N.S. |
| Education | .463 | .123 | .001 | .490 | .148 | .001 |

^a The proportion of total variance (reduced for degrees of freedom used in prediction) explained by five predictors is: Nashville whites = .074, Nashville blacks = .177; Philadelphia whites = .147, Philadelphia blacks = .238.

^b See ^b of table 2.

^c See ^c of table 2.

^d See ^d of table 2.

that (1) among whites, females should have higher levels of psychosomatic symptoms of stress and lower levels of self-esteem; and (2) among blacks this pattern should be muted or reversed. The most consistent outcome is that sexual status has no statistically significant impact on affective state. Only in one case—Philadelphia Negroes for the effects of sex on symptoms of stress—are the results statistically significant. On the other hand, there are some interesting, if complex patterns among the adjusted means. Contrary to our expectations, females tend to have higher (although not significantly so) self-esteem than males. Males tend to have lower rates of symptoms. Thus, sex is very slightly related in the opposite direction to these two aspects of affective state. Evidently, females do not translate any feelings of opposition they may feel as women into negative self-regard, at least if one takes into account other relevant dimensions of social position. They may very well impute blame to the "system" rather than internalizing dissatisfactions as personal inadequacy. That they nevertheless report higher levels of symptoms of stress may reflect greater role isolation and feelings of dissatisfaction with and/or lack of gratification from their roles. Another unexpected finding is a consistent pattern for the β 's representing the effects of sex on affective state for black respondents to be higher than those for white respondents in the same city.

We argued earlier that older people experience disengagement from many important life functions (e.g., work) and experience increasing levels of personal disorganization. Our argument would suggest that particularly people beyond retirement age should experience lower levels of self-esteem and higher rates of symptoms of stress. Further, we suggested that because of generational differences among blacks in experience with and involvement in the civil rights revolution since the late 1950s, the effects of age on affective state for blacks should be greater than those for whites. The empirical results for the effects of age on affective state do not support such a simple conception of their nature. In only three of eight instances does age add a significant amount of variance to the explanation of affective state. Further, the pattern of adjusted means is such that, in most instances, age does not bear a monotonic relationship to affective state. There is a tendency for middle-aged people, as compared with other respondents, to have higher self-esteem and lower rates of symptoms of stress, although sometimes even those in their sixties and seventies had higher than average (adjusting for the effects of other variables) self-esteem or lower than average symptoms rates. There is no tendency for the effects of age to be stronger for black respondents than for white respondents. This latter point contributes further understanding to our earlier conclusion that race has no consistent direct impact on affective state. These results seriously question the argument that similar

affective states among blacks and whites can be attributed to increased feelings of "racial pride" among young blacks.

Finally, we turn to the relationships of our indicators of socioeconomic status with affective state. We suggested earlier that participation in the work force should have its maximal impact on affective state with the other dimensions of social position controlled. That is, participation is likely to affect psychological organization most for those who by society's norms "should" be working. Further, we suggested that because unemployed blacks or those employed in low-level jobs are more likely than similar whites to be involved in a subculture supporting alternative and achievable models of success, the effects of work-force participation may be lower for blacks than for whites. Both these expectations are supported in our data. In seven of eight comparisons, employed people are more favorably situated than unemployed ones, as indicated by adjusted mean scores on the two measures of affective state. Only in the case of the effects of work-force participation on self-esteem for Nashville blacks does this pattern not hold. (The β in this case is virtually zero.) Finally, in each within-city comparison of between-race results, the effects of work-force participation on affective state for whites is about twice that for blacks. Further, the effects are statistically significant in all four cases for whites, only for symptoms of stress, among Nashville blacks. (Note, however, that the comparison here is between employed versus unemployed *and* retired people. In fact, most of the latter category consists of retired respondents.)

In our earlier discussion of the dimensions of social position, we suggested that the effects of education should be similar to those of work-force participation. In particular, it was suggested that education should be directly related to self-esteem and inversely related to symptoms of stress. In addition, we were led to predict that the effects would be stronger for whites than for blacks. The former expectation is supported in our data, the latter is not. Of the dimensions of social position we have incorporated into these analyses, education most often has the strongest effects (in terms of the magnitudes of the β 's) and most often adds statistically significant variance to the explanation of affective state. In general, the higher levels of education are associated with higher average self-esteem and lower average rates of reported symptoms of stress. For most of the analyses, the critical level of education is attainment of some high-school education. That is, those with at least some high-school education (or higher levels of education) had higher than average levels of self-esteem and lower rates of reported symptoms than those with no high-school experience at all.

There is no clear tendency for the effects of education to be stronger for whites than for Negroes. The pattern for self-esteem is opposite to

this expectation—the effects are stronger for blacks. The effects of education on psychosomatic symptoms are slightly stronger for whites. Overall, given these results in conjunction with the results for work-force participation (the effects were consistently stronger for whites than for blacks), we are reluctant to conclude that socioeconomic status has greater impact on the psychological state of mind of whites than blacks.

CONCLUSIONS

We are drawn to three general conclusions. First, in spite of the widespread belief that being black has negative effects upon affective state, we find *no* evidence of a systematic effect of race when other relevant dimensions of social position are taken into account. But our evidence is even more damaging to popular belief: we find that in Nashville (the south), blacks have higher levels of self-esteem and lower rates of reported symptoms of stress than whites. We conclude that there is no systematic effect of race. Our evidence does support the notion that there may be racial differences (even when other relevant variables are controlled) that can be associated with specified contexts or settings within which relations between the races take place. The nature of such settings, their relevant characteristics, and the details of how the interactions between the races affect the psychological organization of the relevant populations all remain to be specified.

Second, beyond the strong effects of educational attainment upon both black and white affective state, there are for this sample several variations in the pattern of relationships between affective state and social position within racial groupings. City of residence appears to have a strong effect upon affective state for our respondents, although we cannot determine whether this is the result of differences between the two cities sampled beyond region or the effect of regional location. The effects of work-force participation upon affective state are greater for our white respondents than for the black, and the effects of being married are greater for the black respondents than for the white. These data, then, suggest that although racial group rates for these measures of affective state are similar, there may be group differences in the genesis of such states.⁶

Finally, the inadequacy of the set of dimensions of social position in explaining variance in the measures of affective state is revealing. A little more than 16% of the variance in self-esteem and a little more than 13%

⁶ It should be pointed out that the adjusted means from the MCA analysis show no difference between unmarried black and white respondents on our measures of affective state. The effects of marital status are solely due to lower self-esteem and high symptoms of stress for the married white respondents compared with the married black respondents.

of the variance in symptoms of stress is explained by the seven dimensions of social position. Although this is impressive compared with much social science research (see Phillips 1971), it suggests that interpersonal processes beyond those linked to social position may be of major importance in understanding the affective states under investigation here. The past research strategy of utilizing significant percentage differences with minimal controls for other dimensions of social position has implied a greater role of each of these dimensions of social position in "understanding" affective states than we believe is suggested by the analysis presented here.

The evidence presented suggests caution in explaining behavioral differences between blacks and whites with intervening psychological constructs related to self-worth. If this evidence is supported by data from other community and national samples, then explanation of group differences in behavior by such constructs becomes statistically untenable no matter how theoretically plausible it may be. The accumulation of additional evidence along these lines should make it increasingly difficult for theorists to appeal to unmeasured constructs such as "feelings of self-worth" as a line of argument to explain presumably otherwise inexplicable behavioral results.

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Self-Evaluations of Blacks and Whites¹

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Hypotheses put forth by McCarthy and Yancey are revised to suggest that the relationship between the self-evaluations of whites and blacks vary according to the trait. For some traits, such as performance of family roles, the self-ratings of blacks are at least equal to those of whites. For other traits, such as trustworthiness, whites rate themselves higher. It is suggested that these patterns are due to variations in (1) the significant others used, (2) the likelihood of using subcultural standards, and (3) the "availability" of a system-blame explanation.

For many years one of the most firmly entrenched sociological truisms was that blacks have lower self-esteem than whites. The idea seems almost unassailable. For one thing, it appeared so reasonable on theoretical grounds, so easy to explain. The general societal evaluation of blacks was negative, individual blacks received many negative evaluations from significant others, the accomplishments of the average black were doomed to be lower because of the barriers he faced, the black was forced to play a subservient role, and the general conditions of his life constantly told him of his inferiority. Here there was something for everyone. All theoretical roads led to the lower self-esteem hypothesis.

Furthermore, there was strong empirical evidence to support the hypothesis, or so it seemed, after constant repetition. There were Kardiner and Ovesey (1951), the famous doll play studies (Clark and Clark 1958; Goodman 1964), and the evidence of everyday observation—hair straightening, skin lightening, etc., in the black community. (See also Dreger and Miller [1968] and Proshansky and Newton [1968] for reviews of less well-known studies.)

All this notwithstanding, McCarthy and Yancey (1971) in a recent article in this *Journal* cast a rather jaundiced eye on the idea. They sug-

¹ This study is based upon two data sets obtained from the National Opinion Research Center. One set is from research conducted by Robert L. Crain under a contract with the U.S. Civil Rights Commission. The other is NORC Amalgam 889A. The authors wish to express their appreciation to Dr. Crain and Patrick Bova for their aid in obtaining these materials. Thanks are also due C. Edward Noll for advice and assistance. The data analysis utilized the computer of the University of Connecticut Computer Center, which is partially supported by National Science Foundation grant GJ-9. A grant to the senior author from the University of Connecticut Research Foundation is gratefully acknowledged.

gested, first of all, that the evidence is really very weak. Kardiner and Ovesey's study is based on 25 persons, and there is no formal control group. Moreover, independent observers reading the case studies concluded that few of the cases show self-hatred. The other empirical props are also clearly weak; they involve small and limited samples in some cases and usually require large jumps from the actual data to the conclusions drawn. Moreover, several studies of young people (e.g., Baughman and Dahlstrom 1968; Rosenberg 1965; Coleman et al. 1966; McDill, Meyers, and Rigsby 1966; Baughman 1971; Bachman 1970; Hartnagel 1970) have come up with findings directly opposed to the idea.

McCarthy and Yancey (1971) also suggest that the idea of lower self-esteem among blacks is not "theoretically inevitable." In fact, they derive alternative hypotheses through a slightly different application of well-established theoretical postulates. Essentially they contend that it is wrong to assume that blacks use whites as significant others. To the contrary, they suggest that the evaluations of blacks are much more relevant. Since evaluations received by blacks from blacks resemble evaluations that whites give whites, one of the assumed causes of lowered self-esteem disappears. Furthermore, McCarthy and Yancey suggested that the criteria of worth used by blacks may be achievable subcultural ones, not necessarily those of the dominant society, and thus another potential source of low self-evaluation is removed. Finally, it is noted that the situation of blacks in this country makes possible a "system-blame reinterpretation of failure" (p. 668), which should further insulate against poor self-esteem. Though these last points might seem to imply that blacks will have higher self-evaluations than whites, McCarthy and Yancey's first hypothesis states: "When social class is controlled Negroes and whites should not differ in levels of self-esteem" (p. 665).

They go on to suggest that their argument has greater applicability to comparisons between lower-class blacks and whites than it does to comparisons within the middle class. The middle-class black is less likely to be able to use the "self-saving" devices discussed above. This leads McCarthy and Yancey to their second hypothesis, that "lower-class blacks will manifest higher self-esteem than lower-class whites, and middle-class blacks will manifest lower self-esteem than middle-class whites" (p. 666).

McCarthy and Yancey do not test their ideas in their 1971 article (but see p. 338). The purpose of this article is to provide an empirical test of hypotheses suggested by their work.

First some changes must be introduced. They speak largely of self-esteem as a general trait. They do not treat the general feeling of worth as a weighted sum of a series of specific self-evaluations, as evaluations of one's standing on a variety of specific traits. We suggest that such a conceptualization is necessary, for the availability of the mechanisms they

discuss probably varies depending upon the trait involved.² On some traits the opinions of whites, for example, might be quite relevant. Our task, therefore, is to consider which traits fit the situation McCarthy and Yancey describe and which do not. It will then be possible to set forth more specific hypotheses.

We believe that the conditions set by McCarthy and Yancey are most likely to occur for those traits which are (1) of little concern to the whites with whom a black interacts; (2) relatively irrelevant for success in the larger society, and (3) thought to be influenced by general social factors. Under these conditions, the evaluations received will be primarily from other blacks and they will not differ from those received by comparable whites.³ Also, when achievements are measured against internalized standards, those standards are more likely to be achievable ones than unrealistic ones derived from the values of the dominant society. Furthermore, for such traits a system-blame explanation is easily available as a means of shifting responsibility.

In general, we have in mind traits that relate to intimate interaction and primary group activities. Such matters may be influenced by external forces, but they are "none of the business of" the larger society. Of the traits available to us, self-evaluations of performance in the child, spouse, and parent statuses seem to fall clearly into this category, as do rating of ability as a conversationalist. Attractiveness to the opposite sex and athletic ability meet the first two criteria. We suggest that McCarthy and Yancey's argument holds for all six of these traits.

In stating this we have not weakened McCarthy and Yancey's basis for the prediction of a statistical interaction with SES. Most of what we have said is clearly *more* true of lower-status blacks. Even on these traits, white evaluations and standards are probably important to higher-status blacks. However, we hesitate to accept the notion that higher SES blacks will have lower self-evaluations than comparable whites. Higher-status blacks do have access to the mechanisms we have discussed, even if it is less than that of lower-status people. Furthermore, the evaluations they receive from blacks may be somewhat inflated. We would tentatively assume that the compensations are sufficient to balance the biased views of whites. Thus we predict: on these traits lower-status blacks rate themselves higher than lower-status whites, but there is no difference in the ratings of higher-status groups.

² McCarthy and Yancey also suggest that there is variation in the applicability of these notions, but they tie it to differences in such things as social class, region, and size of black population, with an emphasis on the first.

³ Rainwater (1966) suggests that in the lower class both whites and blacks are victimized by their peers. We assume that they get a "fair shake" from those close to them. Both assumptions lead to the same prediction in this case, however.

At the same time, the evaluation of some traits may be importantly affected by persons outside the black community. These are traits which are subject to frequent evaluation by whites, and those less subject to the development of subcultural norms. They are those in instrumental areas in which whites have important control over the black man's fate and can therefore impose his norms. Thus, for those traits that operate in the worlds of school and work the opinions of whites are going to weigh heavily for most blacks.⁴ Furthermore, we can expect that evaluations in these areas are going to be relatively low compared with those received by whites. In part this is due to bias, and in part it may be due to a "trained incapacity" to meet the dominant society's "measuring rod." Even if the status-blame explanation is utilized, and we believe it is not easy to use it on some of these traits, the burden is not likely to be overcome. Also, this should be true regardless of SES.

In our data, the relevant traits are self-evaluation of willingness to work hard, trustworthiness, intelligence, and mechanical ability. On these traits we would suggest that McCarthy and Yancey's hypotheses would not gain support. Rather, the more traditional notion should hold. Thus we would predict that blacks have lower self-evaluation than whites on these traits, in the total sample and in each SES group.

In summary, then, for variables such as performance of the spouse role, we predict that lower-status blacks will have a higher self-evaluation than do lower-status whites and that there will be no difference in the higher SES. For variables such as intelligence we predict that whites will show consistently higher self-evaluations, in each SES group and for the total sample with SES controlled.

METHOD

The data to test the hypotheses come from two large-scale surveys conducted by NORC in 1966. The data for the black subjects come largely from Crain's study of the long-range effects of school integration (Crain 1970, 1971; Crain and Weisman, in press). The sample used is a block quota sample of 1,651 black males and females aged 21-45, living in the metropolitan areas of the North. The sample was selected to overrepresent persons in higher income areas and in the smaller metropolitan areas. Therefore, the data are weighted to reflect the actual population. This increases the N in the weighted sample to 4,153. Thus, the true number of cases in the black sample is about 40% of the N 's reported here.

⁴ Even here we do not suggest that the evaluations of blacks are irrelevant. The point is, merely, that on these traits white evaluations and expectations become of considerable importance.

As is typical in samples of this type, lower-status persons are somewhat underrepresented. However, the distribution on education and occupation in the sample is quite close to that of the 1960 Census. Crain and Weisman (in press) report the largest difference they found was 12%, and they believe that this discrepancy is, in part, a reflection of true changes in the universe between the time of the Census and the time of the study.

The data for whites come from an NORC Amalgam Survey conducted at about the same time as the Crain study. From the 1,526 respondents we selected those who were Northern, metropolitan whites aged 21-45. This group numbers 343, and they are directly comparable with the blacks of the Crain sample. In addition, there were 53 blacks in the Amalgam Survey who meet the age and residence requirements of this study. They were weighted 2.5, the average weight applied to the blacks in the Crain study, and added to the black sample. Thus the total weighted sample of blacks numbers 4,286.

The dependent variable, self-evaluation, was measured by responses to a straightforward question which asked the respondent to evaluate himself on the traits previously mentioned. Specifically, the question stated, "Now I would like you to rate yourself as above average, about average, or below average on some things that you do and some things that you are. First, would you say that as a son (daughter) you were above average, about average, or below average?" The question was then repeated for the other traits.

In general, the responses tended to form dichotomies. On six of the 10 items only about 2% of the subjects rated themselves below average, and on an additional two about 10% rated themselves this low. It is clear, therefore, that the items are not particularly sensitive. However, the responses do seem to form a meaningful pattern. Our view of the self would suggest that there should be considerable consistency in a person's rating of himself over the various traits, but at the same time he should make distinctions among them. If the correlations among the traits were very high, we would suspect a response set. If they were very low, we would worry that the respondents were responding randomly. As it turns out, 26 of the 45 γ 's are between .30 and .60, a range which seems consistent with our expectations. Also, all but one of the lower correlations involve the rating on mechanical ability or the one concerning athletic prowess. It seems predictable that these two traits would be the ones least closely related to the others. On the other hand, the four γ 's which are above .60 involve traits which seem to go together: parent and spouse (.86), intelligence and ability as a conversationalist (.66), willingness to work hard and intelligence (.62), intelligence and trustworthiness (.61). We conclude, therefore, that the respondents are answering in valid terms even though the questions permit only gross categorization.

The index of socioeconomic status is based upon the respondent's education, the occupation of the main wage earner in the family, and the total family income. In cases in which there were missing data the index was calculated on the basis of the available information. The total index is an unweighted sum of the score for each element, and its range is from 0 through 9. The full scale was collapsed to two categories for purposes of tabular control by using a score of 3 as the upper limit of the lower category. This led to a lower-higher distribution of 55-45 for the blacks and 20-80 for whites.⁵

FINDINGS

For each variable, several sets of data are presented in table 1. First there is a comparison of the total samples of blacks and whites using percentage distributions and γ 's. These figures answer the question, Do blacks, as a group, differ from whites as a group? Then, in parentheses, we give the standardized γ —the γ which would result if the distribution of SES in the black group was the same as it is for the whites. From these data we can learn whether any differences between blacks and whites, in general, are due to the differential distribution of SES in the two racial categories. The last two sets of data compare blacks and whites within SES groupings. This information will reveal whether there are statistical interactions.

For the variables of Section I of the table, the key hypothesis is that lower-SES blacks will evaluate themselves more highly than lower-SES whites, and there will be no difference in the higher-SES group.

For the four traits evaluation of self as an offspring, parent, spouse, and conversationalist, our expectations are borne out. There are essentially no differences in the total sample, and this holds for both the standardized and unstandardized comparisons. The differences in the lower-SES are somewhat larger, ranging from 6% to 11%, and they all favor the black respondents. In the higher-SES group the racial differences are again negligible. In sum, then, the data for these variables are consistent with expectations. In the total sample, there is no difference between the self-evaluations of blacks and whites. In the lower-SES grouping, blacks rate themselves a little above whites, and in the higher-SES category there is no difference.

There is a possibility, however, that the figures for the separate status groups are misleading. As might be expected, the average white in *each* status category has a somewhat higher score than the average black in the same category. Thus it is possible that apparent racial differences are

⁵ The choice of a cutting point does not seem crucial. When 4 was used as the dividing line, the results were essentially the same.

TABLE 1
SELF-EVALUATIONS OF BLACKS AND WHITES IN THE TOTAL SAMPLE AND IN
EACH SES GROUPING

| RATING SELF: | TOTAL | | LOWER SES | | HIGHER SES | |
|--|---------|--------|-----------|-------|------------|-------|
| | Black | White | Black | White | Black | White |
| Section I | | | | | | |
| 1. Above average as an offspring | 19.8 | 20.4 | 17.2 | 11.3 | 23.1 | 22.5 |
| γ^* | -.04 | (.03) | .14 | | .00 | |
| N^\dagger | (4,221) | (338) | (2,336) | (62) | (1,885) | (276) |
| 2. Above average as a parent | 26.1 | 27.2 | 24.3 | 13.5 | 28.5 | 30.5 |
| γ | -.03 | (.00) | .28 | | -.05 | |
| N | (3,204) | (265) | (1,867) | (52) | (1,338) | (213) |
| 3. Above average as a spouse | 27.3 | 29.1 | 26.0 | 17.6 | 28.7 | 31.7 |
| γ | -.04 | (-.02) | .23 | | -.06 | |
| N | (2,694) | (278) | (1,408) | (51) | (1,287) | (227) |
| 4. Above average as a conversationalist | 21.6 | 27.0 | 17.3 | 18.8 | 26.8 | 28.9 |
| Below average as a conversationalist | 11.2 | 13.2 | 13.5 | 26.6 | 8.4 | 10.1 |
| γ | -.06 | (.02) | .17 | | -.01 | |
| 5. Above average in attractiveness | 15.0 | 13.3 | 13.4 | 14.1 | 17.0 | 13.1 |
| Below average in attractiveness | 8.7 | 12.1 | 9.9 | 9.4 | 7.2 | 12.7 |
| γ | .11 | (.16) | -.03 | | .20 | |
| 6. Above average as an athlete | 12.9 | 22.9 | 8.3 | 18.8 | 18.7 | 23.8 |
| Below average as an athlete | 35.4 | 29.3 | 40.8 | 29.7 | 28.9 | 29.2 |
| γ | -.19 | (-.09) | -.27 | | -.05 | |
| Section II | | | | | | |
| 1. Above average in willingness to work hard | 42.1 | 56.7 | 36.9 | 50.0 | 48.5 | 58.3 |
| γ | -.27 | (-.20) | -.26 | | -.19 | |
| 2. Above average in trustworthiness | 43.0 | 62.9 | 37.9 | 54.7 | 49.4 | 64.7 |
| γ | -.38 | (-.31) | -.34 | | -.30 | |
| 3. Above average in intelligence | 16.6 | 28.4 | 11.9 | 9.4 | 22.4 | 32.7 |
| γ | -.29 | (-.18) | .19 | | -.24 | |
| 4. Above average in mechanical ability | 21.1 | 32.0 | 18.3 | 29.7 | 24.7 | 32.5 |
| Below average in mechanical ability | 25.3 | 33.1 | 25.4 | 32.8 | 25.2 | 33.2 |
| γ | -.03 | (.00) | -.04 | | .00 | |

* Standardized γ 's are presented in parentheses. They represent the correlation which would exist between race and evaluation if SES were distributed in both races as it is in the white group.

† If N 's are not presented for a particular variable, they are very close to the N 's reported in this line.

really due to uncontrolled variation in SES. However, the SES differences are small. In the lower-status group the average black has a score of 1.8, and the average white, a rating of 2.2. In the higher-status category, the means are 5.6 and 6.4. Given the small correlation between SES and the dependent variables, it is not probable that these differences would make a difference. Variation between the races within an SES category are not due to differential distributions of SES scores in that category.

The ratings on attractiveness to the opposite sex are not consistent with the hypothesis. There is no racial difference in the lower-status group,

but there is one, favoring the blacks, in the higher-SES group. Why this is so is not immediately clear, but it may be that higher-status blacks receive unusually high evaluations on this trait because of the association between status and light skin and the traditional high evaluation of such skin.⁶ Thus on this trait they evaluate themselves highly even though they have less access to the mechanisms we spoke of earlier.⁷ Be that as it may, it should be emphasized that whites do not score higher in either SES group. Blacks do not consider themselves unattractive as would be predicted on the basis of the older view.

The final variable of this set, evaluation of athletic ability, runs in a direction opposed to the previous traits. As has generally been the case, the difference in the higher-status group is negligible, but in the lower SES the blacks give themselves a distinctly lower rating. Furthermore, it appears that most of the difference is between white and black females.⁸

Again we cannot be sure why this is so, but we do not believe that one should conclude that blacks are using whites as significant others or that the dominant society's touchstone is being used. Rather, we think sub-cultural standards are being used and that they are inflated. It may be very difficult for the average lower-status black to measure up to those who are used as standards. This results in a low evaluation, particularly since one cannot blame the system.

In general, then, though some revisions are required in our thinking in two out of the six cases, these results tend to support the basic postulates. It seems reasonable to suggest for these variables that blacks use blacks as significant others. Furthermore, they probably do use subcultural standards and, in some instances, a system-blame explanation.

In regard to the traits in Section II of table 1, the prediction was that whites would generally rate themselves higher than blacks do. For the first two variables, willingness to work hard and trustworthiness, the whites do rate higher in the total groups and the indication is that these differences are not due to the differential distribution of SES in the two

⁶ Udry, Bauman, and Chase (1971) present data which indicate that the relation between skin color and status may be changing. However, for this sample the assumed association is probably still present.

⁷ We have already suggested that higher-status blacks sometimes receive especially favorable evaluations from other blacks. That is one basis for our prediction that there will be no racial differences in the higher SES on this group of variables. What we failed to see was that those evaluations could be so high that higher-status blacks could exceed higher-status whites in self-evaluation despite the existence of the other barriers. This is what we think is happening here.

⁸ On the other variables, with one exception, the relationship between the races does not vary by sex. Crain and Weisman (in press) present a detailed table which controls for sex. However, they set up their white sample differently, so their data are not completely comparable with ours.

racial categories. Furthermore, in each SES group blacks rate themselves lower. These findings, then, are completely consistent with expectations.

In regard to ratings of intelligence the matter is more complicated. The predicted lower ratings by blacks appear in the total sample and in the higher-status group but not in the lower-status group. In this latter case, there is a small difference favoring the blacks.⁹ This finding led us to look deeper, and we compared the racial groups with SES and sex controlled. This comparison produced interesting results. The original hypothesis of lower self-evaluation of intelligence for blacks does hold, though weakly, for lower-SES *males*. Five percent of lower-status black males rate themselves below average, but none of the lower-SES whites do ($\gamma = .19$). For lower-status females, the blacks have higher ratings, though this is more a result of a very low self-evaluation by the whites than it is of high evaluation by the black women. (Eleven percent of lower-status white women rate themselves below average, and only 6% rate themselves above average. The comparable figures for blacks are 2% and 11%.) Furthermore, it turns out that the difference in the higher SES is entirely from the male group. Forty-six percent of higher-SES white males rate themselves above average as compared with 22% of the higher-status blacks ($\gamma = .48$). For the females of both races the proportion is about 22%.

Thus the error involved in this hypothesis seems to be the failure to limit it to males. In this group there is support for it. Furthermore, we were probably not wrong in our assumptions of low evaluations by black women; rather, we failed to foresee that white women would rate themselves so low (compare the ratings on willingness to work hard and trustworthiness). In sum, we believe the hypothesis was not totally correct because the postulates were poorly applied.

In general, these results contrast interestingly with the data from the variables of Section I. With the exceptions already noted, blacks do seem to have lower self-evaluation on these traits. We suggest that this is due to the fact that in these areas their access to self-protective mechanisms is only slightly greater than that of whites, but they receive importantly inferior evaluations. That blacks do receive low evaluations seems reasonable if it is true, as we believe, that many of the relevant evaluations come from whites.

The final variable, mechanical ability, shows no pattern. For both sexes whites are overrepresented in both the above-average and below-average categories. We do not have an explanation of this datum, in part because we are not certain how one arrives at a conception of his mechanical ability. In fact, this may be the answer. The process of developing a self-evaluation

⁹ The percentage differences are very small: no more than 3% in either of the extreme categories. The marked skew in the data leads to a relatively high γ .

in regard to mechanical ability may be different from that involved for the other variables of this section. For example, whites may be much more likely to evaluate blacks' intelligence than their mechanical ability.

In summary, our findings indicate that the relationship between the self-evaluations of blacks and whites varies depending upon the traits involved. For characteristics such as performance of family roles there is no difference in the higher-SES and a difference favoring blacks in the lower-status group. For other traits, such as willingness to work hard, there is a tendency for whites to rate themselves higher in all categories. The differences in the trends are thought to relate to variations in the nature of the significant others, differences in evaluations received, variations in the likelihood of using dominant society standards, and differences in the availability of a system-blame explanation. Though application of these explanatory postulates did not always lead to hypotheses which gained support, the general trend of the data seems to permit a tentative acceptance of them. Six of the 10 hypotheses received clear support. Two others were partially supported or seemed valid under particular conditions. The two which received no support, mechanical and athletic ability, are perhaps of a different order from the rest.

Before concluding, we should emphasize clearly that despite the possible theoretical relevance of the data reported, for practical purposes the most important finding is the small size of the differences. One is struck by the general similarity of the white and black subjects. Only one difference is as large as 20 percentage points, and the largest is only .38. Moreover, what difference does exist is to be found in the proportions who rate themselves above average. On most traits, very few in both groups rate themselves below average. There is no evidence here that blacks are "crippled" by low self-evaluations. All in all, it would seem that the traditional view distorts the situation beyond recognition.

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Aspirations of Low-Income Blacks and Whites: A Case of Reference Group Processes¹

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Aspirations of low-income black and white American adults are analyzed. Two alternative hypotheses derived from reference group theory are tested. According to the first hypothesis, individuals in setting their aspirations are more affected by their position in their own racial group than by the position of their racial group in the larger society. The second hypothesis suggests the reverse. Aspirations, it turns out, are racially patterned in such a way that either the one or the other hypothesis is supported, depending on the type of aspirations examined. A model is presented to account for the seemingly disparate patterns.

This paper deals with aspirations of low-income white and black American adults. The question of what raises or curbs aspirations is explored. The main concern is with patterned differences in aspirations between blacks and whites. Sample survey data are analyzed to test two seemingly conflicting hypotheses derived from reference group theory and published research.

The term aspirations in the present context is used to refer to economic and social aspirations or, in other words, the striving for material improvement and social advancement.²

The idea of strain produced in American society by a disjunction between culturally induced high aspirations—aspirations called for by American equalitarian ideology—and differential opportunities to realize

¹ Revised version of a paper presented at the nineteenth annual meeting of the Society for the Study of Social Problems (SSSP), San Francisco, August 31, 1969. The paper is based on my "Patterns of Aspirations among Low-Income Families of Different Ethnic Backgrounds," Master's thesis, Columbia University, 1965. I am indebted to the Bureau of Applied Social Research, Columbia University, in particular to Allen H. Barton, director, and to David Caplovitz, for making the survey data used in this study available to me for secondary analysis. (For the results of the original study, see David Caplovitz 1963.) For helpful suggestions at varying stages of this study I wish to thank Richard A. Henry, Walter Adams, Kenneth J. Lenihan, Louis Lieberman, Allan H. Barton, David Caplovitz, Terence K. Hopkins, Herbert H. Hyman, and Robert K. Merton. I also appreciate the anonymous readers' comments.

² The point has frequently been made that these aspirations are not of one piece. Economic aspirations do not necessarily imply striving for social ascent, and vice versa (see, e.g., Cloward and Ohlin 1960, pp. 94-95; Lockwood 1960, p. 253). They are here discussed together because they are all part of the American success theme. The question of distinctions between types of aspirations will be taken up later.

these aspirations—the result of social and particularly racial barriers—has been the subject of much debate.³ This study attempts to place empirical findings within a broader theoretical context in an effort to contribute to the knowledge of social forces that determine levels of aspirations. Some clues concerning these forces are given by reference group theory, which alerts one to different social frames of reference (groups or individuals) used by the individual in evaluating himself and setting his aspirations.⁴

In a society in which membership in a racial group affects social status and opportunities and has salience in practically all spheres of life, race can be assumed to play a strategic role as a comparative or normative frame of reference for individuals in evaluating themselves and setting their aspirations. Racially patterned differences in aspirations should therefore be expected.

Numerous studies have attested to such differences (Rosen 1959; Jaffe and Adams 1964, 1969; Bloom, Whiteman, and Deutsch 1965; Coleman et al. 1966; Mack 1968; Elder 1970; Kandel 1971. For a summary of relevant findings of experimental studies, see Hyman 1953, p. 438). According to most research findings, blacks tend to have aspirations higher than (or at least as high as) whites (of varying ethnicity) of comparable socioeconomic status,⁵ when fantasy or wishful thinking is an element in these aspirations. However, when pressed for realistic assessment, that is, aspirations for the realization of which the individual is ready to take appropriate action, blacks tend to reduce their aspirations below those of whites. The usually great discrepancy between ambitions and realistic aspirations or serious plans among blacks has generally been attributed to their low status and history of discrimination (Hyman 1953, p. 438; Rosen 1959, p. 55; Mack 1968, p. 444; Jaffe and Adams 1969, p. 44).

One would think that recent developments in race relations have led to higher realistic aspirations among blacks. However, according to some recent research findings this does not seem to be so (Bell 1967; Elder 1970, p. 90).

THEORETICAL FRAMEWORK AND HYPOTHESES

Despite the fact that all persons in the sample are residents of racially integrated low-income public housing projects in slum neighborhoods,

³ For the most influential statement on this subject, see Merton (1957, pp. 131–60).

⁴ See Merton (1957, pp. 225–386) for the source on reference group theory which has provided most of the intellectual stimulation for this paper. For a comprehensive recent collection of writings on reference groups, see Hyman and Singer (1968).

⁵ It is important to make this qualifying statement. Social class, it appears, is the single most important factor affecting levels of aspirations. Race has, nevertheless, an independent effect on each class level (see, e.g., Rosen 1959; Bloom et al. 1965, p. 473; Elder 1970, p. 476).

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blacks and whites are assumed to differ in important respects; over and above skin color. For blacks, who are faced with severe discrimination in housing, residency in the projects may mean a step forward. The whites, who do not have to face similar barriers, may live in the projects because they are failures. In other words, for blacks the projects may represent a vehicle toward social mobility; for whites a custodial institution.⁶ Data will indicate that in spite of differential selective recruitment (positive in the case of blacks and negative in the case of whites) the usual pattern of discrimination against blacks persists.

Considerations concerning these special characteristics of the sample alert one to two dimensions of relative social position which should be particularly relevant in this case: (1) the position of the individual in his own racial group, and (2) the position of the individual's racial group in the larger social structure. The differences between blacks and whites in the sample along these two dimensions can be diagrammed as shown in figure 1.

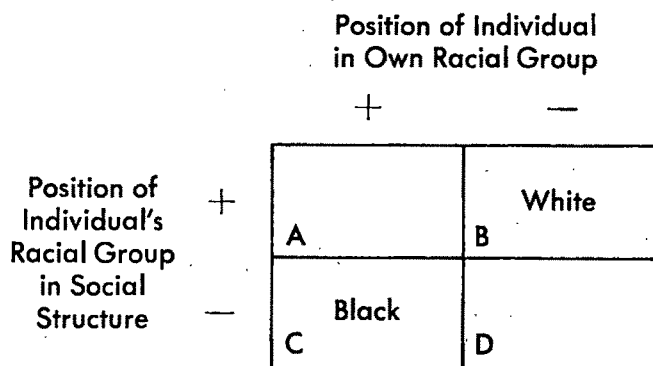


FIG. 1.—Adapted from Runciman (1961, pp. 315–23)

If, as maintained, the individual's racial group is of normative or comparative significance to him in setting his aspirations, prediction of aspirations along racial lines would seem to require an answer to the following question: are the individual's aspirations more affected by his position in his own racial group (does a black American, for instance, set his aspirations primarily according to the position he himself holds relative to other black Americans), or does the position his racial group holds in the larger social structure provide the more significant frame of reference

⁶ The conclusion that public housing projects can be seen either as a vehicle toward social mobility or as a custodial institution and are accordingly preferred by two types of families, the most progressive ones and the most dependent ones, is reached by Back (1962, p. 84) in his study of housing problems in Puerto Rico.

(does a black American, for instance, take the position of black Americans as a group in the larger social structure as the more important context for self-appraisal and formulation of his aspirations)?⁷

The question which of these two dimensions is more important in affecting the individual's aspirations (unless the two are equally effective, which in the present case would mean they cancel each other out) is quite intriguing, because it gives rise to two diametrically opposed hypotheses, both derived from existing reference group theory and supported by empirical data. Yet confirmation of either would seem to imply rejection of the other.

Hypothesis 1. Individuals in setting their aspirations are more affected by the position of their racial group in the larger social structure than by their position in their own racial group. In accordance with figure 1, the whites in this case are more likely to have high aspirations than the blacks.⁸

Hypothesis 2 (alternative hypothesis). Individuals in setting their aspirations are more affected by their position in their own racial group than by the position of their racial group in the larger social structure. In accordance with figure 1, the blacks in this case are more likely to have high aspirations than the whites.⁹

⁷ Other project residents, both of the same as well as of different racial backgrounds, may also be of normative or comparative significance to the respondents in this respect. However, the above two dimensions cover this aspect. The project residents are not isolated but move back and forth between the project and the outside world. It is the very point of the present argument that differences between and similarities among racial groups within the project take on their special meaning only relative to the situation prevailing outside (see Merton, West, and Jahoda 1948, chap. 3, p. 26). Of course, other groups and individuals, irrespective of race, will also be of significance; but in order to account for *racially* patterned differences in aspirations, the two dimensions singled out are considered strategic, given the particular nature of the sample.

⁸ Individuals in this case respond in terms of their objective racial status in the established hierarchy of groups and in terms of their objective position in the opportunity structure, as determined by their race. This is in accordance with the finding that at each successively lower status level (racial and/or social) the level of aspirations decreases (see, esp., Hyman 1953).

⁹ Individuals in this case respond neither in terms of their objective racial status in the established hierarchy of groups nor in terms of their objective position in the opportunity structure as determined by their race. Their definition of the situation reflects the "relationship between their expectations and their achievements *relative to others in the same boat with them*" (Merton [1957, p. 237] quoting Samuel E. Stouffer et al., *The American Soldier* [1949, 1:251]). This theory served to explain the otherwise paradoxical finding that American soldiers were more likely to express satisfaction with their chances of promotion the less their branch afforded opportunity for promotion. According to the argument here followed, those "in the same boat" would be those of the same racial group.

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DATA

Survey data are used for secondary analysis.¹⁰ The sample consists of 177 American adults, 110 blacks and 67 whites, who are residents of four low-income public housing projects in New York City. Two of the projects are located in East Harlem and two on the Lower East Side: Both neighborhoods are known as major slum areas. They are settled predominantly by ethnic minority groups: blacks, Puerto Ricans, people of Italian origin, and Eastern European Jews.

The original sample, of which the one here analyzed represents a sub-sample, was obtained by stratified random sampling.¹¹ Of the 67 whites here included, 42% are Jewish, 25% of Italian origin, and 33% of other ethnic backgrounds. In view of the small number of cases, the different white ethnic groups are combined as "whites" throughout this analysis. Most of the respondents are women, some are men, and in some cases husband and wife were interviewed jointly. Further details concerning the respondents' characteristics will be discussed in the analysis that follows. They are summarized in table 1.

The interviewing was done in 1960. No claim is made that the findings would be the same if the survey were replicated today. But whatever the changes that have occurred since 1960, they do not detract from the purposes of the present analysis which seeks to contribute to the knowledge of social factors of a general nature that help to determine aspirations, over and above specifics of time, place, and persons involved.

TECHNIQUES AND MAJOR VARIABLES

The analysis of survey data will proceed in two steps. First, the claim made concerning the nature of the sample, which prompted the two alternative hypotheses on aspirations, will be substantiated. To this end, blacks and whites in the sample will be compared with regard to three aspects of position. (1) Objective status characteristics, such as education,

¹⁰ The primary purpose of the original study was the examination of consumer practices of low-income families. (For the results see Caplovitz 1963.)

¹¹ The original sample included 464 respondents and was stratified by race, income, and family size. Single-person households were omitted, and two-person households were undersampled. One person per household was interviewed. Further details of the sampling techniques used are discussed in Caplovitz (1963, pp. 6-7). For purposes of this secondary analysis, a considerable number of cases had to be eliminated from the original sample for technical reasons because the necessary data were not available or not properly applicable. Excluded for these reasons are: persons 60 years old or older; persons who are not husband, wife, or head of household; persons without children. Also omitted for the purposes of this paper are all Puerto Ricans. The latter are included as a third group in the more comprehensive analysis presented in Lorenz (1965).

occupation, and the like. Reference will be made in this context to U.S. census data for Manhattan, to show where blacks and whites in the sample stand relative to the larger community. (2) A subjective aspect: the symbolic meaning attached to living in this particular type of housing project, inferred from the respondents' comments on project residency. (3) Objective position in the opportunity structure, measured by indicators of the extent of equality of occupational and income opportunities.

Second, the aspirations of blacks and whites will be examined. Two aspiration indices will be used in the analysis: (1) an index measuring aspirations the respondents express for themselves, referred to as self-aspirations, and (2) an index measuring the aspirations the respondents express for their children, referred to as aspirations for children.¹²

Index of Self-Aspirations

This index is based on answers to three questions, one concerning expectations of future higher earnings, a second concerning expectations of self-employment and/or a different line of work, and a third concerning expectations of home ownership (co-op apartment or house).¹³ The questions refer to expectations rather than preferences. It is assumed that wishful thinking and fantasy answers are minimized that way, and that reasonably realistic aspirations are measured. Since the overall level of aspirations of the people in the sample is low, a person who expressed the expectations specified by either one or more of the three questions is classified as having "high" aspirations, the others as "low." The term high self-aspirations is clearly used in a strictly relative sense.¹⁴

¹² It would have been preferable to combine the two indices in one typology, but the limited number of cases precluded this.

¹³ The index of self-aspirations is based on answers to the following three questions: (a) Income: "What about five years from now? Do you expect that your income then will be much greater than it is now, or less, or about the same?" (b) Occupation: "Does your husband (do you) plan to go into business for himself (yourself) some day?" (If no) "Does your husband (do you) plan to go into a different line of work some day?" (c) Residence: "Do you expect to move out of the project some day?" (If yes) "Are you planning to buy your own home, move to another apartment, or what?" Expectations of future higher earnings, of self-employment and/or a different line of work, and plans to buy a home or co-op apartment are scored as positive.

¹⁴ To ensure that this low cutoff point used for classifying aspirations as "high" does not distort the findings, in view of the three different dimensions of aspirations involved, a second set of tables was prepared in which positive answers to at least *two* of the three self-aspiration questions were classified as "high." While the proportion of persons with "high" self-aspirations was thus obviously reduced, the patterns were the same as those with the classification here used (latter tabulations not shown). A cross-tabulation of the three indicators of self-aspirations shows that they are positively related to each other.

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Index of Aspirations for Children

This index is based on the respondents' answers to two questions, one asking for their preference and a second for their expectations, concerning their children's education.¹⁵ Those who prefer *and* expect their children to go to college are classified as having "high" aspirations for their children, all others as "low."

FINDINGS: STATUS CHARACTERISTICS, THE SYMBOLIC MEANING OF PROJECT RESIDENCY, AND OPPORTUNITY STRUCTURE¹⁶

Status Characteristics

It has been suggested that for most of the blacks the projects may represent a vehicle toward social mobility, and for most of the whites a custodial institution where they live because they have failed to "get ahead." Data on objective status characteristics of blacks and whites support this view (table 1).¹⁷

Age, length of residence in New York City, and place of birth.—From table 1 it may be seen that most of the whites (references here are to household heads) are older (70% are age 40 or over) and have lived in New York for a long time (91% for over 20 years); the majority are in fact born in the city (54%). Most of the blacks, on the other hand, are younger (74% are under age 40), more recent arrivals in New York

¹⁵ The index of aspirations for children is based on answers to the following two questions: (a) Preference: "Would you rather have your boy(s) get a job when he finishes (they finish) high school, or would you rather have him (them) go to college?" (b) Expectation: "Which do you think is more likely to happen? That he'll (they'll) go to work or go to college?" These two questions were to be answered only by parents who had sons. Since they were also answered by some parents who had daughters only, all cases where answers are available are included, and reference is made to "aspirations for children" rather than to "aspirations for sons." Parents' aspirations for sons and daughters may differ (see, e.g., Kandel 1971), but it was felt more useful to include these (few) additional cases, since the data did not suggest an undue bias.

¹⁶ A note on terminology is in order. Sometimes, to avoid cumbersome language in the discussion of the findings, terminological shortcuts are employed. Thus, a statement such as "the blacks have higher aspirations than the whites," or "the blacks rank higher in aspirations than the whites," should be understood to refer to differences in percentage distributions, in accordance with operational definitions. Terms such as "older" and "younger," "high" and "low," when applied to variables, are used in a purely relative sense and should be understood as operationally defined. Also, if at times it is said that a variable "affects" another variable, such as "age affects aspirations," it should be understood that such cause-and-effect relationships are inferred.

¹⁷ Personal characteristics in table 1 are given for heads of households rather than for respondents (in over half of the households the wife rather than the household head was interviewed—see table 1, last item), since the social status of the family is determined primarily by the social characteristics of the household head.

TABLE 1
STATUS CHARACTERISTICS OF BLACKS AND WHITES
(BLACKS *N* = 110; WHITES = 67)
(%)

| Status Characteristics | Black | White |
|--|-------|-------|
| Age of head of household (yrs): | | |
| 20-29 | 32 | 9 |
| 30-39 | 42 | 22 |
| 40-49 | 23 | 43 |
| 50-59 | 4 | 25 |
| | 27 | 70 |
| Length of residence in New York City, head of household (yrs): | | |
| Less than 6 | 7 | 0 |
| 6-10 | 18 | 3 |
| 11-20 | 39 | 6 |
| Over 20 | 37 | 91 |
| Place of birth, head of household: | | |
| New York City | 15 | 54 |
| U.S. South | 69 | 4 |
| U.S. other | 13 | 25 |
| Europe | 0 | 16 |
| Other foreign | 4 | 0 |
| Education, head of household: | | |
| Elementary school or less | 25 | 30 |
| Some high school | 45 | 36 |
| Completed high school | 21 | 29 |
| Some college, including nursing and technical schools .. | 7 | 3 |
| Completed college | 1 | 1 |
| | 29 | 33 |
| Family income (\$): | | |
| Under 3,000 | 29 | 23 |
| 3,000-3,999 | 33 | 22 |
| 4,000-4,999 | 26 | 39 |
| 5,000 or more | 13 | 15 |
| | 39 | 54 |
| Source of income: welfare part or whole | 17 | 10 |
| Occupation, head of household: | | |
| Unskilled | 44 | 15 |
| Skilled or semiskilled | 33 | 48 |
| White collar or more | 11 | 21 |
| Own business | 0 | 3 |
| Does not apply (welfare, social security, etc.) | 13 | 13 |
| Number of children in household: | | |
| One or two | 47 | 65 |
| Three | 22 | 18 |
| Four or more | 30 | 16 |
| Type of respondent: | | |
| Wife | 53 | 55 |
| Female head of household (fatherless family) | 21 | 15 |
| Husband | 13 | 6 |
| Male head of household (not husband) | 0 | 1 |
| Both husband and wife | 13 | 22 |

NOTE.—Occasional "no answer" cases are omitted. This procedure will be followed in subsequent tables without further mentioning. In this and some of the following tables, percentages do not always add up to 100% due to rounding.

City (only 37 % have lived in the city for over 20 years), and were born in the South (69%).

Education, occupation, family income, and source of income.—The majority of blacks as well as of whites have not graduated from high school, are blue-collar workers, and have incomes below \$5,000, a clear indication of the overall low socioeconomic status of the respondents. The similarity of blacks and whites in these respects is what one would expect in view of the eligibility criteria for low-income public housing, but it is remarkable if seen against the great cleavage between the two groups in the larger community (to be presently documented by some census data).¹⁸ Slightly more of the blacks than of the whites (17% and 10%, respectively) receive welfare assistance. The differences in the larger population are in the same direction but more pronounced (see Moynihan 1965, p. 58).

Family composition and number of children.—The proportion of fatherless white families in the sample is nearly as high as the proportion of fatherless black families, 15% and 21%, respectively. In contrast, the corresponding figures for the United States as a whole are 9% for whites and 21% for nonwhites (Moynihan 1965, p. 57).

The blacks in the sample tend to have larger families than the whites (30% of the blacks and 16% of the whites have four or more children). Fertility is known to vary inversely with class position and with upward mobility (Lipset and Bendix 1960, pp. 85–88, 240–44), but blacks traditionally have larger families than whites (Nixon 1962; Moynihan 1965, pp. 72, 118). Considering also the age differences between blacks and whites (many of the whites may have older children who no longer live with their parents, and many of the blacks may have just started founding families), the data on family size tells us little about the comparative social standing of blacks and whites.

To sum up the findings on status characteristics presented in table 1, all but the inconclusive data on family size point to differential selective recruitment—positive in the case of the blacks, and negative in the case of the whites. They suggest that the projects may well represent a vehicle toward social mobility for blacks, and a custodial institution for whites who are “failures.”

¹⁸ The fact that the blacks in this sample are similar to the whites in level of education can at least in part be accounted for by their younger age (since younger adults in the United States tend to have had more years of schooling than older adults). Yet the facts that the whites tend to be somewhat better off than the blacks in terms of income and occupation (54% of the whites and 39% of the blacks have family incomes of \$4,000 or more; 15% of the whites and 44% of the blacks are unskilled workers) can hardly be accounted for by age differences in this sample of predominantly blue-collar workers. These findings rather suggest that with comparable levels of schooling whites have higher income and better jobs than blacks (to be substantiated below).

Education: sample data compared with census data for Manhattan.—A particularly clear picture of the relative social position of blacks and whites in the sample and in the larger social structure can be obtained when relevant sample data are compared with corresponding U.S. census data for Manhattan. Manhattan, the respondents' borough of residence, seems the larger social context most appropriate for these purposes. Table 2 gives the comparative data for level of formal schooling as one important

TABLE 2
RATIOS OF PROPORTIONS OF BLACKS AND WHITES WHO COMPLETED HIGH SCHOOL OR MORE—SAMPLE DATA COMPARED WITH CENSUS DATA FOR MANHATTAN

| Completed High School or More | Black | White |
|--|-------|-------|
| 1. Standing of sampled racial groups relative to total larger community (ratio of sample percentage of each racial group to total Manhattan population percentage) | 0.69 | 0.79 |
| 2. Standing of sampled racial groups relative to corresponding racial groups in larger community (ratio of sample percentage of each racial group to total Manhattan percentage of corresponding racial group) ... | 1.04 | 0.72 |
| 3. Standing of racial groups in larger community relative to total larger community (ratio of total Manhattan racial group percentage to total Manhattan population percentage) | 0.67 | 1.10 |

SOURCE.—For data for Manhattan, see U.S. Bureau of the Census (1962) table P-1, p. 23; table P-4, p. 683.

NOTE.—The data for the sample refer to household heads. The data for Manhattan are based on "years of school completed, persons 25 years old and over." Blacks in the sample are compared with nonwhites in Manhattan. Corresponding data for blacks separately are not available. Since blacks make up 93% of the total nonwhite population of Manhattan (computed from U.S. Bureau of the Census [1962], p. 23), the data seem adequate for comparative purposes. The nonwhites in Manhattan are referred to as blacks with this understanding.

indicator of social status. To make the relative positions of the different groups of interest clear at a glance, ratios calculated from detailed percentage tables are given. Since the general level of education and skill requirements in American society today is such that completion of high school is all but mandatory for "getting ahead," the ratios are based on the percentage of adults who have completed high school or more, as most relevant to the subsequent analysis of aspirations. A ratio of one would mean that the respective racial group equals the group in the larger structure (in proportion of high school graduates) with which it is compared. The lower the ratio, the more unfavorable the standing of the respective racial group relative to the group in the larger social structure singled out for comparison.

The focus in table 2 is on three comparisons which, together with the respective findings, may be summarized as follows:

1. A comparison of each of the two racial groups in the sample with

the total larger community testifies to the overall low educational status of both groups in the sample. That of the whites, as previous data on formal schooling indicated, is slightly higher than that of the blacks. (The respective ratios are 0.79 for whites and 0.69 for blacks.)

2. A comparison of each racial group with its counterpart in all of Manhattan—to indicate the sampled group's position in its racial group at large—reveals that the blacks in the sample rank higher in educational status than the blacks in the larger community (ratio 1.04). The whites, on the other hand, compare very unfavorably with their counterparts in the larger community (ratio 0.72).

3. A comparison of whites and blacks in the larger community with the total Manhattan population—to give an indication of the racial group's position in the larger social structure—clearly shows how much higher the whites' educational status in the larger community is than that of the blacks. (The respective ratios are 1.10 and 0.67.)

In short, the data in table 2 clearly support the claims made concerning the differences in relative positions of blacks and whites as shown in figure 1. The social status (using level of education as an indicator) of blacks as well as of whites in the sample is low relative to the larger community. The blacks compare favorably and the whites very unfavorably with their counterparts in the larger community. The status of whites in the larger community is high, and the status of blacks in the larger community is low.¹⁹

The Symbolic Meaning of Project Residency

It has been suggested that blacks and whites may attach different symbolic meaning to living in integrated public housing projects in these particular neighborhoods. The wider social context, which for the blacks entails discrimination and frequently substandard housing, may endow the projects with special positive significance for the blacks (see Merton et al. 1948, chap. 3, p. 6). For the whites, living in low-income projects in racially mixed slum neighborhoods may be a factor of negative significance.

Data on the respondents' attitudes toward the projects offer some sup-

¹⁹ Since education is inversely related to age generally among American adults, this puts the blacks (largely younger) at an advantage over the whites (largely older) when they are compared with regard to level of education in this particular sample. However, the fact remains that the blacks compare favorably with the whites, which is all that is needed to support the argument concerning selective recruitment. Furthermore, comparable tabulations based on level of occupation and family income produce similar patterns as the one shown in table 2 for education (see Lorenz 1965). They are somewhat less pronounced, as one would expect in view of the point made concerning age plus the fact that discrimination would show in data on levels of occupation and income more than in data on level of education.

port for this assumption. They were asked whether they liked their project apartment better than their previous one, or whether they preferred their former apartment.

As may be seen from table 3, most of the blacks and whites prefer their project apartment to their previous one. Slightly more blacks than whites say so. The differences are in the expected direction. The fact that they are small may be due to similarities in actual objective housing conditions experienced by blacks and whites before they moved into the projects.²⁰ The precoded questions may elicit this factual information.

TABLE 3
ATTITUDE TOWARD PROJECT APARTMENT, BY RACE
(%)

| Attitude toward Project Apartment | Black | White |
|---|-------|-------|
| Response in answer to precoded question: | | |
| Project apartment better | 94 | 88 |
| Old apartment better | 6 | 12 |
| No difference | 0 | ... |
| Total (<i>N</i> = 100) | (109) | (67) |
| Comments about project, neighborhood, or apartment volunteered: | | |
| Positive comments | 47 | 18 |
| Negative comments | 6 | 6 |
| Both positive and negative comments | 15 | 37 |
| Neutral comments | 5 | 0 |
| No comments | 26 | 39 |
| Total (<i>N</i> = 100) | (110) | (67) |

NOTE.—Question: "Do you like this apartment better than the one you were living in before you moved to this project, or did you like your old apartment better?" (Categories: project apartment better/old apartment better/no difference.) Comments volunteered also to be recorded by interviewer.

This assumption is supported by the data in the second part of table 3, which summarizes the comments volunteered by the respondents. These spontaneous comments are particularly interesting because they are more likely to measure subjective feelings. Differences between blacks and whites are in the same direction as in the first part of the table, but they are much more pronounced. Only 18% of the whites, but 47% of the blacks, make spontaneous favorable comments about the projects. The whites are more likely than the blacks to make both negative and positive comments, apparently an indication of their ambivalence created by

²⁰ The majority of blacks and whites alike may have lived in substandard or lower-standard dwellings before. We know, for instance, that almost three in every 10 families did not have their own apartment before they moved into these projects, and that a sizable number lived in the slum buildings that gave way to the projects (Caplovitz 1963, pp. 5, 33).

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their recognition of objective improvement in their housing combined with a somewhat negative subjective evaluation of the situation.

These data would indeed seem to imply that project residency is likely to take on positive significance for the blacks and negative significance for the whites.

Opportunity Structure

The blacks' and whites' "life chances in the opportunity structure" (Merton 1957, p. 175) will be examined as a third aspect of their social position. Some of the earlier data (see n. 18) suggested that the usual pattern of discrimination against blacks also holds for the blacks in this sample, and deprives them of equality of opportunity, in spite of apparent positive selective recruitment. An examination of occupational and income opportunities of blacks and whites will provide some more concrete evidence.

Equality of occupational opportunity is operationally defined as equality of occupational status for a given level of formal schooling. Equality of income opportunity is operationally defined as equality of earning for a given level of occupation. Obviously, these are only crude measures of equality of opportunity. Nevertheless, the corresponding cross-tabulations clearly show that the blacks are at a disadvantage relative to the whites in both instances.

TABLE 4
OCCUPATION OF EMPLOYED HEAD OF HOUSEHOLD, BY EDUCATION AND RACE
(%)

| OCCUPATION | ELEMENTARY SCHOOL OR LESS | | SOME HIGH SCHOOL | | COMPLETED HIGH SCHOOL OR MORE | |
|----------------------------------|------------------------------|-------|------------------|-------|----------------------------------|-------|
| | Black | White | Black | White | Black | White |
| Unskilled | 62 | 19 | 45 | 20 | 32 | 12 |
| Skilled and semiskilled | 38 | 81 | 42 | 65 | 48 | 25 |
| White collar or more | 0 | 0 | 13 | 15 | 20 | 62 |
| Total (<i>N</i> = 100) .. | (21) | (16) | (38) | (20) | (25) | (16) |

Table 4 indicates that at each level of formal schooling the blacks tend to have lower-level jobs than the whites. With increasing education, both groups' occupational status increases, but the whites' more so than the blacks'. For instance, of the blacks who completed high school or more, only 20% have white-collar jobs. The corresponding figure for whites is 62%.

TABLE 5

PERCENT OF MALE HEADS OF HOUSEHOLD WITH ANNUAL INCOMES
OF \$4,000 OR MORE, BY OCCUPATION AND RACE

| Occupation | Black | White |
|-------------------------------|---------|---------|
| Unskilled | 32 (34) | 60 (10) |
| Skilled and semiskilled | 54 (28) | 65 (26) |
| White collar or more | ... (7) | 67 (12) |

NOTE.—Since income data are available only for "total family income," families with more than one earner are here omitted, as well as families with income from sources other than earnings alone. Also, female heads of household are omitted. The base of each percentage is shown in parentheses.

Inequality of opportunity is not confined to the area of occupation. Table 5 shows that it applies to income as well. On each level of occupation for which enough cases are available for meaningful comparison, whites tend to have higher incomes than blacks.

Summary of Findings on Status Characteristics, the Symbolic Meaning of Project Residency, and Opportunity Structure

In sum, the findings concerning the three aspects of social position examined—objective status characteristics, the symbolic meaning attached to project residency, and life chances in the opportunity structure—support the initial assumptions. Differences in status characteristics make the blacks compare favorably, and the whites quite unfavorably, with their counterparts in the larger community and suggest differential selective recruitment. Differences in attitudes toward project residency appear to reflect this situation. Inequalities in the opportunity structure nevertheless persist and mirror the inequalities prevailing in the larger society.

From all available evidence it may be concluded that the two racial groups occupy the positions as shown in figure 1. The preconditions for testing the alternative hypotheses on aspirations are thus established.

ASPIRATIONS

The findings on aspirations, referred to as self-aspirations and aspirations for children, are based on the two indices described above. First, overall percentage distributions and the intercorrelation of the two indices, each according to race, will be examined. Subsequently, selected additional variables which can be assumed to be related to aspirations independently of race, or to mediate the relationship between race and aspirations, will be introduced to test the original relationships for spuriousness and to pro-

²¹ When status and opportunities were examined the head of household was used as the unit of analysis, since he primarily determines the position of the entire family. In the subsequent analysis of aspirations the family member interviewed (see table 1,

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vide for further elaboration.²¹ In this subsequent analysis self-aspirations and aspirations for children will be presented side by side to allow for easy comparison. Since the operational definition of what constitutes "high" aspirations in the case of each index is determined by the specific items included, and by arbitrary cutoff points on the basis of the given distributions, the focus in these comparisons should be not on absolute but on relative percentage figures and on patterned differences.

Self-Aspirations

How do blacks and whites compare with regard to self-aspirations? The blacks, as table 6 indicates, tend to have higher self-aspirations than the whites. Seventy-seven percent of the blacks, as compared with 58%

TABLE 6
SELF-ASPIRATIONS, BY RACE
(%)

| Self-Aspirations | | Black | White |
|------------------|-------|-------|-------|
| High Medium | High* | 45 | 18 |
| | | 31 | 40 |
| Low | Low* | 23 | 42 |
| Total (N = 100) | | (110) | (67) |

* The index will be used in this dichotomous form in the subsequent analysis to allow for multivariate breakdowns.

of the whites, have high self-aspirations according to what is defined as "high" for the purposes of this analysis. (If giving a positive answer to two or all three questions, rather than to at least one, is defined as "high," the difference in aspiration levels of blacks and whites is even more pronounced—45% and 18%, respectively.)

Aspirations for Children

Given this particular sample and the findings on self-aspirations, one might expect that the blacks also outrank the whites in aspirations for their children. The data on aspirations for children may therefore come as a surprise (table 7). Whites are more likely than blacks to have high aspirations for their children. The differences are less pronounced than

last item) will be used as the unit of analysis, since the respondent's aspirations do not necessarily correspond to those of the household head. In cases where husband and wife were interviewed jointly, the husband is used as unit of analysis.

TABLE 7
ASPIRATIONS FOR CHILDREN, BY RACE
(%)

| Aspirations for Children | | Black | White |
|--|-------|-------|-------|
| High (prefer college and expect college) .. | High* | 40 | 45 |
| Medium high (prefer college and don't know what to expect) | Low* | 35 | 28 |
| Medium low (prefer college and expect work) | | 15 | 15 |
| Low (have no preference or prefer work) .. | | 10 | 12 |
| Total (<i>N</i> = 100) | | (110) | (67) |

* The index will be used in this dichotomous form in the subsequent analysis, to allow for multivariate breakdowns.

in the case of self-aspirations. Forty-five percent of the whites and 40% of the blacks have high aspirations for their children, that is, they prefer and expect their children to go to college.

As may also be seen from table 7, the proportion of blacks who would *like* their children to go to college is slightly higher than that of whites: 90% of the blacks and 88% of the whites said they would *like* their children to go to college (the first three percentage figures in each column combined). The blacks appear to be simply less certain about their children's prospects, as the larger proportion of "don't know" answers in reply to the expectation question suggests. The findings on aspirations for children, then, are very much in line with those from other studies, in spite of the unusual sample.

Relations between Self-Aspirations and Aspirations for Children

In trying to predict what a cross-tabulation of the two indices will show, one can, on the basis of the findings just presented, say that the blacks will outrank the whites in the category "high self-aspirations, low aspirations for children," and that the whites will outrank the blacks in the reverse category. But which racial group will have the highest overall aspirations, that is, outrank the other group in the category "high self-aspirations and high aspirations for children," and which of the two groups will have the lowest overall aspirations? The findings are presented in table 8.

The blacks, as may be seen, tend to have higher overall aspirations than the whites: 36% of the blacks express high aspirations of both types, as compared with 25% of the whites. Conversely, the whites slightly exceed the blacks in the "low low" category. The respective percentages in the "inconsistent" categories—"high" in one type of aspirations and "low"

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TABLE 8

RELATIONSHIP BETWEEN SELF-ASPIRATIONS AND ASPIRATIONS OF CHILDREN, BY RACE
(%)

| Self-Aspirations | Aspirations For Children | Black | White | Difference (Black — White) |
|-----------------------|--------------------------|-------|-------|----------------------------|
| High | High | 36 | 25 | +11 |
| High | Low | 41 | 33 | + 8 |
| Low | High | 4 | 19 | -15 |
| Low | Low | 19 | 22 | - 3 |
| Total (N = 100) | | (67) | (110) | |

in the other—are as predicted from the earlier findings. The percentage differences between blacks and whites are not very pronounced. Nevertheless, the finding that the blacks have the highest overall aspirations seems particularly noteworthy.²²

Introduction of Third Variables into the Relationship between Race and Aspirations

Age and socioeconomic status.—In table 9, age and socioeconomic status (SES) are introduced simultaneously into the relationship between race and self-aspirations, and into the relationship between race and aspirations for children.²³ Both variables are strongly related to aspirations²⁴

²² The pattern remains consistent when age and socioeconomic status (SES) are held roughly constant, as far as can be judged from the small number of cases available for comparison in this case (tabulations not shown). Both age and SES are strongly related to aspirations, as will be presently shown.

²³ SES is measured by an index based on income, occupation, and formal schooling. Combining these items is often inadvisable; it has been done here for reasons of expediency. Blacks and whites differ with regard to the three items, and the items cannot be controlled simultaneously unless combined in an index. Level of occupation, income, and formal schooling have each been assigned a score of 0 (low), 1 (medium), or 2 (high). The sum of the scores on the three items represents a person's SES score, so that the lowest possible SES score is 0, the highest possible SES score 6. The SES scores of 0, 1, or 2 are classified in this analysis as "low," and those of 3-6 as "high." For further details, see Lorenz (1965, p. 123).

²⁴ As one might expect on the basis of the items included in the two indices, age is more strongly related to self-aspirations and SES more strongly to aspirations for children (for details see Lorenz 1965). Since blacks and whites are not matched with regard to age and SES, these two factors should be controlled simultaneously. A more refined control for age and SES simultaneously than that presented in table 9 would be desirable, and has in fact been done, but is not meaningful with the available number of cases. More detailed breakdowns by age separately (age categories 20-29, 30-39, 40-49, and 50-59), and by SES separately (five SES categories based on the scores discussed in n. 23 above) suggest that also *within* the broader age and SES categories used in table 9 the racially patterned differences in aspirations are main-

TABLE 9

PERCENT OF BLACKS AND WHITES WITH HIGH ASPIRATIONS,
BY SOCIOECONOMIC STATUS AND AGE

| Aspirations by SES and Age | Black | White | Difference (Black — White) |
|---|---------|---------|-------------------------------|
| Self-aspirations: | | | |
| Higher SES and younger | 83 (53) | 76 (17) | + 7 |
| Lower SES and younger, or higher SES and older | 71 (46) | 54 (35) | +17 |
| Lower SES and older | 55 (11) | 47 (15) | + 8 |
| Aspirations for children: | | | |
| Higher SES and younger | 51 (53) | 53 (17) | — 2 |
| Lower SES and younger, or higher SES and older | 30 (46) | 43 (35) | —13 |
| Lower SES and older | 27 (11) | 40 (15) | —13 |

NOTE.—“Younger” = age 20–39; “older” = age 40–59.

and, as we know from the previous analysis, to race as well (the blacks tend to be younger and of lower SES). Since a four-variable cross-tabulation reduces the number of cases in too many of the cells to insignificance in this small sample, those combinations of age and SES categories which lead to similar results have been combined in table 9 as indicated. The effect of age and SES, in the case of both self-aspirations and aspirations for children, is cumulative. In each racial group the percentages for respondents with aspirations consistently decrease from “higher SES and younger” to the intermediate categories to “lower SES and older.”

The racially patterned differences in aspirations—our main concern—are nevertheless maintained. At each SES and age level, blacks tend to have somewhat higher self-aspirations but somewhat lower aspirations for their children than whites.

Sex and family status.—These variables also tend to be independently related to aspirations (Hyman 1953, p. 431; Coleman et al. 1966, p. 186; Kandel 1971) and have therefore been introduced here as test factors. The relevant findings are presented in table 10.²⁵

tained. There is a marked drop in self-aspirations after age 40. Around this age many appear to give up realistic hopes for improving their own situation. (Tabulations shown.) As one of the respondents, a white taxicab driver in his forties, put it, “When you are over 40, you might as well quit trying.” Use of the age categories 20–39 and 40–59, in order to be able to hold age and SES roughly constant simultaneously, would therefore seem to be the most meaningful alternative under the given circumstances.

²⁵ In view of the findings concerning age and SES discussed above, these two factors should be simultaneously controlled in this and subsequent tabulations dealing with aspirations. This has been done by introducing age and SES simultaneously at least in dichotomous form, but these multivariate breakdowns are not presented here because they leave too many cells with insufficient cases for meaningful comparison. Reference

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TABLE 10
PERCENT OF BLACKS AND WHITES WITH HIGH ASPIRATIONS,
BY SEX AND FAMILY STATUS

| Aspirations and Family Status of Respondent | Black | White | Difference (Black — White) |
|---|---------|---------|-------------------------------|
| Self-aspirations: | | | |
| Female head of household | 63 (24) | 30 (10) | +33 |
| Wife | 78 (58) | 54 (37) | +24 |
| Husband | 86 (14) | ... (4) | ... |
| Husband and wife | 93 (14) | 87 (15) | + 6 |
| Aspirations for children: | | | |
| Female head of household | 29 (24) | 30 (10) | — 1 |
| Wife | 40 (58) | 51 (37) | —11 |
| Husband | 43 (14) | ... (4) | ... |
| Husband and wife | 57 (14) | 33 (15) | +24 |

Female heads of fatherless families, blacks as well as whites, clearly have consistently lower aspirations than others. Family instability and correspondingly low morale may account for it, although this pattern need not necessarily obtain (Kandel 1971). Black fathers tend to have higher self-aspirations and, contrary to other research findings, even somewhat higher aspirations for their children, than mothers. But not much significance can be attached to the specific percentage figures because of the small number of cases involved. One might speculate that the high aspirations expressed by husbands and wives interviewed jointly (except in one subgroup) are the result of particular cohesiveness in these families.

Again focusing on the primary purpose of this analysis, the patterned differences in aspirations between blacks and whites persist when sex and family status are held constant. Blacks tend to have higher self-aspirations and lower aspirations for their children than whites. (The one inversion in the latter case disappears when age and SES are taken into account—latter tabulation not shown.)

Family size.—Numerous research findings show that level of aspirations varies inversely with family size (see, for instance, Jaffe and Adams 1964, p. 271). From table 11 it may be seen that this also holds for the present sample. It is true for blacks as well as for whites, and for both types of aspirations, except that blacks with many children are about as likely to have high aspirations for their children as blacks with few children (the respective percentages are 42% and 43%).

With the exception of the findings involving this last-mentioned subgroup, the racially patterned differences in aspirations of interest here

to these breakdowns will only be made in the case of meaningful findings worth mentioning.

TABLE 11
PERCENT OF BLACKS AND WHITES WITH HIGH ASPIRATIONS,
BY NO. CHILDREN
("COMPLETE" FAMILIES ONLY)

| Aspirations and No. Children | Black | White | Difference (Black — White) |
|---------------------------------|---------|---------|-------------------------------|
| Self-aspirations: | | | |
| Up to three children | 83 (60) | 65 (45) | +18 |
| Four or more children | 77 (26) | 41 (11) | +36 |
| Aspirations for children: | | | |
| Up to three children | 43 (60) | 56 (45) | —13 |
| Four or more children | 42 (26) | 18 (11) | +24 |

NOTE.—Refers to number of children living in the household.

again persist. Blacks have higher self-aspirations and lower aspirations for their children than whites.

Wealthy friends and relatives.—Last, the effect on aspirations of a variable which may be regarded as an indirect measure of anticipatory socialization—the orientation to statuses which a person does not yet occupy but to which he aspires (Merton 1957, pp. 265 ff.)—will be examined as particularly relevant in this context. Respondents were asked whether they had friends or relatives whom they thought were *very* well off financially. Those who answered this question in the affirmative presumably have friends or relatives whose economic status is considerably higher than their own. Do they refer themselves to such individuals, and does any such orientation affect the aspirations of blacks and whites in different ways? Table 12 suggests the answer. It appears that such

TABLE 12
PERCENT OF BLACKS AND WHITES WITH HIGH ASPIRATIONS,
BY ECONOMIC STATUS OF FRIENDS/RELATIVES

| Aspirations and Economic Status of Friends/Relatives | Black | White | Difference (Black — White) |
|--|---------|---------|-------------------------------|
| Self-aspirations: | | | |
| Have wealthy friends/ relatives | 90 (20) | 63 (16) | +27 |
| Have no wealthy friends/ relatives | 75 (89) | 57 (51) | +18 |
| Aspirations for children: | | | |
| Have wealthy friends/ relatives | 45 (20) | 75 (16) | —30 |
| Have no wealthy friends/ relatives | 39 (89) | 35 (51) | + 4 |

NOTE.—Question: "Do you have friends or relatives that you would say are *very* well off financially?"

reference processes do in fact operate. Among both blacks and whites those who have wealthy friends or relatives are more likely to have high self-aspirations as well as high aspirations for their children than those who do not have wealthy friends or relatives. Among blacks, having wealthy friends or relatives seems to encourage self-aspirations more than aspirations for their children. Among whites, the reverse seems to be true.

Assuming these differences are not due to sampling bias, one might speculate on what could account for them. Presumably the wealthy friends and relatives will be mostly of the respondent's own race. For blacks, a college education has so far been less of an avenue to success than for whites. The blacks' wealthy relatives or friends may have become prosperous through what Hyman calls "deviant occupations," that is, "occupations which the more genteel classes might regard with disdain" (Hyman 1953, p. 441). This may provide greater encouragement for the blacks to strive for goals of the type measured by the self-aspiration index than for educational achievement. Conversely, the whites' wealthy relatives or friends may more often be college educated, and the whites may for this reason be more likely to perceive a higher education as important for success. In other words, racially patterned differences in the road to success of wealthy relatives or friends that serve as role models may impress the importance of a college education much more upon the whites than upon the blacks.²⁶

In spite of the strong effect of this intervening variable on aspirations, the original relationship between race and aspirations is consistently maintained in the case of self-aspirations (blacks "high" and whites "low"). In the case of aspirations for children, the usual pattern (whites "high" and blacks "low") holds for those who do have wealthy relatives or friends, but not for those without wealthy relatives or friends.

Summary of Findings on Aspirations

The findings on aspirations of blacks and whites may be briefly summarized as follows. Overall levels of self-aspirations and aspirations for children, as measured by the two indices, are low, as would be expected in a sample of low-income adults.

More blacks than whites have high aspirations when self-aspirations

²⁶ The observation that the type of role models available to blacks plays an important part in their aspirations and achievement has been made many years ago by Dollard (1937) in an entirely different context. Reporting on life in a Southern town, he notes: "A Negro plantation manager, a very rare specimen, said that the Negroes on his plantation tend to improve faster under a Negro manager. They say to themselves that what he can do, they can do; whereas with a white boss they feel that the gulf is too great and make no effort to improve" (p. 66).

and aspirations for children are considered jointly, as indicated by a cross-tabulation of the two indices. This observation, however, conceals what would seem to be a more important finding: blacks and whites differ with regard to *types* of aspirations. Blacks are *more* likely than whites to have high self-aspirations. Conversely, blacks are *less* likely than whites to have high aspirations for their children.

It is important to note that the latter finding holds only when realistic aspirations are measured, but not as long as wishful thinking is involved; a slightly larger proportion of blacks than of whites said they would *like* their children to go to college. There are some indications that the blacks' tendency to reduce their realistic aspirations for their children below those of the whites has to do with feelings of inferiority and discouragement resulting from their low position in the larger society—which is what other researchers cited earlier have suggested. In situations that may be thought of as enhancing their self-esteem, the blacks tend to have high realistic aspirations for their children. High SES, long-term residence in New York City, and a "stationary high" position on the intergenerational mobility scale (the latter two findings are not presented here—see Lorenz 1965) are such instances.

The racially patterned differences in opposite directions persist when additional variables are introduced for control purposes—consistently so in the case of self-aspirations and mostly so in the case of aspirations for children. The differences on the whole are small. Obviously, the blacks' and whites' definition of the situation and correspondingly their aspirations are to a considerable extent determined in similar ways by similarities in physical and social environment and status characteristics unrelated to race. The effect on aspirations of the additional variables introduced as test factors attests to that. These additional variables are related to aspirations in ways generally consistent with other research findings.

Having to operate with slight margins brought forward methodological inadequacies which, in part at least, result from the fact that the sample is not stratified according to the needs of this secondary analysis. While this leaves the interpretation of data often tentative, the findings nevertheless suggest interesting theoretical considerations.

THEORETICAL IMPLICATIONS OF THE FINDINGS

The findings do not lead to the rejection of either of the two hypotheses. They make clear that we cannot speak of aspirations in general but have to distinguish between different types of aspirations. Depending on the kind of aspirations measured, either the one or the other hypothesis is supported.

The findings on aspirations for children support hypothesis 1: the *whites* are more likely to have *high aspirations for their children* than the blacks. The findings on self-aspirations support hypothesis 2: the *blacks* are more likely to have *high self-aspirations* than the whites.

Why would this be so? What is the significant distinction between the two types of aspirations that accounts for these seemingly contradictory patterns?

Differences between the two types of aspirations have been referred to earlier: differences in focus (self vs. children), in content (education vs. higher income etc.), and in level (relative vs. absolute) have been mentioned.

The fact that in one case the focus is on the respondents' aspirations for themselves while in the other case the focus is on their aspirations for their children, might suggest that the blacks are less concerned than the whites with their children's future. However, the analysis at various points indicated that this is not so. A number of findings point to the conclusion that feelings of inferiority and discouragement rather than lack of concern made the blacks reduce their realistic aspirations for their children, even though it was contrary to their preference.

Second, differences in the specific content of the two kinds of aspirations have been mentioned as possibly important. However, to attribute the racially patterned differences to the specific content of the aspirations seems to be ruled out for the same reasons for which the first distinction has been rejected as the significant one. It might be argued that content is relevant nevertheless (see the discussion of table 12), in that the perspective of lower-class blacks—more so than the perspective of lower-class whites—is such that a higher education is not perceived as really important and for this reason not realistically aspired to. To whatever extent this may be true, it cannot be the significant dimension here, since it does not explain racially patterned differences for the two aspiration indices in *opposite* directions, that is, it does not explain why blacks have higher *self-aspirations* than whites. To follow the logic of the argument, one would have to assume that the perspective of lower-class whites—more so than the perspective of lower-class blacks—is such that the aspirations measured by the self-aspiration index (improvement of income, occupation, and type of residence) are not perceived as really important and are for this reason not realistically aspired to. This argument seems hardly tenable.

The distinction which may account for the fact that the two types of aspirations are not only racially patterned, but that they are so patterned in opposite directions, seems to be related to the third dimension referred to, that between *relative* and *absolute* aspirations.

The index of self-aspirations measures relative aspirations.—Persons

differentially located in the social hierarchy may expect some improvement with respect to income, occupation, and type of residence, without necessarily having the same absolute goals in mind.

The index of aspirations for children measures absolute aspirations.—The respondents' desire and expectations to have their children attend college, that is, a fixed goal level, is involved. The lower a person's current status, and the less his socially defined opportunities for advancement, the higher for him is the reach if he strives for this absolute goal.

These considerations would seem to make it clear why the blacks reduce their realistic aspirations for their children below those of the whites. Their racial status and their socially defined opportunities make the reach for them higher than for whites when a high absolute goal is involved.

If, however, people's objective assessment of their place in the social hierarchy alone would determine their level of aspirations, there would still be no reason for blacks to have higher *self*-aspirations than whites of otherwise similar status characteristics (the argument that was also made to rule out the content dimension of aspirations as the significant one for explaining the racially patterned differences). At this point the two hypotheses suggest a clue.

Hypothesis 2 predicted higher aspirations for the blacks than for the whites on the assumption that the individual will evaluate himself and set his aspirations according to his achievement relative to members of his own racial group who are seen as the "others in the same boat with him." Only the findings on self-aspirations support this hypothesis.

If the relative aspect, that is, the lack of a fixed goal level, is the significant characteristic of self-aspirations, the data in support of hypothesis 2 suggests—to put it more generally—that *in the case of relative aspirations, the individual's position in his own racial group provides the significant frame of reference.*

Hypothesis 1 predicted higher aspirations for the whites than for the blacks on the assumption that the individual in evaluating himself and setting his aspirations will focus on the position of his racial group in the larger social structure. Only the findings on aspirations for children support this hypothesis.

If the absolute goal level implied in the index of aspirations for children is the significant characteristic, the data in support of hypothesis 1 suggest—to put it again more generally—that *in the case of absolute aspirations, the position of the individual's racial group in the larger social structure provides the significant frame of reference.*

If this is so, it raises a further question: why would in one case the individual's position in his own group, and in the other case the position of the individual's group in the social structure, provide the significant social frame of reference?

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The choice of significant frame of reference, it is suggested, is determined by the *type of norms* called into play, depending on whether the aspirations are relative or absolute. In American society open-class norms exist with caste norms (Merton 1957, p. 192). Open-class norms prescribe achievement striving and success goals for all members of society. Caste norms limit the access to these goals for certain minority groups, particularly for blacks.

Absolute aspirations (here aspirations for children) seem to force the individual's attention upon caste norms, since these determine whether or not the individual has easy access to the fixed absolute goal level implied. With the primacy of caste norms, the position of the individual's group in the larger social structure becomes the significant frame of reference. The structure of the situation in this case appears to elicit appraisal of the institutional arrangement rather than self-appraisal.²⁷

Relative aspirations (here self-aspirations), on the other hand, seem not to force the individual's attention upon caste norms, since no fixed goal level is involved, and consequently the question of access is not so salient. Aspirations of this type therefore seem to call open-class norms into play which prescribe success striving for all members of the society. With the primacy of open-class norms, the group's position in the social structure loses its relevance. The individual's position in his own group seems to become significant as a comparative frame of reference, because the members of his own group are presumably the "others in the same boat" relative to whom he evaluates his personal achievement and sets his aspirations.

The two hypotheses which guided the analysis thus led to the suggestion that two intervening variables—norms and social frames of reference—link type of aspirations with level of aspirations and account for the two seemingly disparate patterns. The assumed relationships are set forth in a generalized model as shown in figure 2.

It should once more be emphasized that the conclusions suggested by this analysis are hypothetical. As long as they are not confirmed by direct empirical data they remain speculative. The operation of the two intervening variables, norms and social frames of reference, is inferred rather than factually demonstrated, even though the assumption that either one of the two social frames of reference dealt with would be relevant was made at the outset of the analysis and is not an *ex post facto* conception. The assumption that the distinction between relative and absolute aspirations is the significant one in this context also remains to be verified.

²⁷ The importance of the distinction between an evaluation of the institutional system and self-evaluation of personal achievement within that system has been pointed out by Merton (1957, p. 240).

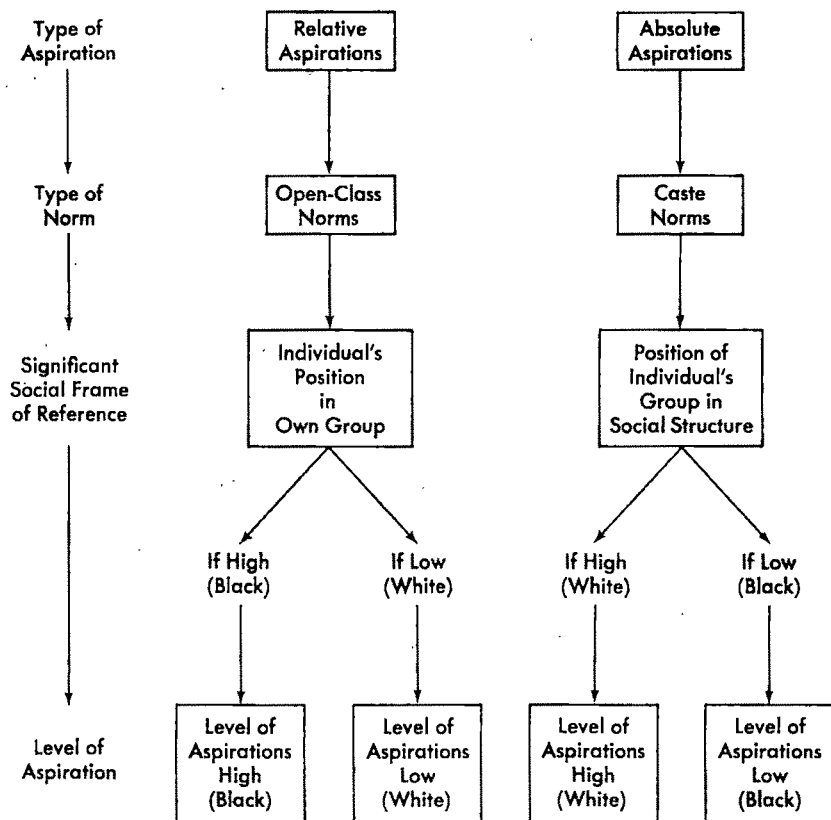


FIG. 2.—Factors affecting levels of aspirations. Designation of race in parentheses refers to the sample on which the present analysis is based. "High" and "low" are used in a purely relative sense.

CONCLUDING REMARKS

It was the explicit purpose of this study to place empirical findings on aspirations of blacks and whites within a broader theoretical context in an attempt to contribute to the understanding of social processes operative in determining levels of aspirations.

The findings and theoretical implications may serve this purpose in several ways. They point to conditions under which specific norms and social frames of reference appear to take on significance in the setting of aspirations. They call attention to different types of aspirations which, it appears, deserve more systematic consideration. They suggest new hypotheses which may be worth pursuing. These new hypotheses, it may be added, need not necessarily be limited in their application to the specific racial groups and aspirations dealt with in this study. They may be tested

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with regard to other salient groups and with regard to other culturally defined aspirations and goals.

Nevertheless, in view of the history of race relations over the past few years and the changing mood among black Americans, a follow-up to the present study—which, it has been noted, is based on data collected in 1960—now or at some future date would certainly be most interesting.

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Contrasting Institutions of Air Force Socialization: Happenstance or Bellwether?

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Two institutions of military socialization, Officers Training School (OTS) and Aviation Cadet Pre-Flight Training School (Pre-Flight), are compared. Light is shed on existing studies which fail to use indicators demarcating basic military subculture. The ACPFTS used harsh techniques to inculcate heroic values and eliminate the "unfit." The OTS uses low-key techniques to presocialize managerial values. The air force is pleased with OTS. It is now the largest source of accessions. Nevertheless, there is a disturbing lack of proof that it produces men who will retain cool judgment in crisis. At the same time, its seeming success challenges the efficacy of existing "harsh" institutions.

I hold no brief for the military, whether it is efficient or not and I would like to see a world in which it simply did not exist. But since that happy day is not exactly imminent, this country should worry a little more about how it picks the [military] men who hold the life and death of the planet in their hands. [MICHAEL HARRINGTON, 1969]

The institutions that assimilate civilians into the military literally raise life and death questions—not only for the initiates but for countless other humans on this planet. Despite the elaborate technology of fail-safe systems, even junior officers have areas of discretionary action with grave implications for world peace. For example, after 18 months training, an air force lieutenant may be piloting a plane with a tactical nuclear weapon larger than those which obliterated Hiroshima and Nagasaki; in the early 1960s, a lone and isolated air force lieutenant was the "custodian" of nuclear weapons in the hands of Turkish military in the eastern Anatolian plateau.¹

Since institutions of military socialization perform the function of initial selection and preparation of men whom it is presumed will not only perform tasks competently but will also retain cool judgment in an intense crisis, the values assimilated, the techniques for imparting them, and changes in such institutions are critically important (Raser 1969).

¹ Conversations with Paul Y. Hammond, 1971.

Until now, there have been few studies of institutions of socialization, and those have concentrated upon the academies.

But it is no longer feasible, in a crisis, to augment a small core of specially socialized, academy-trained professionals by calling up reservists and citizen-soldiers. The force structure has grown in size and complexity; the services (especially the air force) have had to rely upon multiple sources to augment the regular corps and assume (or hope) that, from supplemental sources, new patterns of socialization will produce a sizeable number of officers with a degree of professionalism and a commitment comparable to that imparted by the academies.² Although the academies provide the officers of star and flag ranks, an elite within an elite (Segal and Willick 1968), the fact remains that officers from the so-called supplemental sources make up the great bulk of the corps. Their socialization can be no less important, given the nature of modern weaponry and the responsibilities thrust upon them. The concentration upon academy socialization may be somewhat misplaced.³

PROBLEMS OF EXISTING STUDIES

Studies of institutions of military socialization have been limited, their findings inconclusive, and their conceptualization fraught with problems. One of their unfortunate characteristics is that they have only singled out a few values or attitudes assumed to be associated with military socialization. Usually these have been values assumed to have significance in the politics of civil-military relations. For example, John Lovell (1964) tries to trace the inculcation at West Point of a hard-nosed realist's "strategic perspective" toward international politics that includes such items as expectations about the likelihood of total war. William Lucas (1971) looks for an increased discontent among ROTC cadets over the degree to which government leaders seem willing to listen to military men. Neither find much change in attitudes between freshmen and senior years.⁴ The problem lies in the lack of a more thorough conceptualization which recognizes that we have been looking for the wrong things at the wrong point in an officer's career. First, there must be explicit treatment

² These supplemental programs produce extended active-duty reservists (5-20 years), with some (10%-20%) offered regular commissions.

³ It is not certain that academy graduates are increasing their dominance of flag and star ranks. Segal and Willick (1968) present data for 1951 and 1964 showing an increase in academy graduates for the army and rear admiral ranks of the navy. (Other navy flag ranks have been 100% filled by academy graduates for sometime.) But academy dominance declined in the air force and Marine Corps. The authors dismiss these declines as idiosyncrotic but are less than convincing.

⁴ Dornbush's (1955) study is solely of process, not of the values inculcated. Kaats's (1969) study deals with psychological dimensions such as receptivity to new ideas.

of a point which has so far been only implicitly recognized: the military is a subculture distinguishable from the larger general culture and is characterized by values and attitudes that have only been touched upon incidentally in existing studies. The fundamental traits of the military subculture are introduced early in the career and are more elemental than those for which Lucas and Lovell have been searching. The primary subculture values produce "overt, patterned ways of behaving, feeling and reacting" and rest on "unstated premises and categories" (Kroeber and Kluckhohn 1963, p. 157). These fundamental values are familiar to students of military sociology and can be found in the works of Janowitz (1960, 1964) and the various "officer guides" endorsed by the services.⁵ The problem has been that socialization studies have not pulled these values together in explicit form and recognized that it is these values that are inculcated by primary socializing institutions, and that other more specific values and attitudes with obvious relevance for civil-military relations are added later as the officer's career progresses.

The fundamental values of the military subculture are: (1) acceptance of all-pervasive hierarchy and deference patterns; (2) extreme emphasis on dress, bearing, and grooming; (3) specialized vocabulary; (4) emphasis on honor, integrity, and professional responsibility; (5) emphasis on brotherhood; (6) fighter spirit marked by aggressive enthusiasm; (7) special reverence for history and traditions; (8) social proximity for dependents.

THE APPROACH OF THIS STUDY

Through participant observation, two sharply contrasting "supplemental" institutions of military socialization are analyzed: the now-defunct Aviation Cadet Pre-Flight Training School (Pre-Flight) and the relatively new Officer Training School (OTS). These two programs demonstrate in unusually stark form the contrast between what Lawrence Radway (1971) has pointed out as two dichotomous sets of skills and attitudes required of military leaders. One is useful in battle and puts a premium on "heroic" qualities like loyalty, unity, obedience, hardiness, and zeal. The other is "managerial" in nature and is useful in coping with the larger political and technological environment. The managerial qualities are converse to those valuable in combat. The OTS and Pre-Flight would seem to capture the quintessence of the dichotomy: Pre-Flight strove to produce heroic skills and attitudes to such a degree as to seem caricatured; and OTS seems to go just as far in the opposite direction toward the managerial.

⁵ The air force's version is the *Officer's Guide* (U.S. Air Force 1957).

Following Perrow (1967), the differences between the two institutions will be brought out by an organizational analysis which focuses upon the raw materials each organization works to transform, the techniques utilized, and the perceptions of the raw materials held by the respective staffs.

This analysis of two "supplemental" institutions seeks to offer (1) insights into the functionality of different socialization procedures and processes, (2) a clearer conceptualization of the content of the values transmitted, (3) an increased awareness of the role played by on-the-job performance in socialization, and (4) generally, some further insights into the sociology of the military and civil-military relations.

All institutions of military socialization are incrementally shifting toward the managerial (Radway 1971). But the contrast between OTS and Pre-Flight is so sharp that speculative questions are inescapable. Is this change a carefully planned effort to adapt to changes in the military, the larger society, and the changing nature of the relations between the two?⁶ Is it a change that provides officers with the skills they (and ultimately the nation) need at certain points in their careers? Is it a bellwether change that might set a pattern for other services to follow? Or is it merely a happenstance?

THE TWO ORGANIZATIONS

Pre-Flight was "phased out" between 1961 and 1964 after a long, illustrious history. It began shortly after the United States entered World War I, and thereafter it varied in length and size with the needs of the air corps. During its World War II peak, it produced 114,000 officer-aviators a year. It took civilians (and some enlisted men) with a maximum of a twelfth-grade education and, in a six-month period, produced an advanced cadet ready for flight training and subsequent commissioning. Throughout its history, it had a consistent organizational ethos and related techniques for processing its raw material. In essence, this consisted of the creation of a harsh, structured stress through the format of military training. There seem to be three reasons behind this. First, the program was founded by West Pointers who applied the techniques of the Point: the "brace," demerits, walking "tours," rigid standards, etc. Tradition assured its continuation. Second, those officers responsible for the program, whether they were academy graduates, felt that they were not only producing flyers but also fellow officers and that there was very little time to perform this "miracle." Accordingly, harshness was necessary.⁷ Third, because upper-

⁶ For a discussion of the changes in the military, see Janowitz (1960, 1964).

⁷ See Dean Smith (1961) for a description of the earliest days. Also see Historical Division, Air Training Command (1944, 1945).

classmen actually administered the program, they may have exaggerated what they perceived the West Point system to be.

In the late 1950s and early 1960s, increasingly complex technology and the prospects of a role in space created a special concern among air force leaders for upgrading the educational level of the corps. There was also concern over the inability to meet fluctuating needs for officer requirements. A procurement program was needed that could produce better-educated officers than Pre-Flight but which did not have the two to three years lead time needed in AFROTC.⁸ In 1957, it was first proposed that a program be established that would assure "an adequate complement of academically trained, intellectually disciplined, career officers" by obtaining college graduates "whose major could be closely identified with a needed Air Force specialty."⁹ The source of the proposal is unclear, although General Curtis LeMay is credited with most of the "steam" behind it.¹⁰

The principle of the program was approved by the secretary of the air force in 1958, and the program was developed by officer military schools within the Air Training Command. The program accepted college graduates, most of them without prior military experience, and, in three months, produced a commissioned officer ready for assignment or further schooling. The first class graduated in February 1960.

The two organizations had formal structures that were strikingly similar. Beneath the commanders and supporting staff, the organizations were divided into Academic Training Divisions and Military Training Divisions. In academic training, the basic task unit was the classroom instructor (commissioned officers). On the military side, there were organizations staffed with cadets or officer trainees (O.T.'s) and paralleled by a staff of commissioned officers. But despite these outward similarities, the two organizations differed sharply in their ways of operating.

RAW MATERIALS

Both the O.T.'s and the cadets were pretested and selected volunteers with above-average intelligence and in excellent health. The cadets were single males between 18 and 26, with at least a high school education and averaging about two years of college. The primary motivation for the majority was to fly and, secondarily, to become officers. Their dayrooms were filled with model airplanes, and conversations centered on flying.

⁸ U.S. Air Force, Air Training Command Briefing Documents on file OTS headquarters.

⁹ In OTS documents and papers on file, OTS headquarters.

¹⁰ Correspondence with William R. Porretto, chief, History and Research Division, Air Training Commands. Other memos from LeMay to USAF Directory of Personnel show an overriding concern with improving the level of education in the officer corps.

Although no data is available on the matter, impressionistic accounts and participant observation suggests that they were rather heterogenous in socioeconomic and regional backgrounds, particularly in contrast with O.T.'s.

The O.T.'s included both males and females (7% were WAF). Their ages ranged between 22 and 34 (the average was 23); 21% were married; 8%–10% were enlisted men. A few had M.A.'s, and occasionally a Ph.D. was recruited.¹¹ With the possible exception of the peak years of the Vietnam conflict, the O.T.'s prime motivation was to become an officer and only secondarily to fly.¹² They wished to fulfill their draft-imposed military obligations as officers rather than enlisted men. They tended to see flying as a near anachronism, and they had considerable doubts about the future of manned aircraft in light of the army's preempting close ground support with helicopters, NASA's capture of space missions, and the development of ICBMs. They tended to feel that the more valuable things the air force could provide were junior-executive and technical skills useful in military or civilian life. They appeared more homogeneous and middle class in values and orientations than cadets, although this might be attributable to a common polish or veneer imparted by college.

STAFF PERCEPTIONS OF THE RAW MATERIAL

The staff of Pre-Flight saw their raw material as heterogenous and threatening to the officer corps, an unrefined, inexhaustible mass to be screened and culled. The ethos and informal mission of the organization centered upon elimination of such threats as the potential psychological breakdown under flying or combat stress, the man who might make fatal mistakes, the potential dishonorable discharge, the "coward," all of which represented potential stains on the collective image, integrity, and professional competence of the corps.

The cadets' strong desire to fly was viewed ambivalently, even though the Pre-Flight staff was composed largely of flyers. It was recognized that such a desire had to be present but that unless it was balanced by other factors it could be dysfunctional. Some cadets, it was felt, were drawn to flying largely because of the power and masculinity it symbolized and the compensation it could offer for feelings of inferiority, a belief which

¹¹ In OTS documents and papers on file OTS headquarters.

¹² The war created a need for pilots, and OTS tried to produce them. In 1968, 62% of the O.T.'s were slated for flying. But the staff felt this was an exception to the rule, and the effort was plagued by high rates of self-initiated eliminations among those slated for flying. Probably lower interest in flying and high loss rates for pilots over North Vietnam were at work.

had some psychological foundation (Grinker and Spiegel 1945). Desire to fly in itself could not make a "good officer."

The Pre-Flight staff had also developed an experientially based belief that many cadets were "running away from something," for example, their families, job dissatisfaction, unhappy love affairs, or failure in college. It was felt that some of these men could make good officers but that unless "tested and proven" they might lack the proper qualities. Essentially, they brought nothing to the air force. The air force had to create in them everything it needed.

The OTS staff sees its raw material in a much different light. The O.T.'s come with an ascribed status based on their college degrees and presumed specialized skills (engineering, science, technology). Flight-training officers (FTOs) are fond of saying things like, "My OT squadron commander is a Princeton grad, cum laude. And I have an engineer that helped design the nose cone on the Apollo rocket. You learn from the students all the time."¹³

This attitude persists despite the fact that large numbers, perhaps a majority, of the O.T.'s lack specialized skills and instead are merely college graduates slated for more general administrative and staff positions or for further training in specialties. It also persists in the face of a rather mundane college grade average of 2.4 on a four-point scale for entering O.T.'s.

Still, O.T.'s are seen as a commodity the air force needs; they are a commodity to be processed and oriented as rapidly as possible for their first duty assignment rather than culled and screened. The staff also sees the O.T.'s as older, more mature, sprinkled with veterans, and less willing to tolerate strict discipline and hazing. In point of fact, although they are older than cadets, there are no more veterans among O.T.'s than among cadets. Indeed, they are no older than were the officer candidates of the Officer Candidate School which closed along with Pre-Flight; and both cadets and officer candidates (O.C.'s) tolerated hazing and rigid discipline.

TECHNIQUES FOR PROCESSING THE RAW MATERIAL: PRE-FLIGHT

Isolation from General Culture

When cadets entered Pre-Flight, they were plunged into a completely military environment, and the door was closed on all civilian contacts and preexisting status. With the exception of a few carefully prescribed items, everything from cars to underwear were tagged and stored. For the first month, the cadet was allowed no personal or off-duty contacts with anyone outside his squadron without permission of his tactical train-

¹³ Interview with FTO, 1968.

ing officer (TAC). Later, there would be incremental but carefully defined contacts within the cadet wing. For four months, the cadet's only contact would be within the military subculture. The only exceptions were mail and emergency phone calls through the Red Cross. The isolation was as complete as possible or acceptable in American society and probably exceeded that of many prisons.¹⁴

Shock, Discredit, and Deprivation

From the moment of their arrival, the new cadets, called Raunchies, were harassed by upperclassmen, called White Gloves. These smartly attired and poised tormentors administered savagely impersonal abuse which created feelings of both fear and envy. Raunchies were made to feel fumbling, inept, helpless, and outraged.

These feelings were heightened when their heads were shaved and they were issued an ill-fitting fatigue uniform called "the Green Tux." This same uniform had to be worn all week in a subtropical climate. The result lent considerable realism to the name Raunchie. Feelings of inadequacy were also fostered by physical deprivation, although it was never admitted to be a formal aspect of the program. Marching miles in stiff new shoes, regular calisthenics, and impromptu exercises as physical punishment left them sore and stiff. Meals were stress filled, minutely detailed rituals, and, as a result, food consumption was limited. Sleep was cut as the Raunchies tried to complete their duties after "lights out," and fire drills interrupted their rest. They were constantly and caustically told they were "nothing," "nobody," spastic, slovenly, gross, and, worst of all, "casual."

The process had the effect of screening out those men who it was believed would not be supportive of the subculture's self-image or goals. Raunchies, like all cadets, could leave any time they wished by simply telling their TAC they wished to SIE (self-initiate elimination). The first heavy losses came in the first week. Each class suffered attrition rates of roughly 33% by the end of their training. Those with low capacities for anxiety, insufficient self-esteem to withstand and discredit abuse, inability to control or suppress anger, or those with latent neuroses or psychoses literally "cracked" under the stress. Attempted suicides and psychiatric referrals were not uncommon.

Although they could leave quickly, powerful peer and organizational pressures did not make it easy. Classmates counseled and tried to help in every way possible ("cooperate and graduate"). A request to SIE was

¹⁴ Some descriptions of Pre-Flight exist: Texas Writers Project (1942); Office of Base Historian (1952); Aviation Cadet Regulations (1962, 1963).

met by instant removal from his squadron of the Raunch and all his belongings and his assignment to "casual" barracks. Until processed off base, he cleaned latrines and was treated as a complete pariah.

Directional Cueing to Subculture

Within a couple of weeks, the organization had succeeded in bringing survivors to the lowest common denominator; their self-images discredited and every "spastic" move, display of ignorance, emotion, or weakness attributed to their contemptibly insufficient civilian or enlisted background. At the same time, they had always before them as a model their tormentors, the White Gloves. The latter seemed the epitome of grace, power and self-assurance. Raunchies were thus receptive to anything that might restore self-esteem and make them more like White Gloves.

An impressively strict honor code was introduced which helped develop group cohesion. It called for absolute honesty in all statements and the reporting of others who failed to comply. Raunchies were directed to brotherhood by being told in a variety of ways to find strength to withstand stress, not only from within themselves but from their classmates. They could only meet the impossible demands upon them by close cooperation. Their common misery and despair created a bond not unlike the one Grinker and Spiegel (1945) had found developed in combat.

They were cued to an aggressive fighter spirit by constant exhortations to "Get eager, mister!" or "Get proud, Raunch!" Raunchies were punished for looking "casual." They could not look bored, aloof, or sulky—only eager. Collectively, fighter spirit was manifested in an intense "tiger spirit." Marching was accompanied by roisterous singing, "jody calls," and cadence counts. Fighter spirit (and brotherhood) was also encouraged by intensive intersquadron competition for weekly honors. Competition covered drill and ceremonies, barracks appearance, and athletics. It goes without saying that elements of subculture like hierarchy, dress, and vocabulary were cued and assimilated.

Incremental Returns of Status within the Subculture

After two weeks, the Raunchies were "recognized" as underclassmen and permitted to wear tailored uniforms and shoulder boards similar to those of the upper class. Thus, the first increases in status were within the subculture. As they mastered the physical training, room arrangement, and drill, their confidence and self-esteem rose. That so much indignity and abuse had been suffered to reach these bits of status only enhanced their value. The air force was credited by cadets with having made "new men"

or "better men" of them. Cadets began to develop pride in themselves, their class, their squadron and the air force.

Subculture Assimilation under More Complex Stress

Although the grosser forms of degradation disappeared, the cadets were still under stress. They faced academic difficulties and increasing complexities in military training. The program was intended to place more demands upon the cadets than they could possibly meet. The sanctions for mistakes and failures were severe. Demerits were issued as sanctions. As Raunchies, demerits had been received, but they were "dry runs" without sanctions. But, as underclassmen, each demerit meant an hour of marching on the "tour ramp" in full dress uniform.¹⁵

The cadets were thus faced with more than they could do and with stiff sanctions for failure. They could meet the situation of too much to do in too little time by increasing effort or efficiency, by cooperative efforts and division of responsibility and labor, or by taking calculated risks (C.R.'s). In the latter instance cadets learned to cut corners and take risks by letting some things "slide," while concentrating on others. They learned to balance their performance by concentrating efforts on their weaknesses and giving less attention to their areas of strength.

Some cadets who survived the earlier onslaught of the system could not cope with this more subtle and prolonged stress. Many "toured" each weekend, all the while struggling to avoid more demerits as a result of inattention to duties because of their touring. Some found the milieu of cross-pressures too much to bear. Those adapting successfully, however, expressed gratitude to the system for showing them how far below their intellectual and physical capacities they had normally operated.

Those who were adapting and assimilating the subculture also developed a different attitude toward hazing and the upperclass. No longer did they feel upperclassmen were "out to get them." Instead, they saw them as playing a role demanded by the system—a system in which they both now believed. Hazing was still stressful and involved demerits, but now a certain amount of humor was seen in what was formerly viewed as torture. Cadets referred to hazing at this point as "Mickey Mouse" or "f and g" (fun and games). Many said they "couldn't have made it if it had not been for the "Mickey Mouse." A typical game was "flying the spad" in which an underclassman was placed at attention with his nose pressed against the hub of a whirling ventilator fan, wearing an old ath-

¹⁵ To obtain a full comprehension of the restrictions on a cadet's behavior, the reader would have to see the 11-page delinquency and award list. Examples: hands, i.e., in pockets—five demerits; carelessness, i.e., gross-commander's investigation; escorting, i.e., lady, showing affection (in public)—five demerits.

letic supporter as an oxygen mask, while an upperclassman squirted shaving cream through the blades. Underclassmen could respond to hazing by attempting to make upperclassmen "blither" or break out laughing. They also took turns "running a C.R. day," during which they tried to wear their belt backward, fly open, or name tag upside down without being caught.

Opportunities to Alternate Roles

Training as underclassmen culminated in "turn-around day," when lower-classmen assumed all the responsibilities and privileges of the upperclass. Dornbush (1955) labeled this phenomenon as one that teaches "relativity of status." The ritual symbolized that both classes were part of the same brotherhood, the lowerclass was competent to succeed the upperclass and to pass on the subculture to new Raunchies. It was also symbolic recognition and preparation for a real career possibility—that some of the lowerclass would eventually outrank their seniors.

Some have dismissed these techniques for processing the raw material as a discharge of pent-up sadism (Mullan 1948). But this would not take into account the institutional function performed. It would also give a psychological interpretation to what is primarily a social process (Dornbush 1955). Upperclassmen often expressed their distaste for "White Gloves duty," but all performed it with zealous craftsmanship. Though dissemblance is possible, it is more likely that they had grasped the institutional rationality for the process and the techniques.

TECHNIQUES FOR PROCESSING THE RAW MATERIAL: OTS

The techniques for processing O.T.'s is sharply different from that for Pre-Flights. Even before they arrive, the O.T.'s have received different treatment. In return for volunteering, they are given one of the rarest gifts of military bureaucracy—orders indicating the next duty assignment and career field. The O.T.'s probably regard this as merely common sense; in the eyes of the staff, it is a radical and impressive departure on a matter that is the constant preoccupation of military men.

Transition, Not Isolation

Rather than traumatically severing the O.T. from civilian life and isolating him behind a "khaki curtain," as Pre-Flight did, OTS strives for an "orderly transition" to military life. The O.T.'s are not encouraged to bring wives and families, but it is not forbidden, and so many do that a "wives club" has developed. Visitors, autos, and personal effects are per-

mitted. (Clothes closets have civilian clothes hanging with uniforms—something absolutely unthinkable at Pre-Flight.) The furthest step toward “isolation” is restriction to base and a prohibition against driving for three weeks. This merely means O.T.’s must travel by base shuttle bus and are “confined” amid six movie theatres, O.T. club with pool and bar, a hobby shop, three department store-like PX’s, picnic grounds, three ice-cream shops, two libraries, a museum, roller rink, bowling alley, and two gymnasiums.

The first-class O.T.’s play a part in training, but it is drastically limited. The positions they assume are seen as laboratory situations to develop leadership skills instead of vital roles in the socialization process as was the case at Pre-Flight.

A Realistic Model of Authority

From the moment of his arrival, an O.T. is dealt with solely by commissioned officers. The FTOs give orientation briefings and instruction in basic elements of drill and room arrangement. Such tasks are generally considered somewhat menial and left to noncommissioned officers (NCOs) or, as at Pre-Flight, to upperclassmen.¹⁶ The FTOs have impressed upon them the importance of keeping their college-trained charges in contact with commissioned officers they can emulate. They can never “chew” or “stress” the O.T.’s but must deal with them as managerial superiors dealing with subordinates. As one FTO put it, “I’m sometimes torn between treating them decently or rough as used to be done in the old days in other programs. If it came to a choice of treating them rough, or as one realistically treats a subordinate—I would do the latter, to teach them awareness and consideration.” Another FTO put it more bluntly, “If we don’t treat them the same way we do our own subordinates—how are they going to learn?”¹⁷ Officers and O.T.’s are brought together in informal seminars on military life, career orientation briefings, and informal club gatherings.

After initial processing by commissioned officers, the second-class O.T.’s (note the adjectives “upper” and “lower” were expressly forbidden) are given further instruction by first-class O.T.’s but always under careful supervision. First-class O.T.’s are graded on their ability to handle their charges in the same managerial style that commissioned staff have used.

¹⁶ It may be slightly overstating it to say such tasks are seen as menial. An officer may take pride in his ability to perform them. Usually, however, an officer does not engage in these activities often enough to perform them with greater skill than an NCO. Accordingly, they are left to NCOs, and the slight disdain for the tasks may be a defensive rationalization for low skill level.

¹⁷ Interview with FTO, 1968.

Uniform differentials between classes which had been purposely exaggerated at Pre-Flight are minimized at O.T.S. The uniforms of the two classes are identical from the outset except for the color of the shoulder boards. The informational brochure sent to incoming O.T.'s seeks to answer typical questions. Its answer to the question, Will I be subjected to hazing? is particularly revealing: "No! Hazing is prohibited at OTS. Hazing is defined as any unauthorized assumption of authority by one trainee over another trainee whereby the latter shall be exposed to suffer any cruelty, indignity, humiliation, hardship, oppression, the deprivation or abridgement of any right, privilege or advantage, to which he shall be legally or properly entitled. Each trainee is expected to act as a gentleman, and to treat his fellow trainees as such."¹⁸

One of the school's first internal crises involved a second classman who was shoved into a shower by friends with his clothes on. In the process, he sprained his ankle. Staff officers were sternly reprimanded; and participants, restricted to dormitory (never referred to as barracks) and classes for a week.

Demerits as Teaching Devices

A demerit system exists at OTS, but any resemblance to Pre-Flight is purely coincidental. Instead of heat-stroke-inducing marches on a tour ramp, a second-class O.T. with 10 demerits is restricted to base, where he has access to the rather fullsome list of diversions noted above. If he receives over 17 a week, he is confined to the immediate training area which only contains a club, pool, gym and library. For a first classman, the respective limits are five to 10 demerits per week.

In no way could the system be construed as applying stress in order to "screen out" individuals unable to adapt or assimilate the subculture. An FTO who had once been an O.T. himself stated, "We try to introduce them to the military gradually. We weren't under much pressure when I came through and they aren't now. They don't have any spare time but that's about the biggest factor they face."¹⁹ Whereas Pre-Flight had as a major informal goal the elimination of a sizable number of cadets, OTS was ambiguous if not anguished on this point. The OTS staff recognizes that not all trainees can successfully complete the program. Yet, from its inception, OTS has manifested considerable internal anxiety and tension over SIEs. This had only slightly abated at the time of a second ob-

¹⁸ Headquarters, Officer Training School (1968), p. 23. The definition of hazing is standard, indeed statutory. Pre-Flight, however, would never deny that it took place, and its regulations carefully followed the definition with the point that "strict discipline" and "intense training" was not hazing. The difference, of course, is unclear.

¹⁹ Interview with FTO, 1968.

servation in 1968. The SIEs or staff recommendations for elimination were routine at Pre-Flight, but at OTS they still have aspects of a "flap." Although the attrition rate in 1968 was only 10%, compared with 33% at Pre-Flight, it was still a matter of grave concern. The prevalent feeling is that the O.T. is rejecting the air force rather than vice versa. Extensive reports have to be filed, the O.T. has to be counseled at length by several levels of authority, boards must be convened for hearings, and anxious informal inquiries come from upper echelons. Even though many of the same procedures were followed at Pre-Flight, the spirit surrounding them was vastly different. There are half-jesting remarks among the OTS staff that recommending an O.T. for elimination is not worth the extra work and that "you would lose anyway."

Assimilation of Subculture?

At Pre-Flight one could clearly see a cuing to elements of a subculture, changed behavior, and verbal expression of new values, all of which led to presumption of subculture assimilation in line with well-defined concepts from social psychology and small-group theory. But at OTS it is less clear what is taking place. In fact, that lack of clarity may be informative concerning existing notions about military socialization as well as raise some serious questions about them.

At OTS there is changed behavior and verbal expression of changed values, but this is definitely muted compared with Pre-Flight. The subculture element of honor is introduced in the form of a code but given relatively little emphasis. It is taught as part of a more general course titled "office responsibilities." A cadet honor board at Pre-Flight could dismiss anyone found guilty of a violation. But at OTS the matter is handled by the commissioned staff, and an O.T. found guilty can only be dismissed by the commander. In one instance, in the early years, a "convicted" violator was merely held over, placed in another class, and allowed to graduate later. One does not hear from O.T.'s expressions of enthusiastic appreciation and wonder at the working of an honor code as was the case at Pre-Flight.²⁰

Although O.T.'s come to manifest a general enthusiastic "can do" spirit, fighter spirit in the form of cocksure aggressiveness is difficult to see among O.T.'s, whereas it was blatantly manifest among graduating cadets.

²⁰ As for that aspect of honor which holds that an officer always fights, OTS, like other institutions of socialization, has had its problems. For example, an OTS graduate refused to arm and load nuclear weapons. Cadet Pre-Flight did not have such problems, but there were too many intervening variables to say that the OTS socialization process failed to instill that aspect of honor. The OTS operates in an era when attitudes toward the military and war have changed sharply. Furthermore, a reluctance to fight was easier to camouflage as a reluctance to fly at Pre-Flight.

Where Pre-Flight had resounded to singing, the silence of OTS was broken, only by methodical and business-like footfalls and an occasional half-embarrassed, laconic cadence count. Competition, which had been the hallmark of Pre-Flight, was expressly eschewed at OTS. In the early years of OTS, one FTO insisted that his squadron "beat the ass off all the others," and fostered distinctiveness by calling his squadron "Big Red One," having it outfitted for physical training in red T-shirts and caps and by applying red paint to parts of the dormitory. His actions threw the Military Training Division of OTS into turmoil, because such action had not been forbidden in the written regulations and because his frowned-upon tactics were decidedly successful in improving his squadron's performance. He was eventually transferred.

The sharply contrasting techniques developed by Pre-Flight and OTS for processing their raw material are reflected in their contrasting assigned and published "missions." Pre-Flight's formal mission statement spoke merely of "receiving and training pilot and observer (navigator) aviation cadets as junior flying officers." The mission document of OTS, however, speaks of "training *selected* personnel to meet the *fundamental* requirements for newly commissioned officers." The mission of OTS also refers pointedly to "fundamental requirements" for "newly commissioned officers," an emphasis that received operationalization in a commonplace expression by staff members: "We're training 2nd Lieutenants for their first assignment and first six months. We're not training future air commanders. We expect the field to take him from there."²¹

Air force units characteristically spell out more specific objectives under their general mission. Pre-Flight revealingly listed as its first objective the "instilling of military discipline and qualities of leadership." It went on to list "furnishing a cadet training in the development of personal discipline by *regulating his life rigidly* through cadet customs as *implemented by the class system.*" The OTS, on the other hand, significantly lists as its first three objectives: (1) meeting USAF manpower requirements, (2) *providing orderly transitions from civilian to military life*, and (3) *initial processing.*²²

CONSEQUENCES: INFERENCES AND CONUNDRUMS

By any measure, OTS has been an unqualified success in the eyes of the air force. It has consistently monitored the reception of its products by the commands to which O.T.'s have been assigned.²³ It has found its

²¹ Interview with FTO, 1968.

²² All the above material is quoted from OTS documents, 1968; and from Office of Base Historian (1952) (italics added).

²³ By 1968, it had made six field-evaluation visits to "user commands."

graduates have higher retention rates than AFROTC products (50% vs. 30%–31% for AFROTC); 78.8% were offered regular commissions in 1966, and 92.6% of them accepted. Supervisors of graduates find them highly acceptable and compare them favorably to products of AFROTC and the Air Force Academy. Effectiveness reports bear them out. In 1970, OTS produced more than all other sources of "line" officers combined.²⁴ Its graduates are the largest single source of commissioned officers, supplying a little over 50% of the accessions in 1970.²⁵

It would be satisfying to conclude that the leadership of the air force made a calculated change in its socialization processes as a result of new officer requirements produced by changes in the military that Janowitz (1960, 1964) and others have described. Unfortunately, no such clear and comforting picture or rationality emerges. The design of OTS is at least partially a result of idiosyncratic and situationally specific variables. In early 1958, the office of air force deputy chief of staff for personnel passed on to officer military schools for comment informal information on the proposed OTS. The comments both levels made on the proposal make it clear that they were thinking of an orientation and processing course for specialists like those run at that time for chaplains, medical doctors, and legal officers. It was expected that the first year's production would include large numbers of specialists in such fields as armaments electronics, communications electronics, electronic data processing, etc. Indeed, in various items of correspondence, it is referred to as the Supplemental Officer Procurement Program.²⁶ It would be only natural, therefore, for those charged with developing the program to think of a low-key, nonrigorous, processing program. Later, when some of the officer military schools' staff were specifically assigned the task of drawing together detailed plans for the new school, they no doubt felt constrained to produce something like a "Supplemental Officer" approach. No doubt they also felt the need to design a program that contrasted with the approach of Pre-Flight and OCS, the two programs then in existence for non-college graduates. To be different, the gestating OTS program would have to be less harsh and rigorous, for it could not surpass OCS and Pre-Flight on that score.

²⁴ "Line" or command officers are distinguished from "staff" officers like chaplains, attorneys, and medical doctors.

²⁵ Data from the Office of Assistant Secretary of Defense for Manpower and Reserve Affairs (1969). The OTS production obtained by including 225 WAF.

²⁶ This information, from OTS files. The informal information came from Air Force Personnel Procurement and Training Center (deputy chief of staff personnel). It was passed through Air Training Command, and 3700 Basic Training Wing to officer military schools. That communication is apparently unavailable. The files at OTS, however, contain the comments and reactions of officer military schools' staff to the deputy chief of staff personnel correspondence.

The available evidence suggests that only in the most general way was the switch from Pre-Flight to OTS calculated and planned with a clear means-end relationship in mind. Whether the methods developed at OTS will select and prepare leaders who "will retain cool judgment in an intense crisis" remains an open question. In point of fact, there was never satisfactory proof that the methods of Pre-Flight, so incredibly costly in human terms, were successful in selecting and preparing men for crisis.²⁷ Thus, the air force abandoned one process for producing "supplemental officers" that had never really been subjected to thorough examination in terms of its efficacy, and it adopted another that was equally unexamined. Viewed in the light of the awesome weaponry these supplemental officers will control and the responsibilities they will shoulder, this half-planned, half-accidental means of developing an institution of socialization is less than reassuring. The fact that OTS is producing over half the input of officers into the corps is also unsettling.

What does this comparative case tell us about military socialization? For better or worse, it raises more questions than it answers. First, it bears repeating that existing socialization studies have persistently rested on indicators that do not necessarily demarcate the military subculture as distinct from the general civil culture. Unfortunately, the indicators for which analysts have tested are in some ways related to the military subculture but are not central to it. The indicators for which we must test are the things that mark the military officer from the civilian, the sorts of things imparted by institutions of military socialization like acceptance of all-pervasive hierarchy and deference patterns, acceptance of extreme emphasis on dress and bearing, honor, brotherhood, fighting spirit, etc.

The items for which Lovell (1964) and Lucas (1971) were testing emerge over years of professional task engagement (as Lucas found in responses from officers at the National War College). Small wonder then that both analysts found little change in outlooks of either West Point or ROTC cadets as they proceeded through training. Lovell reacts to the findings by suggesting that the socialization process is becoming more diffuse and extending into the career years. Lucas suggests the lack of change is attributable to "anticipatory socialization" that occurs before enrollment in ROTC. No doubt, there is some truth in each suggestion. But it is also possible that the lack of sharp change lies in the failure to focus on values more central to the military subculture.

But what can be said of the sharp difference between Pre-Flight and OTS? Even without data from questionnaires, it seems inescapable to the

²⁷ Grinker and Spiegel's (1945) extensive studies during World War II did not present data that controlled for commission source, but studies involving many Pre-Flight products led them to conclude that there is no way to select men who will not "break" under combat stress.

participant-observer that socialization occurred at Pre-Flight. The transformation in behavior and expressed values was too great to leave any doubt. It should be noted again, however, that the transformation rested on adoption of the primary values of military subculture, not on things like strategic perspective or career commitment. But one conclusion that does emerge from a comparative analysis is the lack of a clear utility for Pre-Flight's intense socialization. That socialization process was brutally expensive in human terms and produced exaggerated forms of behavior which were not clearly related to effective task accomplishment. In reaching a decision to terminate the program, it is not clear which of these factors were given consideration by the air force or how they were weighted. The only known fact is that air-force leadership felt that more highly educated officers were needed. But regardless of how well informed and logical that decision really was, the comparative analysis of the two programs should be suggestive for other branches of the armed forces. To be sure, there are differing service requirements and, accordingly, institutions of socialization might reasonably vary in order to meet them. It may indeed be quite functional to have the Platoon Leader School of the Marine Corps differ from OTS. But other services might do well to go further than the mere incremental "softening" of their programs as is currently in progress. The success of OTS makes it eminently reasonable to suggest that all programs in the Pre-Flight style be critically reexamined. It may be possible to get as good or better a product at lower dollar and human costs.

The socialization process at OTS is so much more low-key and subtle than is found elsewhere that a more powerful method than participant-observation is required to determine the extent of behavior and value change. Apparently, there takes place a form of preparatory or presocialization processing on the assumption that the "career field" or task will complete the socialization process.²⁸ That assumption would seem to be borne out by the acceptability of the product and the high retention rates of OTS graduates. Task requirements may do more to socialize than has heretofore been assumed. In other words, a man put through a Pre-Flight type program and assigned to accounting would maintain manifestations of the basic military subculture but would at the same time quickly forsake the more exaggerated forms of behavior he had learned and adopt those more functional to accountancy. Similarly, a man put through OTS and assigned to jet fighters might quickly adopt the aggressive stance of a "jet-jock." If this is so, then we are past due for a more searching examination of institutions of military socialization if we are to develop and maintain institutions that are of lower human and financial cost or which will pro-

²⁸ There is also probably some "anticipatory socialization" at work.

duce men that can "retain cool judgment in a crisis" in the era of technological warfare.

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The Impact of Military Service on Authoritarian Attitudes: Evidence from West Germany

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A study by Campbell and McCormack in 1957 of the U.S. Army was replicated 10 years later in the West German Army. Age and education were controlled, and the possibility of a panel effect was checked, and the earlier finding that military experience reduces authoritarianism of draftees was confirmed. This study also investigated the permanence of the observed effect. The effect was a very temporary response to the specific military-life situation. An interpretation is offered that the experienced deprivation increases the sensitivity of soldiers to those items in the scale indicating respect and tolerance for fellow citizens.

There are some widely held prescientific notions about an affinity between authoritarian and military attitudes. Studies within the military have shown that, when recruits were asked to guess responses of typical NCOs to an *F*-scale (Roghmann 1966, p. 278), or when the *F*-scale was declared a leadership aptitude test (Hollander 1954), or when recruits had to give their name instead of answering anonymously (French and Ernest 1955), they consistently had higher scores than when they reported their own view to the various *F*-scale items in a proper test situation. These notions also found their way into the scientific literature as hypotheses in various studies (Christie 1952; Campbell and McCormack 1957; Firestone 1959; Roghmann 1966), but the results thus far remain inconclusive as contributions to the study either of authoritarianism or of military organizations.

The inconclusive knowledge about this particular question may come as a surprise. Certainly, the notion is of "relevance," that is, it should be of political concern to sociologists as well as political scientists. If such an affinity between authoritarian and military attitudes were to exist, through selective recruitment, socialization, selective promotion, or other mechanisms, it would have considerable political implications. Civilian control over the military would be more difficult to achieve. Correct treatment of subordinates would be more difficult to guarantee. Respecting the rights of enemy soldiers and noncombatants would be more difficult to ensure. Military thinking, which traditionally has a large influence on foreign

policy, might distort relations with other countries. In short, there are sufficient reasons why a closer examination of the relationship between authoritarian and military attitudes is desirable.

OBJECTIVES

The question of the relationship between authoritarian and military attitudes has to be specified to be accessible to empirical test. There is no doubt, as the studies cited above indicate, that the military is perceived as being authoritarian. This perception probably reflects recognition of the rigid rank and command structure typical of military organization. The question which should concern us here is to what extent the organizational structure actually leads to authoritarian attitudes as operationalized in the *F*-scale or *D*-scale (Adorno et al. 1950; Rokeach 1960). Does military organization inadvertently reduce belief in the values it is supposed to defend in democratic societies? Short-term panel studies could not show such an effect. A panel study of 186 air force soldiers revealed no significant changes over a period of six weeks (French and Ernest 1955). A survey of 500 marines resulted in a similar negative result (Firestone 1959). Christie (1952) showed a small increase in the scores of infantry soldiers over a six-week training period. The only long-term panel study, however, did not confirm this finding. Campbell and McCormack (1957) showed a significant decrease in *F*-scores over a one-year period. This result is consistent with their report of a negative correlation between military rank and *F*-scores and between length of service and *F*-scores. This negative correlation was confirmed for West Germany by Roghmann (1966) in a secondary analysis of survey data initially gathered by Waldman (1963, 1964). The longer the previous period of service and the higher the rank, the lower were the *D*-scores.

However, this finding might be spurious. It has been shown (Roghmann 1966, pp. 254-89) that authoritarian personalities have a stronger preference for the military. We know that military men tend to be more conservative than other occupational and professional groups (Abrahamsson 1970). Only one of the studies quoted above used civilian control groups. Is it possible that the decline of *F*-scores only reflects a normal maturation process of young males, also observable outside the military? Is the correlation with rank and length of service due to differences in educational background? There is strong evidence of a relationship between age and authoritarianism as well as between education and authoritarianism. Military rank correlates with military experience as well as with education. The finding from the only long-term panel study might reflect a panel effect. Why should there be a negative correlation once we control for age and education? What mechanism could bring about such a change? Waldman

(1963) argues, for the West German Army, that political education in the form of the "Inner Guidance" program is such a mechanism. However, Roghmann (1971) claims that the program was a failure.

A more rigorous study had to be conducted to assess the pure effect of military socialization on authoritarian attitudes, controlling for age and educational background, and also to give some insight into the mechanisms possibly involved. If the reported negative relationship persists, what does it tell us about the political socialization or education of military personnel? The question about the mechanisms forces us also to examine the concept of authoritarianism as it survived nearly 30 years of empirical research. Are we in fact dealing with permanent attitudes or personality structures, or are responses to *F*-scale or *D*-scale items reflecting social attitudes that can be affected by short-term processes? What is the stability of these scale scores over time? What is the relationship of these scale scores to political behavior? While there has been extensive discussion of the changing nature of authority and organizational structures in the military (Janowitz 1959, 1964), changes in authoritarianism in individuals due to organizational contexts have received less attention. Attempts to identify the structure of the *F*-scale or of the authoritarian personality have not been successful (Roghmann 1966, 1970). Results from some empirical studies (Roghmann 1966, pp. 278-300) indicate that situational factors and short-term social processes may indeed determine *F*-scores. If this is the case, it would not be useful to employ these scores as independent variables to explain political behavior but, rather, to regard them as measuring one aspect of political behavior. A careful study of item responses over time in the military may then not only tell us something about the relation between authoritarian and military attitudes, but about verbal political behavior as expressed in the *F*-scale or *D*-scale in general.

PROCEDURES

Main Study

A panel study¹ was conducted covering 12 companies of the West German Army. The study followed the soldiers from basic training to shortly before their discharge some 18 months later. Questionnaires were administered at three points in time, with a 12-item authoritarianism scale included at the first administration (October 1966) and at the third administration (March 1968). A total of 1,590 soldiers were interviewed in the first wave, when they were gathered in the basic training camps. Only

¹ The survey data used in this study were gathered by the Military Sociology Research Group, Institute for Social Research, University of Cologne, in cooperation with and under contract to the West German Office of Defense, Bonn.

1,149 could be reinterviewed in the third wave, when they were dispersed for special training to bases all over West Germany. Complete data sets for both scale administrations were available for 1,037 soldiers. Dropout reasons were unrelated to the content of the scale. This sample of draftees in peacetime may or may not be representative of a volunteer or wartime army.

The operationalization of authoritarianism relied more on the dogmatism scale (Rokeach 1960) than on the fascism scale (Adorno, Frenkel-Brunswick, Levinson et al. 1950) because the latter is highly loaded with time-space-specific content. The scale used had been developed as a short version (12 items) of the dogmatism scale, with one *F*-scale item, after extensive analysis of over 4,000 interviews conducted in the summer of 1960 in the military and of about 1,700 interviews conducted in the fall of 1960 in the civilian population, using a translation of the original 40-item dogmatism scale. This 40-item scale was later reversed, together with a 22-item *F*-scale, and tested on a student population (June 1962). The short 12-item version consisted of nine positive and three negative items and had previously (January 1964) been tested in a one-company ($N = 108$) study in the West German Army. Respondents were permitted two choices of intensity of agreement or disagreement (strongly or somewhat). Further details have been reported elsewhere (Rogmann 1966, p. 259).

Correlations between items as well as the total score over the 18-month span reflect test-retest reliability as well as attitude change. Table 1 presents results for the correlations of the items with themselves over the two administrations and the item-total correlations at each time, corrected for spurious overlap (Guilford 1954, p. 439). Two findings are striking. First, all the correlations over time between items were positive and highly significant. Correlation coefficients ranged from $+.198$ to $+.563$. In spite of the measurement error involved and the attitude change that did occur, there is, for individual items, a remarkable stability over time. Second, the item-total correlations of the positive items were all positive and significant. The rank order of all the item-total correlation coefficients is nearly the same for the two points in time.

The reversed items made, both times, no significant contribution to the overall score. It makes no difference whether they are included or excluded in the scoring. This was expected and confirms earlier findings about the discriminatory power of negative items. The function of the three negative items in the scale was to detect and assess response set.

The uncorrected² test-retest correlation of the total scale was $r = +.563$, which is fairly high if we consider the low split-half correlations at each

² No corrections, either with the Spearman-Brown formula or for attenuation, have been applied to the correlation coefficients.

TABLE 1

CORRELATIONS BETWEEN ITEMS OVER TIME AND BETWEEN ITEM AND TOTAL SCORE
AT EACH TIME, WEST GERMAN ARMY, 1966-68 ($N = 1,037$)

| IDENTIFICATION | | SHORT VERSION | CORRELATION OVER TIME: FIRST WITH THIRD INTERVIEW | ITEM-TOTAL CORRELATION | |
|----------------|-----|--|--|---------------------------|--------------------|
| Old* | New | | | First Interview | Third Interview |
| D35 | 1 | In religious matters, no compromise | + .382 | + .354 | + .366 |
| D30 | 2† | Believe in great cause | + .245 | — .067 | — .003 |
| D14 | 3 | Rely on leaders | + .258 | + .260 | + .301 |
| D32 | 4† | Only one correct philosophy | + .340 | + .089 | + .050 |
| D9 | 5 | Public attack of friends worst crime | + .357 | + .288 | + .315 |
| F1 | 6 | Obedience and respect | + .563 | + .412 | + .446 |
| D47 | 7 | Anxiety about the future is natural | + .407 | + .288 | + .297 |
| D33 | 8 | Enthusiastic people are wishy-washy | + .331 | + .299 | + .349 |
| D15 | 9 | Reserve judgment | + .255 | + .133 | + .225 |
| D31 | 10† | Devote life to great idea | + .198 | — .037 | + .021 |
| D24 | 11 | People don't know what is good for them | + .230 | + .237 | + .246 |
| D43 | 12 | Man is helpless creature | + .427 | + .301 | + .327 |
| Test-retest | | Odd halves | + .485 | Not applicable | |
| | | Even halves | + .486 | | |
| | | Plus parts | + .566 | | |
| | | Minus parts | + .328 | | |
| | | Total scale | + .563 | | |
| Split-half | | Odd-even | Not applicable | + .353 | + .376 |
| | | Plus-minus | | — .091 | — .072 |

* Numbering corresponds to Rokeach (1956) for *D*-scale items and Adorno et al. (1950) for the *F*-scale item.
† Reversal.

administration. The odd-even correlations are low because the reversals, all in the even half, were included in the score computations. Though the scoring for the minus items was reversed, we find a slightly negative reliability coefficient instead of the desirable positive one for the plus-minus correlation. As a result, the test-retest correlation for the positive part (nine items) is higher ($r = .566$) than for the total scale (12 items), in spite of its shorter length.

Control Studies

In October 1968, the political views of members of a German Catholic fraternity were studied (Bartcher 1970). The same authoritarianism scale

with 12 items mentioned above was included, but with three intensity grades (strongly, moderately, slightly) of agreeing or disagreeing. A total of 2,247 questionnaires were mailed, 1,631 (73%) were returned, and 1,510 (67%) had complete information. Only fraternity members up to age 33 ($N = 901$) will be analyzed here in order to have a comparable age range for the army study.

A panel study of political attitudes of West German students (first interview, January/February 1968; second interview, June/July 1968, random sample of all registered students) served as a second control group (Kaase 1971). A complete data set was available on 959 students. A 14-item authoritarianism scale was used which was developed from the same item pool from which the 12-item scale was constructed.

Though the three studies are not completely comparable as to age range, educational level, and authoritarianism scale, they are sufficiently alike to answer a variety of questions. To achieve comparability of scale scores, the same treatment of missing values and score computation was applied. For each study, the overall distribution of scores (first interview in panel studies) was divided in quintiles. The cutting points in the scores thus established were then adhered to for subgroup analysis or groupings of later interview results in panel studies.

RESULTS

Change in Scores

There was a significant decrease in authoritarianism, over the 17-month period of service in the army, on all nine plus items (see table 2). The greatest decrease was for the well-known "obedience and respect" item from the original *F*-scale, with which there was a tendency to agree when recruits entered the army and a tendency to disagree at the end of service. The mean response to all 12 items (2.42) was close to the neutral point at the beginning, but dropped to a general rejection of the items (2.27) at the end.

The reactions to the three reversed items are of special interest. There was a modest and partly significant increase in authoritarianism with these items. In the first interview, responses to these three items showed the lowest authoritarian values; they were the items worded to express tolerance which the respondents felt were most acceptable and which they endorsed strongly. All the other items expressed some intolerance. In the last interview, the respondents more generally disagreed with all items. Their endorsement of the three reversed items was no longer as strong as before. If there was any acquiescence response set which inflated the scale scores at the first administration, it was certainly reduced at the second

TABLE 2
RESPONSES TO 12-ITEM AUTHORITARIANISM SCALE OVER A 17-MONTH INTERVAL, WEST GERMAN ARMY, 1966-68 ($N = 1,037$)

| IDENTIFICATION | SHORT VERSION | FIRST INTERVIEW | | THIRD INTERVIEW | | CHANGE | |
|----------------|------------------------|--------------------|-----------------|-------------------|-----------------|--------|-------|
| | | \bar{X}_1 | SD ₁ | \bar{X}_3 | SD ₃ | d | t |
| 1 | In religious matters | 2.195 | 1.162 | 2.093 | 1.068 | -0.102 | 2.084 |
| 2* | Great cause | 1.961 | 1.002 | 2.001 | 0.945 | +0.040 | 0.940 |
| 3 | Rely on leaders | 2.777 | 0.914 | 2.569 | 0.817 | -0.209 | 5.502 |
| 4* | One correct philosophy | 1.731 | 0.870 | 1.824 | 0.814 | +0.093 | 2.523 |
| 5 | Attack of friends | 3.044 | 1.031 | 2.738 | 0.971 | -0.306 | 6.975 |
| 6 | Obedience and respect | 2.656 | 1.103 | 2.222 | 1.060 | -0.434 | 9.162 |
| 7 | Anxiety about future | 2.314 | 1.035 | 2.199 | 0.947 | -0.115 | 2.647 |
| 8 | Enthusiastic | 2.544 | 0.987 | 2.305 | 0.903 | -0.239 | 5.761 |
| 9 | Reserve judgment | 2.894 | 1.006 | 2.724 | 0.916 | -0.170 | 4.035 |
| 10* | Great idea | 1.759 | 0.871 | 1.842 | 0.776 | +0.084 | 2.218 |
| 11 | People don't know | 3.001 | 0.841 | 2.762 | 0.797 | -0.239 | 6.645 |
| 12 | Man is helpless | 2.107 | 1.062 | 1.939 | 1.042 | -0.167 | 3.627 |
| Total | | 28.983 (2.415)† | 4.941 | 27.219 (2.268) | 4.864 | -1.764 | 8.209 |
| Odd | | 16.226 (2.704) | 3.231 | 15.086 (2.514) | 3.096 | -1.140 | 8.212 |
| Even | | 12.757 (2.126) | 2.768 | 12.134 (2.022) | 2.765 | -0.623 | 5.138 |

NOTE.—Values above 2.50 indicate tendency to agree with authoritarian statement; values below 2.50 indicate tendency to disagree. Range: 1.00 (lowest possible) to 4.00 (highest possible).
* Reversed items, but scoring takes reversal into account.
† Mean in parentheses.

Authoritarian Attitudes in the Military

administration or had turned into a disagreement response set. But accepting the hypothesis of a reduced-acquiescence response set does not preclude also accepting the reduced-authoritarianism hypothesis. The change in response set, if it indeed occurred, would only account for a small fraction of the reduction of authoritarianism.³

Using the quintile groupings, table 3 describes the change from time 1 to time 3 in tabular form. The number of soldiers in the highest group was reduced by nearly 50% at the second administration; the number in the lowest group increased by more than 50%.

TABLE 3

CHANGE IN AUTHORITARIANISM SCORES OF DRAFTEES FROM THE BEGINNING TO THE END OF THEIR SERVICE (17 MONTHS APART), WEST GERMAN ARMY, 1966-68 ($N = 1,037$)

| AUTHORITARIANISM QUINTILE, THIRD INTERVIEW | AUTHORITARIANISM SCORE, BY QUINTILE, FIRST INTERVIEW* | | | | | Row Sums | PER- CENTAGE |
|--|--|-----|-----|-----|----------------|-------------|-----------------|
| | 1 (Lowest) | 2 | 3 | 4 | 5 (Highest) | | |
| 1 (lowest) | 135 | 91 | 52 | 28 | 17 | 323 | 31 |
| 2 | 37 | 65 | 59 | 46 | 22 | 229 | 22 |
| 3 | 19 | 36 | 60 | 66 | 46 | 227 | 22 |
| 4 | 7 | 10 | 48 | 29 | 48 | 142 | 14 |
| 5 (highest) | 2 | 3 | 12 | 35 | 64 | 116 | 11 |
| Total | 200 | 205 | 231 | 204 | 197 | 1,037 | 100 |
| Percentage | 19 | 20 | 22 | 20 | 19 | 100 | ... |

* This initial distribution provided the cutting points in the scores to be used at the last interview.

Control for Age and Education

The question remains whether the increase in tolerance is part of a normal maturation process that would also have occurred outside the army, or whether it is due to the specific learning experience in military training. Without a civilian control group, it is difficult to distinguish between maturation and military-training effect. A first assessment is a comparison of the effects of age and education on the scores. Age correlated in this sample with a limited age range in a negative way, but at a negligible strength ($r = -.03$), with the authoritarianism scores. The older the soldiers were, the lower were their scores at both administrations. This is a confirmation of the earlier finding by Roghmann (1966, p. 184) that authoritarianism decreases with age until the mid-twenties and continually increases from that age on. However, compared with the effects of education, the age effect is small indeed. The higher the amount of education,

³ We are grateful to an anonymous reviewer whose critical remarks on this issue helped greatly to clarify this paragraph.

the lower the authoritarianism ($r = -.37$ in this sample). The indirect evidence from the comparison of the age effect and the education effect indicates that the observed decrease in authoritarianism, or increase in tolerance, during military training is not due to age per se but possibly to the learning experience in the military.

A second assessment of the problem is to control for age in tabular form, that is, to analyze the various age groups and their positions at the beginning and end of service. Table 4 presents clear evidence that, when age is controlled, the reduction of authoritarian attitudes is undiminished in all age groups. A similar statement can be made when controlling for education (see table 4). Within each educational group, experience in the military reduces authoritarianism. Education in the civilian school system and military socialization have a cumulative effect in reducing authoritarianism, with education having the stronger effect.

Possibility of Panel Effect

Since a true causal effect of the military in reducing authoritarianism is against general expectations, the possibility of a memory or learning effect should be considered. Possibly, it was not the military experience, but the test experience, at point 1 that led to the reduction in authoritarianism as measured at point 3. If this effect were operating, it should also be observed in panel studies outside the military, and the reduction should, in our view, be stronger the closer in time the two scale administrations occur.

The data from the student survey can answer this question. Table 5 presents panel data for this civilian control group. Although the scale administrations were just six months apart, we only find a small reduction, limited to the highest category. This, in our view, speaks against a panel effect, but if the reader does not accept the argument that the reduction should be stronger because of the shorter time span and instead argues for a "sleeping effect," that is, that the effect is a function of the length of time, we can test that argument, too, with the analysis presented in table 6. The observed data for the two studies were conceived, in this analysis, as reflecting two points in a process over time, which can be extrapolated to cover longer time spans than actually studied and thus achieve comparability of time periods covered. If we assume that the observed changes would occur again with every additional time period, this process can be described with a Markov chain, computing the matrix of transition probabilities from the data in table 3 (soldiers) and table 5 (students). Extrapolating the process for another three periods for both groups made it very obvious that the reduction of authoritarianism scores in the army panel is much stronger than in the student panel. Even after the third period, the quintile distribution for the students does not approach the one for

TABLE 4
DEVELOPMENT OF AUTHORITARIANISM SCORES OF DRAFTEES DURING THEIR SERVICE IN THE WEST GERMAN ARMY, CONTROLLING
FOR AGE AND FOR EDUCATION

| CONTROLLING FOR: | STAGE OF SERVICE | AUTHORITARIANISM SCORE, BY QUINTILE | | | | | Total | N |
|----------------------|---------------------|--|----|----|----|----------------|-------|-----|
| | | 1 (Lowest) | 2 | 3 | 4 | 5 (Highest) | | |
| Age (years): | | | | | | | | |
| 17-19 | Begin | 19 | 21 | 23 | 21 | 16 | 100 | 297 |
| | End | 34 | 21 | 25 | 10 | 10 | 100 | |
| 20 | Begin | 19 | 19 | 20 | 18 | 23 | 100 | 399 |
| | End | 27 | 22 | 22 | 18 | 12 | 100 | |
| 21-23 | Begin | 19 | 19 | 26 | 19 | 17 | 100 | 290 |
| | End | 34 | 23 | 20 | 12 | 11 | 100 | |
| Education: | | | | | | | | |
| Up to junior high | Begin | 12 | 15 | 22 | 25 | 26 | 100 | 661 |
| | End | 20 | 19 | 26 | 19 | 17 | 100 | |
| Senior high and more | Begin | 33 | 28 | 23 | 10 | 6 | 100 | 373 |
| | End | 51 | 28 | 15 | 5 | 1 | 100 | |

TABLE 5

CHANGE IN AUTHORITARIANISM SCORES OF WEST GERMAN STUDENTS AT TWO INTERVIEWS
SIX MONTHS APART, 1968 ($N = 959$)

| AUTHORITARIANISM QUINTILE, SECOND INTERVIEW | AUTHORITARIANISM SCORE, BY QUINTILE, FIRST INTERVIEW* | | | | | Row Sums | PER- CENTAGE |
|--|--|-----|-----|-----|----------------|-------------|-----------------|
| | 1 (Lowest) | 2 | 3 | 4 | 5 (Highest) | | |
| 1 (lowest) | 57 | 53 | 39 | 21 | 9 | 179 | 19 |
| 2 | 43 | 48 | 50 | 39 | 15 | 195 | 20 |
| 3 | 41 | 54 | 54 | 59 | 50 | 258 | 27 |
| 4 | 22 | 32 | 41 | 46 | 48 | 189 | 20 |
| 5 (highest) | 8 | 20 | 24 | 32 | 54 | 138 | 14 |
| Column sums .. | 171 | 207 | 108 | 197 | 176 | 959 | 100 |
| Percentage | 18 | 22 | 22 | 21 | 18 | 100 | ... |

* This initial distribution provided the cutting points in the scores to be used at the second interview.

the soldiers at the end of the first period. The argument of a panel effect, in whatever form, must, therefore, be refuted.

Results from Civilian Control Groups

Thus far, we replicated the findings by Campbell and McCormack (1957) that experience in the military indeed causes a reduction in authoritarian-

TABLE 6

HYPOTHETICAL DISTRIBUTIONS COMPUTED BY APPLYING THE OBSERVED TRANSITION
PROBABILITIES FOR ADDITIONAL TIME PERIODS

| TIME PERIOD | AUTHORITARIANISM SCORE, BY QUINTILE | | | | | | Total |
|--|-------------------------------------|----|----|----|----------------|-----|--------------------------|
| | 1 (Lowest) | 2 | 3 | 4 | 5 (Highest) | | |
| A. Hypothetical Distribution of Soldiers by Dogmatism Scores after Extrapolation for Additional Three Time Periods (17 Months Each) | | | | | | | |
| Begin | 19 | 20 | 22 | 20 | 19 | 100 | } Observed values |
| End first period | 31 | 22 | 22 | 14 | 11 | 100 | |
| End second period | 39 | 23 | 20 | 11 | 8 | 100 | } Extrapolated values |
| End third period | 43 | 23 | 18 | 10 | 6 | 100 | |
| End fourth period | 45 | 23 | 18 | 9 | 6 | 100 | |
| B. Hypothetical Distribution of Students by Dogmatism Scores after Extrapolation for Additional Three Time Periods (6 Months Each) | | | | | | | |
| Begin | 18 | 22 | 22 | 21 | 18 | 100 | } Observed values |
| End first period | 19 | 20 | 27 | 20 | 14 | 100 | |
| End second period | 19 | 21 | 27 | 19 | 14 | 100 | } Extrapolated values |
| End third period | 20 | 21 | 27 | 19 | 13 | 100 | |
| End fourth period | 20 | 21 | 27 | 19 | 13 | 100 | |

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ism. Differences in scores at the beginning and the end of military service cannot be attributed to different educational composition, to a maturation process, to a change in acquiescence set, or to a panel effect. If we were to accept the theory by Adorno et al. (1950), that is, that the scale measures a basic personality structure as formed in early childhood experiences, we would have to conclude that army life is in fact a very successful form of group therapy. But if changes in personality structure occur, they should be of a permanent nature. Men who served in the military at one time or another should, therefore, have lower authoritarianism scores, with age and educational background controlled.

Table 7 does not confirm this prediction. The scores of former servicemen were not significantly different from the scores of those who had not

TABLE 7

AUTHORITARIANISM AS A FUNCTION OF PREVIOUS MILITARY SERVICE, WEST GERMAN CATHOLIC FRATERNITY STUDENTS, 1968, AND WEST GERMAN STUDENTS, 1968

| COMPLETED MILITARY SERVICE? | AUTHORITARIANISM SCORE, BY QUINTILE | | | | | | N |
|-----------------------------------|---|----|----|----|----------------|-------|-----|
| | 1 (Lowest) | 2 | 3 | 4 | 5 (Highest) | Total | |
| | Present and Former Fraternity Members, Age 19-33* | | | | | | |
| No | 26 | 20 | 23 | 17 | 14 | 100 | 707 |
| Yes | 26 | 21 | 20 | 21 | 13 | 100 | 194 |
| Student Panel, First Interview† | | | | | | | |
| No | 17 | 20 | 22 | 20 | 21 | 100 | 519 |
| Yes | 15 | 20 | 23 | 24 | 19 | 100 | 186 |

* Distribution of all 1,510 sample persons was used to establish quintile cutting points.

† For 254 students there was no information about their veteran status.

served in the army. The effect of the military organization on the scores is only a temporary one, at least so far as former draftees with a relatively high educational level are concerned. (We have no comparable data on former draftees with a low educational background.) Controlling for age does not change the situation either (table 8). We notice that, in general, authoritarianism increases with age after the mid-twenties, but at each age bracket there are only random differences between those who have served in the military and those who have not.

However, table 8 yields more information than just the result of a control of age. If the reduction effect on authoritarianism due to the military were to fade away slowly, we would still expect some differences in the younger groups; but no differences in the older groups. No such fading effect can be observed. The "therapeutic effect" of the military is a very short-term one.

TABLE 8

AUTHORITARIANISM AS A FUNCTION OF MILITARY SERVICE, CONTROLLING FOR AGE, WEST GERMAN CATHOLIC FRATERNITY STUDENTS, 1968, AND WEST GERMAN STUDENTS, 1968

| AGE (YEARS) | COMPLETED MILITARY SERVICE? | AUTHORITARIANISM SCORE, BY QUINTILE | | | | | Total | N |
|--|-----------------------------------|-------------------------------------|----|----|----|----------------|-------|-----|
| | | 1 (Lowest) | 2 | 3 | 4 | 5 (Highest) | | |
| Present and Former Fraternity Members, Age 19-33 | | | | | | | | |
| Up to 23 | No | 29 | 21 | 20 | 18 | 12 | 100 | 143 |
| | Yes | 25 | 29 | 13 | 21 | 13 | 100 | 48 |
| 24-27 | No | 27 | 22 | 22 | 17 | 13 | 100 | 335 |
| | Yes | 27 | 17 | 21 | 23 | 12 | 100 | 108 |
| 28-33 | No | 23 | 18 | 26 | 15 | 18 | 100 | 229 |
| | Yes | 24 | 24 | 24 | 13 | 16 | 100 | 38 |
| Student Panel, First Interview* | | | | | | | | |
| Up to 23 | No | 19 | 20 | 23 | 19 | 19 | 100 | 259 |
| | Yes | 17 | 21 | 22 | 26 | 15 | 100 | 78 |
| 24-25 | No | 17 | 23 | 19 | 24 | 18 | 100 | 139 |
| | Yes | 15 | 17 | 23 | 25 | 20 | 100 | 65 |
| 26 and over ... | No | 14 | 18 | 24 | 17 | 26 | 100 | 121 |
| | Yes | 12 | 23 | 23 | 19 | 23 | 100 | 43 |

* For 254 students there was no information about their veteran status.

DISCUSSION

On the preceding pages we analyzed one relationship in great detail. We wanted to make sure that reasonable alternative explanations have been rejected. Because of the large samples involved, the availability of panel data, and number of controls introduced, we feel that the relationship described has been well proven. Though the evidence presented was only for one country, the similarity of our findings to those of the earlier American study by Campbell and McCormack (1957) indicates that most likely the findings would also be supported by data from other peacetime conscription armies in modern industrial nations. The following discussion will, therefore, limit itself to the question of how to interpret the findings and what implications they have for further studies in this field.

We had anticipated that the prescientific notion of an affinity between military and authoritarian attitudes would not be true. But we had also expected that the opposite relationship as reported in the literature would not stand up against a rigorous methodological test. As this expectation was not borne out, we will first examine the interpretation which would be offered by the theory of authoritarianism as originally formulated, and then propose our own interpretation. According to Adorno et al. (1950), we would have to argue, as was done above, that the military experience over an 18-month period indeed leads to a change in the basic personality

of the soldiers. Pointing to the radically different form of life in the military as a total institution could possibly support an argument toward such a "group therapy" interpretation. There are, however, major counter-arguments, in addition to the lack of permanence, against this interpretation. The theory in no way predicts the direction of such a group-therapy effect. If we had to speculate about the direction of such an effect, we would have argued in the opposite way. We would also have to accept a therapeutic effect in reverse when discharged soldiers reenter civilian life. Not only was such a reverse effect never argued, it certainly would not have been argued to be one from tolerance toward a greater authoritarian outlook.

The interpretation which we would like to propose was inspired by the concept of "relative deprivation" as developed by Stouffer et al. (1949). We will argue that we should take the items of the authoritarianism scale at face value, that is, not as necessarily reflecting a basic personality structure, but as a number of social attitudes expressing lack of tolerance and respect toward fellow citizens. We know from morale studies in the West German Army that draftees feel disadvantaged and underprivileged and complain about the reduction of human rights—feelings similar to those experienced by inmates of total institutions (Rogmann 1971). It is our hypothesis that people who feel temporarily underprivileged and disadvantaged relative to others will be more sensitive to items reflecting lack of respect and tolerance to fellow citizens. Morale was found to be relatively high in basic training but deteriorated rapidly during the later months of service. The reduction of authoritarianism scores would, therefore, indicate an increase in sensitivity for basic human rights, increased desire for social recognition, and greater dissatisfaction with the situation at the end of service than at the beginning. In terms of a theory of respondents' behavior, we should argue that this change in morale would also be reflected in decreased acquiescence, though in this case we doubt that there was much acquiescence to start with.

The reference to "relative deprivation" was not only made because of the special military situation, but because it also symbolizes a short- or medium-term process of attitude formation that is reversible and very much dependent on the social situation. This contrasts with the earlier research on determinants of authoritarianism that focused on irreversible, permanent, or long-term effects of childhood experience, social class, aging, or education.

There are two major implications of such a view. The first is that authoritarianism as an analytical concept, or independent variable, is only useful if we control for age and education *and* such short- or medium-term processes as the one described in the preceding paragraphs. Evidence for situational determinants of authoritarian attitudes has been presented

before (Rogghmann 1966, pp. 317-18). But the usefulness of the concept of authoritarianism becomes very limited if situational determinants are also to be controlled in order to observe its pure effect on any phenomenon. In fact, when it comes to explaining the distribution and formation of such important social attitudes as expressed in the items of the *F*- and *D*-scales, we may find other theories more useful than the one proposed by Adorno et al. (1950).

The second implication is that the search for attitude structures per se, as reflected in the many factor analyses of responses to *F*-scale or *D*-scale items, is possibly useless. If indeed short-term and reversible social processes lead to many of the item responses, it may be futile to expect consistent attitude or personality structures in the same way as we expect persistent intelligence configurations. The argument is not a methodological one, like the "spuriousness" argument proposed by Selvin (1969, p. 131), but a substantive one. There may indeed exist many different structures, but they may fluctuate from situation to situation. It is not their existence which is disputed, but the usefulness of their knowledge because of lack of stability over time.

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Erratum

A typographic error was made in "A Method for Detecting Structure in Sociometric Data" by Paul W. Holland and Samuel Leinhardt (November 1970). On page 503, in row 5, column 6 of table 3, the formula for $D_3 p_2(i, j)$, when i is triad 120C and j is triad 201, appears as $m^{(2)}a^{(4)}n$ but should be $m^{(2)}a^{(2)}n$.

Book Reviews

Sect Ideologies and Social Status. By Gary Schwartz. Chicago: University of Chicago Press, 1970. Pp. x+260. \$9.00.

Johannes Fabian

Northwestern University

This is a study of Seventh-day Adventist and Pentecostal groups in an urban context in the United States. The title may suggest, at first, the kind of straightforward empirical treatment, consisting of some prose around a lot of statistical tables, that one has come to expect from recent sociology of religion. Instead, the reader is treated to what I regard as one of the most intelligent, sensible expositions of major theoretical issues, of perceptive empirical observations, and, above all, of critical reflections on the nature of social scientific inquiry to appear in recent years.

The main thrust of the book is directed against attempts to relegate religious and other "cultural" orientations to a residual, epiphenomenal status in sociological understanding. Developing ideas derived from Weber, Parsons, Geertz, and others; the author makes a cogent theoretical argument in the course of which he successfully debunks a number of entrenched notions, such as the ill-conceived distinction between instrumental and expressive means of action (pp. 46-47). He gives a well-documented, critical discussion of the general theory of sect and sect affiliation and then turns to extensive analyses of the two belief systems, focusing on key tenets of cosmology and ethics. In this part, in which reports on doctrine are intertwined with historical and sociological commentary, his style of presentation makes the book eminently readable. The exposition of the doctrines is followed by chapters on the social organization of the groups and the social biographies ("status trajectories") of some of the key informants. The book concludes with a reexamination of the basic thesis in the light of distinctions and qualifications that had to be made to accommodate the two empirical cases under consideration. Perhaps most important is the proposition that, in examining the impact of religious ideas on social action, theoretical and methodological distinctions must be made between segments (or aspects or levels) of doctrine directly governing the individual believer's life and those that go into the formulation and elaboration of doctrinal systems. As Schwartz puts it, his "material indicates that it is hazardous to make inferences about a group's prevailing attitude to the prevailing social order solely on the basis of its most striking doctrines" (p. 222). That thesis indicates the theoretical limits to which this study is carried; but it also dramatizes the limitations inherent in a basically Weberian-Parsonian approach. The point is that a conceptual framework in which intellectual orientations (ideas) are seen to "govern" social behavior (actions) always seems to lead to a focus on *motivation*. Often this reduces orientation to sentiments and emotions (especially with regard

to religious action). But even when this is not the case, motivations have to be *imputed* and imputation generally ends precisely in the ahistorical, moralistic, and sociologistic style of interpretation which both Weber and Mannheim tried to avoid. In my opinion, this outcome is inevitable unless the theoretical frame is extended to include the intersubjective, communicative dimension of social reality. Epistemologically, access to that dimension is gained when one views religious ideology as a communicative product and relates it to a medium of communication and a community of producers. All these are notions which, to some extent, are already developed in an "ethnography of communication" (formulated by Dell Hymes and others). The most promising avenue for further work should then be sought in the line of current efforts in the field of anthropological linguistics (or sociolinguistics) in which language communication is studied as the crucial (but not the only) medium that constitutes and articulates intersubjective, shared social reality. Schwartz shows implicit awareness of that dimension when he introduces notions such as "grammar of motives" (p. 118), "idiom of sincerity" (p. 125), "dominant metaphors" (p. 137), "vocabulary of motives" (pp. 215, 216), an actor's "idiolect" (p. 232), or the "language of religious discourse" (p. 234). If we successfully integrate notions of this kind into a consistent frame of theory and research methodology they should make it possible to identify concrete, historical-processual mediations between actors and their beliefs, but also between observer and observed, and between the analyst and his presentation of a system of doctrine (see also pp. 89 ff.). All this would add rigor and depth to the highly perceptive and critical observations which Schwartz makes concerning the nature of anthropological participatory field work. Despite some minor shortcomings and debatable assumptions, such as an unconvincing treatment of the concept of charisma (pp. 158 ff.), a neglect of the ethnic background of one of the groups, and the related, important issue of the nature of "second conversions" among adherents of these doctrines (pp. 184-85), this study has raised the discussion to a level on which further and new theoretical developments appear necessary and imminent.

The Lutheran Ethic: The Impact of Religion on Laymen and Clergy. By Lawrence K. Kersten. Detroit: Wayne State University Press, 1970. Pp. 309. \$8.95.

Karl H. Hertz

Hamma School of Theology

This study, carried out in metropolitan Detroit, examines both the prevalence and the consequences of a traditional theological ethic among the laity ($N = 886$; sample ratio varied to equalize representation) and the clergy ($N = 241$; approximately 89% of all clergy) of the four major Lutheran groups in the United States—the Lutheran Church in America, the American Lutheran Church, the Missouri Synod, and the Wisconsin

Synod. A sample of students ($N = 1,095$) at Eastern Michigan University also completed the questionnaire.

Kersten used Troeltsch's account of Luther's two-kingdom ethic to describe the Lutheran world view. Drawing on Fromm, Tawney, and H. Richard Niebuhr, he developed the theological presuppositions and moral directives of the ethic. To questions about specific beliefs, he added questions about religious involvement taken from the work of Charles Glock and Rodney Stark, then probed attitudes towards minority groups, civil rights and civil liberties, a variety of moral issues (abortion, gun control, Vietnam, etc.) organized religions, and religious teachings about man and God, free will and salvation. Finally, he used the student data to compare the attitudes and ideologies of members of various denominations.

The findings support the hypothesis that many Lutherans take conservative positions on social issues. Clear differences exist among the clergy and laity of the four groups: the conservative-liberal continuum runs Missouri-Wisconsin clergy, Missouri-Wisconsin laity, ALC-LCA laity, and finally ALC-LCA clergy. Among the clergy, where Kersten used only the belief index, orthodoxy showed the familiar association with conservative political attitudes. Among the laity, however, except for a moderate correlation with anti-Semitism, orthodoxy did not predict responses. Associational and communal involvement, which Gerhard Lenski used in his Detroit study, showed some relationships: associational involvement with liberal attitudes, communal with conservative attitudes.

Unfortunately, serious questions must be raised about Kersten's use of history and his interpretation. In describing a Lutheran world view, Kersten inexplicably uses primarily 20th-century critics of Luther, not Luther himself or American Lutheran teachers of ethics. Having developed a "unique" Lutheran position, he then apparently did not ask his respondents questions derived from this position, not even whether they accepted the two-kingdom perspective. None of his questions on religious beliefs require uniquely Lutheran answers; his conservatives could just as well be Protestant fundamentalists.

Kersten separates theological doctrine totally from history, assuming an immaculate transmission of teachings from Luther to the present; world views seem to be unrelated to contexts, structures, and actual encounters with issues. Had Kersten examined even recent Lutheran history, he would have found a social action variant of the two-kingdom doctrine and also Lutheran ethics without the two-kingdom perspective. The renaissance of two-kingdom thinking is very recent, and closely associated with German controversies, especially the resistance to Hitler. Until after World War II the doctrine received little attention in most American books on ethics.

Thus the sociological question is why certain groups have recently adopted the conservative variant, others the liberal one. Can we relate beliefs, issues, and the sociohistorical location of the four groups? How does each use its beliefs to define itself, interpret events, respond to them,

and justify its responses?

We need more than the assertion of relative positions (which this study documents); we also need an explanation with more sociological substance than the immaculate transmission of doctrine from the 16th century to the present. If Lutherans are unique, we need to show how beliefs peculiar to them relate to political positions. Kersten's findings are important, but his uses of history and his interpretations are inadequate and misleading.

Social Stratification: Research and Theory for the 1970s. Edited by Edward O. Laumann. Indianapolis: Bobbs-Merrill Co., 1970. Pp. 280. \$2.50 (paper).

David D. McFarland

University of Chicago

The book under review belongs on the shelf—when it isn't actually in use—of every serious student of social stratification. It is a reprint of a special issue of *Sociological Inquiry* (Spring 1970) devoted entirely to social stratification.

The market these days is flooded with readers, volumes of conference proceedings, and other "books" consisting of collections of papers by many authors. Unlike many of these, the present volume has (with one major exception to be noted below) a great deal of coherence—the various authors are largely operating within a single frame of reference.

After an overview of the book provided by Laumann in the editor's foreword, the book proper begins incongruously with an essay by Talcott Parsons, an analytic approach to a further revision of his revised analytic approach to the theory of social stratification—incongruously, because that chapter does not seem to have anything to do with the remainder of the book. Indeed, some doubt is raised concerning the importance of Parsons in the field of social stratification since only one of the remaining authors (James Kimberly) finds occasion to cite any of Parsons' writings.

The subsequent chapters are as follows: James Kimberly considers the emergence and stabilization of stratification in societies, drawing upon what is known about the emergence and stabilization of stratification in small groups; Robert Hauser writes on the effects of social origins on schooling and the effects of schooling on socioeconomic achievement; Bruce Warren treats the relationship between socioeconomic achievement and religion; Paul Siegel discusses occupational prestige within the Negro subculture; Stanley Lieberman considers stratification among other ethnic groups; Robert Hodge analyzes the relationships between some psychological and socioeconomic variables; Donald Treiman studies the relationships between industrialization and stratification; Harriet Zuckerman analyzes stratification within American science; and Thomas Pullum summarizes mathematical models of occupational mobility.

This book is presumably intended for classroom use, in competition with the various other stratification readers, such as those edited by Bendix and Lipset; Heller; Laumann, Siegel, and Hodge; etc. But it is really a different sort of book, for three reasons: (1) it is more narrowly focused—in contrast with such readers as the Bendix and Lipset volume—almost exclusively on stratification in the United States; (2) It is current: the Davis-Moore debate is something everyone should read *once*, but this volume thankfully takes the position that it is better to ignore such matters than to rehash them yet another time without providing any new insights; and the reader completely unfamiliar with path analysis may have difficulties at points—as, indeed, he will when reading the major sociology journals these days; (3) and to a large extent it follows a single, coherent conceptual scheme—a matter to be elaborated shortly.

These features seem, to this reviewer, to make the book under review more suitable for *advanced* courses and seminars, whose students have already been through an introduction to stratification, and have a minimal acquaintance with such contemporary tools as regression analysis. A reader, such as the one by Laumann, Siegel, and Hodge, is undoubtedly more appropriate for the usual upper division/early graduate first course in social stratification.

The American Occupational Structure (New York: Wiley, 1967), by Blau and Duncan would be the best background reading for students not yet fully prepared for the volume under review. Although *Social Stratification* contains nothing written by Duncan, his influence is visible throughout in such matters as repeated use of his index of socioeconomic status and variants of his path-analysis model of occupational attainment. In fact, many of the authors included are Duncan's first- or second-generation students. This is a rather surprising bias to be engaged in by an editor trained under Parsons—if, indeed, it is a bias. Perhaps Duncan, rather than Parsons, should have been selected to write the opening chapter, which attempts to give the big picture on current research and theory in social stratification.

The Future of Inequality. By S. M. Miller and Pamela A. Roby. New York: Basic Books, 1970. Pp. xvi+272. \$7.95.

Edward O. Laumann

University of Michigan

For the policy-oriented, this book provides a reasonably up-to-date and useful compendium of data, usually from published governmental sources, on the differential distribution of social inequalities in America, most especially income, by selected demographic and social characteristics, and is a useful guide and set of standard references to the voluminous American literature documenting and analyzing various aspects of social inequality. The professional student of social stratification will find con-

siderably less that is unfamiliar or new to him in either the data presented or the theoretical perspective (watered-down Weberian) employed. This is partially due to the authors' avowed purpose of trying to suggest specific policy implications of their data, given their general objective of reducing inequalities in America, which leads them to address their book largely to a "nonprofessional" audience of what they hope is an emerging coalition of "the poor, the black, the students, the marginal whites, and the disaffected professionals," who at present "do not share one common agenda" or program. "We hope this book provides analysis and data that can lead to a program" (p. ix). The partial solutions proposed for various problems (e.g., relating to inadequate incomes, low educational opportunities, and low political participation of the poor, to name but a few) are all fairly familiar to any of the more left-leaning members of the liberal Democratic coalition, and should come as a surprise to no one with respect to either their objectives or justifications.

The book is divided into two major sections. The first and much larger part (and the more successful in achieving its stated purpose) is devoted to a discussion of six dimensions of well-being (including income, assets, basic services, self-respect, opportunities for education and social mobility, and participation in many forms of decision making). The relatively short second part is more an exercise in futurology, albeit perhaps somewhat more plausible than most, that attempts to project the likely contours and extent of inequality in the next several decades and what should be done about it.

In part 1, one finds two chapters on the income dimension and one chapter devoted to each of the other dimensions of well-being, in which the authors seek to "(1) explain the dimension of well-being under consideration, (2) examine existing data on distribution with regard to that dimension, (3) identify the social indicators needed for further analysis, (4) outline the short-term goals of inequality reduction and define minimum levels of well-being, and (5) suggest implications of the data and analysis for social policy" (p. 12). The central starting point (chap. 1) is that we, and especially the federal government probably for political reasons, have gone the wrong way around by focusing on poverty in and of itself rather than seeing it in the broader perspective of the general institutional arrangements of social stratification for the society as a whole—that is, inequality rather than poverty should be the main issue of social policy. Miller and Roby make a number of well-founded and useful objections to the position of those who adhere to a "culture of poverty" and "inheritance of poverty" view by indicating the rather high rates of mobility into and out of the "poverty category" however defined, and by suggesting that the six dimensions are not that highly correlated and, therefore, rather different sorts of people are numbered among the poor, depending on the dimension of well-being considered. These ambiguities pose some important implications for policy strategies which are noted at appropriate places in the text.

Perhaps one of the strongest sections of the book is the second and

third chapters where Miller and Roby provide a useful review of the three approaches to poverty defined narrowly as income deficiency: poverty defined by some absolute standard in terms of a cost-of-living budget estimate and the resulting pseudoscientific precision of the "poverty line" that marks current policy, and poverty defined by a relative standard, either as a percentage of the mean or median family income in the United States (where changes in the median income automatically shift the definition of what is inadequate income) or as the share of the total national income going to the bottom 10%, 20%, or 30% of the population. They rather nicely delineate the different implications of these three approaches for identifying different characteristics of the poor and for policy and clearly indicate their reasons for preferring one or the other of the "relative" approaches.

They proceed to a fairly conventional and straightforward discussion of the other five dimensions of well-being, covering the standard literature in a reasonably representative manner, with some useful, but hardly novel, conceptual distinctions that should help to clarify at least what the issues at stake are. Here the authors consider such conventionally discussed matters as the differential distributions of assets (including adequate housing, consumer durables, and insurance), basic services (including health, neighborhood amenities, mass transportation, and legal services), education and social mobility (with special reference to black education), power (including representation of disadvantaged group interests in the political process, their treatment at the hands of bureaucracies, and client participation in bureaucratic decision making). The treatment is rather superficial since they can manage only to touch base on these complex issues in the three to five pages devoted to each topic. Oddly enough, I found one of the most disappointing chapters to be the one on education and social mobility, given Miller's previous work in the area. The discussion is rather conventional and old-hat (e.g., beating away at excessive credentialism and the need for alternative avenues of entry into the job market than educational certification, without making any concrete suggestions about these alternatives), and the literature coverage is rather selective, missing some important literature (e.g., Paul Siegel's article, "On the Cost of Being a Negro," *Sociological Inquiry*, vol. 35 [Winter 1965]). They are to be congratulated for at least raising the question of more often neglected subjective aspects of inequality, such as social honor, styles of life, and self-respect (chap. 8), although their coverage of the relevant literature on this topic is probably the weakest in the book.

One of the high points of the book, in my opinion, is the very short chapter 9, "Toward a Redefinition of Well-Being," which succinctly raises some of the most central policy issues in the handling of societal inequality, pulls together the many themes previously discussed in a sophisticated manner, and puts them in perspective in a way I have rarely seen matched. Perhaps Miller and Roby should have stopped at this point, because I found the four chapters in part 2 on the future of inequality to be quite a letdown. There is a perceptible decline in conceptual and

analytic clarity (admittedly, the authors are addressing themselves to very fuzzy ideas—e.g., authenticity and the quality of human relations), and the prognoses and diagnoses are sometimes dubious and at times contradictory.

Throughout the book, policy goals are typically formulated in a very specific fashion, but often there is little rationale provided for picking the stipulated objective rather than some other (see, e.g., pp. 60–61). More annoying, the means or technology by which these objectives might be realized and a rationale for choosing among competing means are rarely indicated in any clear fashion. The result is a certain arbitrariness of treatment of policy objectives and, particularly, their implementation.

In sum, the book is easy to read, relatively free of jargon, sophisticated in its handling of the social scientific literature, clear about its objectives and preferences, provocative, and highly suitable for undergraduate courses oriented to social problems and social stratification, where it should stimulate some useful discussion.

Poverty and Politicization: Political Socialization in an American Sub-Culture. By Herbert Hirsch. New York: Free Press, 1971. Pp. xv+206. \$8.95.

The Politics of Participation in Poverty. By Dale Rogers Marshall. Berkeley: University of California Press, 1971. Pp. xi+210. \$7.95.

Poverty and Politics in Harlem: Report on Project Uplift, 1965. By Alphonso Pinkney and Roger S. Woock. New Haven, Conn.: College & University Press Services, 1971. Pp. 191. \$6.00 (cloth); \$2.95 (paper).

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To review three books of so little merit is disappointing. In two of the three, one searches in vain for some reason to justify publishing these materials in book form. The third has some redeeming features, but its value is diminished by a limited conceptual framework.

Herbert Hirsch studied 2,544 Knox County, Kentucky, schoolchildren in grades 5–12. The major objective of the study was to assess the impact on political socialization of four agents: the family, peers, the schools, and the mass media. The data were gathered by a questionnaire of 274 items administered during regular school hours; the same schedule was used for all eight grades.

The introductory chapter reviews a wide range of literature on political socialization and concludes with a critique and a call for a new theoretical framework. In the next chapter, Hirsch provides in four pages an alternative theory which seeks to combine the existing data on political socialization, social learning theory, and "some propositions derived from cultural theory and personality theory" (p. 20). A large number of distracting

quotations are employed to support Hirsch's theoretical position; whatever original contribution to theory he hopes to make is obscured by his constant recourse to quotations from others.

Poverty plays a role in the study design because the Appalachian region is viewed as a subculture in which the various agents of political socialization may manifest themselves in unique ways that will contribute to a better understanding of political socialization in general. If this region is indeed marked by severe poverty, and there is no reason to doubt it, the research instrument appears overly sophisticated and frequently ambiguous. Consider the following question: "Do you believe that people have the right to change the government if they don't like what it is doing, or do you think the government should be kept like it is?" (p. 173). How to interpret what a fifth grader, a ninth grader, or even a high-school senior in a culturally deprived area means by selecting any response to this double-barreled question is a mystery to me. Such problems raise doubts about the reliability of the instrument.

Furthermore, Hirsch appears to be dominated by his data rather than in command of them—104 tables with 116 substantive pages is an obvious case of data overkill. For instance, it does not seem particularly useful to employ five tables to test the following hypothesis: "When the father is absent from the home, he will rank significantly lower than the mother as an agent of political socialization" (p. 62). Even then, the reader does not know how long the father has been absent or, in fact, if he was ever present.

Although the author promises to deal in his conclusions with poverty as a filter through which the stimuli of political socialization pass, only cursory attention is paid to the culture of poverty. One gains no new insight into how poverty influences the stimuli studied nor its consequences for political socialization. Had Hirsch been more discriminating in sharing his data with us and less ambitious in his theoretical promises, the distillate might have made a readable journal article.

The Politics of Participation in Poverty suffers from a different set of problems. Dale Rogers Marshall's study of the Economic and Youth Opportunities Agency of Greater Los Angeles (EYOA) takes as a central problem interaction between poverty members and other members of the agency's board, focusing on processes of socialization and cooptation. The data were gathered from two major sources: unstructured, but focused, interviews with all board members and field observation at board meetings and committee meetings.

To study cooptation in an organization means to investigate the decision-making structure. But Marshall's focus on cooptation at the board level is a case of misplaced emphasis. As she carefully documents, the board of EYOA was extremely weak and the agency staff held real power: "The executive director carefully prestructured issues which came before the board, and the board for a variety of reasons such as the culture that stressed avoiding controversy and their lack of information, were not in

a position to question the actions" (p. 71). Once this situation has been documented—and there is substantial supporting evidence—one cannot help but ask what purpose there is in investigating cooptation among members of the board. The politics to which the title alludes is certainly more manifest in board-staff relations than within the board itself.

Another critical condition downgrades the importance of the problem as conceptualized. The composition of the board gave the public-agency representatives a dominant voice in policy making. These board members held veto power over board decisions and were satisfied for the most part with how EYOA was administered. In addition, the public-agency representatives "pointed out how nice it is that when there are crises in the war on poverty the pickets go to EYOA rather than to city or county hall" (p. 73). This function of a community action agency suggests some interesting questions about the organization of poverty politics. Unfortunately, Marshall did not take this as her central theme.

Given the thrust of these findings, one questions seriously the relevance of data on board members' ages, ethnic identities, class backgrounds, and so on. Nor is it demonstrated why these data are important when considering the questions of socialization or, as Marshall puts it, "Who is learning what" (p. 4). Although frequent interactions among board members over the several months produced modifications in the attitudes each held toward the others, there is little evidence that these modifications resulted in changes in the poverty programs or in the structure of power within EYOA.

In *Poverty and Politics in Harlem*, Pinkney and Woock present a case study in the war on poverty. Their two goals are to offer a descriptive analysis "of one large, short-term poverty program in a single community" (p. 20), and then to use this analysis as a springboard to formulate more constructive tactics and strategies for battling poverty than were used by the program under review.

Beginning with a concise summary of the social conditions in Harlem in the middle-1960s, the authors assess the response to the riots of 1964 by government officials at several levels. The consensus of these officials was that some action needed to be taken to reduce the possibility of future disturbances, tactics that would resolve some of the causes of urban unrest. The palliatives suggested and implemented were focused on keeping large numbers of Harlem residents—particularly young Harlemites—busy in a wide range of activities, including administering Project Uplift itself during the summer of 1965.

The study documents the organizational stress within the demonstration project itself—raising serious questions about the suitability of demonstration projects as a tactic in the war on poverty—and the organizational problems of working in an ongoing poverty program, HARYOU, that was concerned with maintaining its image and power in the community. In addition, Project Uplift's mandate was unrealistic, given the extraordinarily short time for planning and staffing (the final funding

was approved late in June 1965). The results were predictable—poor coordination, bad administration, frustrated staff and clients, and bureaucratic infighting.

These conditions are documented by a number of anecdotes from field work carried out by the authors. Additional data from a survey of Project Uplift enrollees are also used. But these data are not presented in tabular form, and it is impossible to discover the size of the sample or any variations that might have resulted from differences in age, sex, family status, and so on. An appendix including such tables would have been useful, even if the authors wished to keep the narrative free of them.

The final chapter is programmatic. The authors make a number of recommendations that point to the major issue in community action programs, namely, that effectiveness necessitates both power and payoffs—power to the community in which the programs are being implemented and payoffs to those who become their clients. It is of little use, the authors conclude, to train persons for nonexistent jobs and to develop skills for which there is no market. But neither the anecdotal data nor the results of the survey are tied to a more theoretical statement that would have provided a general insight into mobilizing a community for self-help.

Of the three books reviewed here, only the Pinkney and Woock study merits serious attention. Its value lies in the documentation of the futility of the hasty, stop-gap measures employed in fighting poverty during the 1960s.

Regulating the Poor: The Functions of Public Welfare. By Frances Fox Piven and Richard A. Cloward. New York: Pantheon Books, 1971. Pp. xvii+348. \$10.00.

Thomas Blau

University of Colorado

This book is written with facility and the political importance of the authors is underscored in the price. The publisher's presumption of inelastic demand, however, ignores the existence of excellent substitutes, such as Gilbert Steiner's *The State of Welfare* (Washington, D.C.: Brookings, 1971) and *The Deserving Poor* by Joel Handler and Ellen Jane Hollingsworth (Chicago: Markham, 1971).

The virtues of this book include the deftness of the writing, its sense of well-restrained anger, and the attempt to understand the history of welfare in a unified perspective. Ideological and emotional sympathy from the reader are assumed. This prediction is no doubt frequently correct, but the result is a failure to clarify values and goals beyond the obvious, such as, let's stop being mean to poor people; but who is left to endorse the midnight raid on the ADC mother? Its faults originate

in the intellectually limited nature of its argument, which concludes where it should begin.

Piven and Cloward's idea of what welfare ought to be (which provides an idea of the baseline norms of the book) is too simple; welfare ought to be *more*, unadulterated by work requirements:

Large-scale work relief—unlike direct relief which merely mutes the worst outbursts of discontent—tends to stabilize lower-class occupational, familial, and communal life, and by doing so diminishes the proclivities toward disruptive behavior which gives rise to the expansion of relief in the first place. Once order is restored in this far more profound sense, *relief-giving can be virtually abolished*. . . . The point is not just that when a relief concession is offered up, peace and order reign; it is, rather, that when peace and order reign, the relief concession is withdrawn. [P. 347, italics in original]

In the absence of fundamental economic reforms, . . . *we take the position that the explosion of the rolls is the true relief reform*. [P. 348, italics in original]

There is an important calculation buried here that deserves to be made explicit. Piven and Cloward have presumably weighed the benefits of the stabilization of lower-class life against its tendency to diminish "the proclivities toward disruptive behavior which gives rise to the expansion of relief" and found the net impact on the poor to be negative. That is, it is better for the poor to lead an unstable life on welfare than a stable one off. Piven and Cloward also calculate implicitly that the gains of welfare are such as to make worthwhile any costs to the poor from disruptive behavior itself. There are also costs of waiting and risk involved in attempting to expand the rolls through disruption, and the supply of welfare as a function of disruption is probably a very discontinuous curve, with tortuous plateaus for the disrupting poor. There is a calculus here, and I believe that if the unstated assumptions of Piven and Cloward are thought through, it will be found wanting.

Any attempt to "stabilize lower-class . . . life" draws their ire because it means bringing the poor into the occupational structure. They write that "the array of activities relief agencies carry on in the name of re-equipping recipients for jobs . . . is also an important source of degradation" (p. 170). They cite as evidence an HEW memo which they contend demonstrates clearly the unscrupulous designs of government: "There is no more urgent and vital task facing us today than to assist increasingly large numbers of the nation's public welfare applicants to become economically independent."

In other words, attempting to get people off welfare into participation in economic and social life is degradation. Responding to the welfare mess by putting everybody on welfare is not. I do not know if Piven and Cloward are so dismayed by the failures and abuses of the welfare system in its attempt to apply a principle such as vocational training that they feel compelled to call for the abandonment of the principle, or whether

they are opposed simply to integration of the poor into the country's economic life on any scale less than middle class. If the first, it may be a matter of babies and bath water and requires an explanation which they do not give. If the second, we are again faced with the specter of Tom Wolfe's radical chic. I have no criticism of such tastes, except when they are alleged to be the proper ones of the people under study, who are charged with deviance if they fail to share them.

Piven and Cloward ruefully present evidence of the desire of welfare clients for jobs and their support for agency practices which are questionable and worse (pp. 171-73). Their explanation is that "recipients tend to identify with their oppressors" (p. 172) and that "when victims are induced to collaborate as victimizers, submission is assured." There is a problem here, the perception of which is obscured by vague sympathy and outrage about welfare which readers will often share with the authors.

Many people have become very subtle about ideas such as "oppression," even to the Dostoevskyian extent (e.g., Saul Bellow's *The Victim*) of suggesting that the identification of victim and oppressor may be the reverse of that expected from conventional categories. This is an entertaining conceit in literature and important in psychoanalysis, but it requires considerably more analysis and explication when applied to large-scale social phenomena in order to avoid being a kind of sophisticated name-calling that is too clever by half. For instance, Piven and Cloward might have attempted to show that clients are initially resentful of practices to which they become accustomed and supportive progressively under the stressful process of agency pressure, producing an unconscious acquiescence which they would never grant if they were emotionally autonomous. The useful model would have been Bettelheim's work on victim-oppressor relations in *The Informed Heart*.

Suggesting that endorsement of such a relationship is evidence for its existence is chancy at best, and must be made with a care which Piven and Cloward do not exercise. If we assume that this kind of proposition serves any purpose, the answer must begin by noting one solid piece of data: Piven and Cloward (and probably almost every reader of such a book) are oppressed by the welfare relationship. Welfare is perceived as oppressive not because it starves people, which it does not appear to make a matter of practice, but because it keeps clients at a level below that which many define as "decency." The exclusion of the poor from the possession of socially accepted standards of "decency" in favor of subsistence may result in political consequences as severe as those which might be produced by the denial of subsistence. Without the explication of what those socially accepted standards are, discussion of welfare easily becomes exhortation, name-calling, and confusion. This book suffers from this confusion because it fails to perform this explication: Welfare for what? How far beyond subsistence should welfare go, and for what ends? The only end I find in their proposal is welfare for its own sake.

Piven and Cloward's thesis is that the function of welfare has been

to keep the unemployed from getting restive and to be less attractive than the low-wage manpower pool. Not only is cheap labor a goal for the rest of society, but a corollary is the enforcement of the work role as the "cornerstone of social control." I do not understand the metaphor, and I do not know what the unpopular "social control" tells us instead of the less evocative "social integration," except that the authors do not like it.

For instance, they write of WPA:

By once more enmeshing people in the work role, the cornerstone of social control in any society, it went far toward moderating local disorder. Had there been no work relief to quiet the populace, it is difficult to say (even in retrospect) whether the consequent turmoil would not have imperiled the nation's economic and political institutions. But the people were amenable to work relief; indeed, they demanded it, for the "habit" of work, as Harry Hopkins observed, was what they "liked." [P. 97]

Once again, people are having something bad done to them (they are being enmeshed) but they seem to be suckers enough to like it. Piven and Cloward are unaware of any irony; the danger of their solicitude becoming patronizing does not seem to have occurred to them. The second sentence is so qualified that it leaves no meaning except a lame attempt to slip in a weak and unnecessary thesis.

Piven and Cloward are bright, write smoothly, and are passionately involved with the subject. They are so involved that they fail to see any other interests beside the immediate ones of the welfare poor, and therefore choose a short-run solution which extends the failure of the past. I got a sense of harrowing conspiracy from the book which I find not so much wrong as pointless, which is worse. They see local variation in relief arrangements as a product of variation in the needs of local economies for low-wage workers. They are not moved by local variation in ability to contribute to welfare or local variation in welfare demand. The external economy that New York performs for the nation by being the repository of human tragedy is great for Oregon and Colorado (busy trying to figure out how to discourage others from emigrating in). It is not so great for New York. I suggest that the inequalities of localized relief is a major, rational, and nonracist reason for antagonism to welfare. I share the horror Piven and Cloward apparently feel at the amounts that will be forthcoming from Family Assistance, but I doubt that any improvement can occur without the establishment of the principle of a unified *national* commitment. With that accomplished, we can argue the purposes of welfare in the framework of national responsibility for the unfortunate of the nation, which leads to considerably different results than argument about the responsibility of Gary, Indianans for the unfortunate of Gary. The issue is whether analysts are willing to see the tragedy of welfare as a national one, and not just for the poor who suffer it. Once again, the group-conflict model leads us straight to nowhere, both as scholarship and as politics.

Blaming the Victim. By William Ryan. New York: Pantheon Books, 1971. Pp. xv+299. \$6.95.

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Scapegoating is not new, nor is there any reason to believe that wealth and power enable people to be honest and decent enough to shun the mind's ever-ready capacity to fool itself and make use of others for all sorts of apparent and not so apparent reasons. Nor is scapegoating simply one among many of the mind's maneuvers, and chosen more or less randomly—in response to a particular person's whim or fancy. For one thing, scapegoating is a social art. Others become implicated in our private dilemmas. We shove off things from ourselves to those others (blame, guilt, faults, sins—the list goes on and on) when there is a good, strong reason for us to do so: we are afraid to acknowledge what is in ourselves, or we profit enormously by keeping intact one rather than another view of ourselves, or we have practically no access to ourselves at all, and so grope for others (yes, even the people we scapegoat) as a means of self-revelation. William Ryan is a psychologist who knows all that from his experiences with particular individuals, but he is also a long-time political activist, and in recent years he has rather clearly decided that until this nation's political and economic system is changed, and changed quite significantly (rather than patched up here and there every time a crisis occurs), there is no escaping the kind of psychopathology he describes in *Blaming the Victim*.

Yet, by no means is Dr. Ryan primarily interested in individual psychopathology. He wants us to look all around us as well as inward; he wants us to stop and think why we (who read books like his and journals like this one) spend so very much time talking about the "cultural deprivation" among blacks or Chicanos or Indians, about the "mark of oppression" or the psychological "disadvantage" to be found among ghetto children—and much less time trying to analyze what in our particular social and economic system makes for the very special circumstances some groups of people have to contend with: the scandalously high infant mortality rate among those people, the high unemployment rate they have to face as a fact of life, the wretched housing, insulting welfare regulations, not to mention the thoroughly discouraging schools their children must attend.

To examine some of these "structural" problems, to ask why the world's richest and most powerful nation chooses to spend 70 billion dollars a year on its armed forces, while thousands and thousands of our children grow up malnourished, get bitten by rats, suffer lead poisoning because certain buildings, thousands of them, are allowed to stand—to look at such awful paradoxes is unnerving, Dr. Ryan keeps pointing out, and so we have to do something to ease the tension. We could, of course, look at this nation very carefully. We could ask who owns what, who

profits enormously from whose labor. We could, that is, ask the kind of blunt, unsparing questions that a Ralph Nader does or a Cesar Chavez or a Saul Alinsky or a Jesse Jackson. We could, if we wish the sanction of our own history, take up the southern and midwestern populist cause; that is to say, we could emphasize the rights *all* men have—for decent work at substantial wages, for a strong say in how the wealth of this country is divided up; and it is wealth that was made over the years by the labor of millions of men and women, many of them penniless slaves. But as Dr. Ryan keeps emphasizing, we are likely to find it painful to look at such matters. The closer we get to looking, the more probable it is that we will be confronted with—ourselves. True, we are liberals, most of us who even bother to read *Blaming the Victim*; we want all sorts of changes, and are proud to enumerate them and declare openly our hopes and dissatisfactions. There are limits, though; and especially when “they” begin pressing too hard, and even start calling us condescending or self-centered or bossy—then we stand up in all our pride and impatience and self-righteousness and let it be known who is where on this nation’s various ladders and scales. Not that we are the kind to be rednecks or members of some “silent majority.” Well-educated, articulate, determined to be known as compassionate and fair-minded, we shun nasty or abusive language and instead simply reach for the labels: they are *this* or *that*, they need *help*—of a kind that takes years to give, and even then the outcome is in doubt.

Meanwhile, every day thousands of bright, intelligent, resourceful children fight hard in our ghettos to live, to find their “deal,” to outwit “the man,” to come to terms with odds the rest of us never have faced and never will know, either—not in the concrete, day-to-day way that the “victims” do whom Dr. Ryan writes about. At times in the pages of this book the author gets rather angry, even shrill. He seems to be trying to grab us all by the scruff of our necks—so that we can leave our libraries and laboratories and look at others, whose struggle requires justice not pity. He also seems to want us to stop and think about *our* predicament: we, too, have a stake in making this society more decent, more responsive to the needs that every single human being ought to have filled as a political and social right in a nation as rich as this one. The longer millions suffer, the more many of us more privileged citizens continue to feel queasy, feel ashamed—and feel obligated to vent our vague unease on *them*, who become to us irritants of a certain status quo our consciences seek after. So, blaming them, the victims, we become victims ourselves. And meanwhile, as I once heard a black youth say: “They keep on raking it in, the bossmen all over America.”

It is the genius of this nation that more and more of us are, within definite limits, living like bossmen, even if we are not really “on top,” so to speak. But those limits are indeed definite, and they demand stretching—until millions now essentially excluded come in and enjoy (in their chosen ways) what we have to enjoy. Dr. Ryan says that if “they” were convincingly beckoned they would be quite able to respond. I suppose

there is one good way to prove him right or wrong: try them, try it—try making the political and economic changes that will shift significantly in the direction of the poor the present concentrations of wealth and power in this nation.

Black Americans: A Psychological Analysis. By E. Earl Baughman. Foreword by M. Brewster Smith. New York: Academic Press, 1971. Pp. xxii+113. \$6.95.

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This small book, aimed at providing undergraduate psychology students with a comprehensive psychological analysis of black Americans, is based heavily on research reported in *Negro and White Children: A Psychological Study in the Rural South* (New York: Academic Press, 1968) which Baughman coauthored with W. Grant Dahlstrom. The findings of the earlier research are presented in a more understandable form along with some of the more recent research on the psychological characteristics of blacks and whites. The student is introduced to a relatively wide range of controversial issues, that is, racial differences in "Intelligence," "Scholastic Performance," "Self Esteem," "Rage and Aggression," "Psychopathology," and "Socialization and the Family." Most chapters review the major theoretical controversies and issues characterizing the particular area along with the results of research comparing blacks and whites. Even though much of the research remains clouded with ambiguous data, Baughman's insistence on systematic racial comparisons should be welcomed in a field of inquiry which has frequently relied on case studies and anecdotal data.

While the strength of this book lies with the author's insistence on confronting theoretical controversies with empirical data, its major weakness is in the exclusion of much significant research on blacks and whites. Three chapters on the affective states of blacks—"Self Esteem," "Rage and Aggression," and "Psychopathology"—make little use of the extensive research literature on alienation, anomie, alcoholism, suicide and homicide, and mental illness. The chapter on "Scholastic Achievement" makes only a casual reference to the Coleman report. (It is excluded because Baughman was able to apply more stringent controls in gathering his own data.) The chapter on "Socialization and the Family," based principally on the Moynihan Report, ignores many of the criticisms made of that study and leads the reader to unquestioningly accept many of the traditional assumptions associating instability and pathology with the black female-headed family. That is, Baughman takes the position that racial differences in family structure and size and the resulting socialization patterns account for many differences in the psychological characteristics of blacks and whites.

After an initial statement pointing to the relevance of social and economic status for the psychology of blacks and whites, and to the correlation between socioeconomic status and race, these crucial structural factors are largely ignored. The social and economic status of Baughman's sample is neither identified nor controlled when racial comparisons are made. While we can not be sure, one imagines that the blacks were generally lower in social status than the whites. If this is the case, and if it is also true that social status is related to psychological characteristics, then it comes as no surprise that Baughman's data indicate that "in a number of behavioral domains the difficulties of blacks exceed those of whites" (p. 90).

The absence of any examination of relationship between structural factors and psychological characteristics is reflected in the public policy suggestions which Baughman offers. These are aimed at actions which the black community and black leadership should take, and little attention is given to the institutional processes which have maintained the marginal social and economic positions of blacks in America. Given the controversial nature of the subjects discussed, the ambiguous nature of most of the empirical research, and the absence of an examination of structural factors, I find it somewhat difficult to agree with such suggestions as, it is up to blacks to "fight against the unfair application of comparative IQ data" (p. 13), or that "blacks must become more assertive and shed the postal clerk syndrome; yet they must also skillfully manipulate forces so as to prevent counteraggression that could destroy the progress that has been made" (p. 64), or that blacks must renounce the "low-life" associated with the nonrespectable family.

Baughman's suggestions regarding education are perhaps most disturbing. After arguing that integration of public schools has demoralizing effects on black children who must compete with the better-prepared white children, he suggests that it might be better to integrate schools beginning at the first grade and proceed in a "grade-per-year" desegregation plan. As an alternative to the immediate integration of schools, Baughman suggests that black leaders should insist on the expansion of preschool training in the black community. Given the small amount of data supporting the hypotheses that integration has demoralizing effects on black children, as well as the evidence indicating that the positive effects of programs such as Head Start are greatly reduced as children progress through inadequate public schools, I have difficulty accepting either of these suggestions.

In the conclusion of the chapter on psychopathology, Baughman includes a short discussion of psychotherapists. He writes that "the highest priority, we believe, should be given to the recruitment and training of blacks to deliver such services [psychotherapy for black patients]; no matter how well intentioned the white professional may be, he is likely to encounter suspicion and hostility in his work with blacks that will subvert his effectiveness" (p. 75). This book, despite its good intentions, may arouse suspicion and hostility in its black readers.

Black Life and Culture in the United States. Edited by Rhoda L. Goldstein. New York: Thomas Y. Crowell Co., 1971. Pp. xiii+400. \$6.95.

Joyce A. Ladner

Howard University

Almost three decades ago the Swedish sociologist-economist, Gunnar Myrdal, offered the proposition that America's race problem constitutes a "moral dilemma," arising from the contradiction between the ideals expressed in the American Creed and the actual racist behavior whites practice toward blacks. I was reminded of Myrdal's theory while reading Rhoda Goldstein's introduction to *Black Life and Culture in the United States*, where she questions the contradictory nature of American society as manifested in the way it handles its race problem. She writes: "How did the fate of black people become an inexorable part of American history—and how was it possible to ignore their role for so long" (p. 2). America's race problem never should have been couched within *moral* terms because Afro-Americans have always existed within and been subjected to the framework, policies, and practices of an "amoral" society—a society which has never expressed very much collective guilt about its conduct toward its largest minority group. Hence, most of the authors of this anthology have used an alternative analysis of black life and culture, and have concentrated on the institutional racist character of American society as being the major causative factor, instead of the "moral" dilemma proposition. In this sense, perhaps the contributors to this volume understand the nature of the problem better than Goldstein, the editor.

Black Life and Culture in the United States is a collection of previously unpublished essays written by a group of predominantly black authors (African, Caribbean, and Afro-American). The volume covers basically four historical periods: (1) precolonial West Africa, (2) slavery, (3) reconstruction, and (4) various themes on contemporary black American life. Its multidisciplinary focus covers history, sociology, black art and drama, psychology, politics, education, and community organization. Unfortunately, areas such as economics, literature, and topical areas including an examination of black culture, the family, and Pan-Africanism have been omitted.

Because this volume grows out of a course which Goldstein offered at Douglas College, it tends to be uneven in quality, content, and style. The volume also tends to be diffused and is not organized around particular themes. Rather, one gets a potpourri of topics. It also has a regional flavor because most of the contributors are drawn from the Rutgers University area and nearby communities. I doubt this could have been avoided because, according to Goldstein, the book "is intended to be, in part, a platform for what a number of varied black people, who have studied it and lived it, have to say about aspects of the black experience" (p. 5). Hence, the most important contribution this volume makes is that it allows blacks to interpret the black experience, thereby bringing to bear a very *positive*

perspective to the values, attitudes, beliefs, and behavior patterns of Afro-Americans. It is refreshing to read a work on blacks which does not focus on the so-called pathology, deviance, matriarchy, and male-castration themes. According to the editor, "sociologists, like historians, are learning that much of their past analyses were facile, over-simplified, and at times biased when they dealt with black Americans" (p. 5). Hence, Goldstein, a white sociologist, understands the necessity of allowing black people who have "lived the life" to write their own interpretations of these social phenomena.

Although the topics covered in this anthology vary widely, a number of relevant contemporary issues are raised in some of the essays. The controversy over *class* versus *race* is one of the themes debated by Theodore Taylor and Lennox S. Hinds. A Rutgers University law student, Hinds writes one of the more insightful analyses, entitled "The Relevance of the Past to the Present." Other authors have dealt with *cultural nationalism* versus *revolutionary nationalism* and, in general, resolved it in favor of the latter. Other exceptional works were written by Herbert Aptheker (one of the few whites who contributed to this anthology), on "Afro-American Superiority: A Neglected Theme in the Literature." Aptheker's analysis of historical data depicts a *superior*, not *inferior*, self-concept expressed in the writings of blacks during the 18th, 19th, and early 20th centuries. In a very interesting essay, Julius M. Waiguchu, a Kenyan, argues that the survival of African cultural traits in America is a function of the genetic process and the transmitted cultural traits. Ivan Vansertima, a Guyanese, presents an incisive analysis of the influence of African languages on the language of African descendants, and he refutes some of the common myths about the so-called bankruptcy of black language. William H. Phillips, Cecilia Drewry and Henry M. Drewry, Samuel D. Proctor, and Geoffrey Hendricks also present stimulating articles on survival techniques, black drama, slavery, the black middle class, and black art.

Rhoda Goldstein is to be congratulated for her recognition of the necessity for blacks from all walks of life to interpret the black experience.

The Black Middle Class. By Sidney Kronus. Columbus, Ohio: Charles Merrill Publishing Co., 1971. Pp. v+182. \$2.25 (paper).

William H. Turner

Southern University

The thesis of this short book is that the contemporary black middle class in America does not conform to the Frazerian model or to that framework offered more recently by Nathan Hare. Kronus focuses on blacks in America and offers several propositions on "what the black middle class is really like in the third quarter of the twentieth century." The book claims to be an "empirical" refutation of Frazier's and Hare's themes be-

cause, unlike these works, it was not based on "hearsay, personal anecdotes, and unsystematic content analysis of the mass media" (p. 14). The propositions develop around the author's description of the black middle class: (1) the black middle class of the 1960s and 1970s does not conform to earlier descriptions of them, namely, Frazier's *Black Bourgeoisie* and Hare's *Black Anglo Saxons*, (2) that intraclass structural differentiation excludes blue-collar blacks in the working definition of the black middle class, and that (3) the black middle class can be profitably typed into categories along some dimensions of interracial and intraracial sentiments. In the major section of the book, Kronus uses an account of a *typical* middle-class black man to portray the dominant themes of the collective middle class (p. 16; italics added).

His primary source of data is "a sample of sixty Black males employed in white-collar occupations and twenty Black males employed in blue-collar occupations." The respondents lived in an all-black residential area of Chicago's South Side.

Students familiar with the literature will not be surprised by the author's use of social types. His types include: "Benevolents," "Uncle Toms," "Race Men," and "World Haters." The basis for his typology is not clear, but Kronus has obviously followed the well-trod method of scoring respondents above or below mean scores on statements dealing with attitudes toward whites and blacks. A glaring weakness of this typology is its inability to demonstrate the precise meaning (operational definition) of each category. Moreover, the labels themselves are inherently ambiguous at best and pejorative at worst. The subsequent discussion of these types reflects little sensitivity or "inside" knowledge of certain subtleties and the double-entendre ambiguities of responses which blacks reveal on open-ended questionnaires, such as the one used by Kronus (see appendix A, esp. pp. 151-52). In my view as a black man, these categories reflect the worst kind of sociological vulgarism; the author implies that they were derived from the technical language of Myrdal, Allport, Rosenzweig, and especially the work of DuBois—all of whom the author uses to formulate his typology. The self-indulgent and obscurantist nature of this typology reaches the epitome of imprecision when the author asserts categorically and consistently that skin color (gradations of pigmentation) is still a major variable in the intraracial dispositions of his so-called "new Negro." Frazier and Hare emphasized this matter because at that time blacks, in aping white culture believed that: "If you're bright, you're all right, if you're brown stick around, [but] if you're Black, get back." At the present time, however, blacks' dispositions are couched in attitudinal and behavioral perceptions of blackness and not outward manifestations of that "phenomena"—that is, the blackest black is not black just because he happens to be darker. Despite such a turn of events, Kronus contends that the majority of black people in America still relate to one another on the basis of skin color . . . "marriage to lighter colored females." This results in considerable vagueness in the author's conclusions, especially those concerning the "Race Men" types who are the

darkest of his sample and who also exemplify the most positive attitudes toward other blacks, and who also marry dark-skinned black women.

In addition to presenting a typological framework that is no more than an adaptation of Marx's *Protest and Prejudice*, the book offers few conclusions that depart from past empirical works on the black middle class. The miscellany is nothing new. For example, he *reveals* that youthfulness is associated positively with cosmopolitanism, that blacks manifest an exacerbated hostility toward whites, and that the events of the civil rights era marked a "coming-out" of latent black militancy. Such events as these, such encounters with white racism, and the subsequent awakening of the black middle class reinforces (rather than refutes) the themes of Frazier and Hare—for the thesis of Frazier was that the black middle class lives in a world of make-believe.

Frazier and Hare discussed the behavioral aspects of the black middle class, especially the expressive mechanisms of tension management and stress. Kronus *departs* again from these offerings by defining stress and symptoms of tension in terms of such declamatory criteria as "status-conferring liquor," consumption of alcohol, and the incidence of chest colds immediately prior to the interview. One wonders about the validity of the generalizations given a Saturday morning interview after a Labor Day weekend, not to mention the biasing effects of Chicago winters. The black middle class value of "conspicuous consumption," reports Kronus, is on the decline. In light of present-day buying power, such a finding takes few of us by surprise. Then again, they could be conspicuously consuming less white-oriented goods, such as dashikis and the various other paraphernalia associated with the "militant" stance. The gist of Frazier's classic was that middle-class blacks are not bearing their burden in the advancement of black people as a race class. Moreover, I see little virtue in this book as a disclaimer, when one studies 80 blacks in Chicago and pretends that it serves as an adequate representation of the growing and heterogeneous black middle class.

In my opinion, the book's potential value may be for students who need an introduction to the varieties of empirical works about black Americans. It offers little to professional sociologists and other behavioral scientists seeking theoretically and empirically sound alternatives. It represents a kind of methodological arbitrariness in which we all could indulge and present as some significant revelation. The major contribution of this work is the introductory chapter, where the author offers a splendid review of the literature of the black middle class. Kronus's critique of Frazier is commendable, but his takeoff and his alternative methodology raises more complex questions and is perhaps more obfuscating than lucid. It fails miserably to refute Frazier's claims, which evolved around the notion that the black middle class is largely "without substance" and that they fail to bear their responsibility to the black *lumpenproletariat*.

Then too, as a black man and as a sociologist, I take serious academic and ideological issue with Kronus's claim that "negative feelings towards whites is highly related to heavy alcohol consumption" (p. 131). On the

"brighter" side, however, such works challenge "DuBois's talented tenth" to embark on our own homework, to construct our own categorizations, and to attempt to explain the black reality in America. We trust that publishers will be open to our views and our *right* to be *wrong*.

Tomorrow's Tomorrow: The Black Woman. By Joyce A. Ladner. Garden City, N.Y.: Doubleday & Co., 1971. Pp. xxvi+304. \$6.95.

Carol L. Adams

Northeastern Illinois University

This work is an examination of the meaning of womanhood in the black community. It begins with a sociohistorical look at black womanhood, pointing out the rich African traditions that helped to mold the contemporary black woman. Joyce Ladner analyzes the effects of institutionalized racism on the black family and the survival-oriented black woman that emerged in response to the vicissitudes of being black in America.

The period of female adolescence is the major focus of this study. The data were collected from interviews with black teenage girls residing in and around the Pruitt-Igoe housing project in Saint Louis, Missouri. These interviews dealt with the life histories of the girls and their attitudes and behavior as they approach womanhood.

The contributions made by Ladner in *Tomorrow's Tomorrow* fall into three general, although not mutually exclusive, categories: (1) the emergence and definition of a black conceptual framework; (2) the destruction of stereotypic myths about blacks in general, and black women in particular; (3) the recognition of some social characteristics about black family life and black women traditionally overlooked by social scientists. In a sense, it is the first accomplishment which makes the other two possible.

Conceptual framework.—Essential to the methodology Ladner employs here is the idea that blacks must formulate their own definitions and concepts of social phenomena from a perspective untainted by the ethnocentricity and cultural arrogance of those who seek to compare aspects of black culture to the white middle-class model. This process is referred to as *decolonization*—"the refusal to allow the oppressor to define the problems and solutions of the Oppressed" (p. 272).

With the advancement of the idea that a different set of moral codes regulates the sexual behavior of black people, the author questions the validity of the "moral-immoral dichotomy" in which sex is framed by the dominant society. Ladner suggests that the attitudes of low-income black people are certainly more realistic, more human. Within this context, another concept viewed as inappropriate when studying the black community is "illegitimacy." She observes that the low-income black community holds an inherent value that no child can be "illegal." The child is seen as having the right to exist and representing the fulfillment of

womanhood, thus neither the mother nor the child is degraded and stigmatized.

Ladner recognizes that the notion of an "objective, value-free science" is hypocritical and states her biases at the outset: "I decided whose side I was on and resolved within myself that as a black social scientist I must take a stand and that there could be no value-free sanctuary for me" (p. xviii).

Central to the author's interpretation of the black condition is her realization of the effect of the American social system, through its institutional racism, on the black community. Ladner contends that the contemporary black community is a *product* rather than the cause of American social policy.

Destruction of myths.—Ladner attacks many of the dominant stereotypic notions about blacks which are propagated and perpetuated by its scholars and philosophers, for instance:

1. The allegation that the black community is inherently pathological is denounced as the invention of members of the dominant society who seek to obtain a "superior" status by creating an "inferior" class.

2. The charge that black people suffer from an inability to defer gratification is seen as stemming from the Protestant ethic of self-denial which white society itself "deviates" from in its preoccupation with materialistic goods as manifested in patterns of conspicuous consumption.

3. The notion of black self-hatred or low self-esteem is viewed not as an idea that emerges from blacks themselves but as a concept falling "within the realm of institutional subjugation that is designed to perpetuate an oppressive class" (p. 100). The girls in Ladner's sample were more prone to express a disdain for those individuals and institutions which exploited and oppressed them than for themselves.

4. The more prevalent myth abounding in social science literature about the black woman is that which depicts her as a matriarch and an emasculator of black men. From this perspective, the strength and the resourcefulness of the black woman is portrayed as a negative thing and researchers once again point to the "pathologies" of the victim. Thus, instead of the society which has institutionally subjugated the black man and denied him the opportunity to fulfill the responsibilities perceived as central to the male role, the black woman is seen as the villain. Ladner does not pretend that there are no frustrations in the relationships between black men and women, but places the locus of these tensions in external forces impinging upon the black community rather than on the defects of the black woman.

Characteristics of black family life.—Repeatedly, the author emphasizes the need to acknowledge as a positive force the strength and stability which enabled blacks in this country to survive. The ability of black people to cope with and adapt to the conditions they experience is viewed as testimony to the control exerted by blacks over their environment.

Marriage among black people, according to Ladner, is based more upon rational than idealistic factors such as love and emotional security. A

realistic assessment of the chances for a marriage to survive in the tension-producing environment created for blacks by the dominant society is made before marriage occurs. Ladner points out that the traditional reasons for marriage often prove dysfunctional even for those who espouse their validity.

Finally, she advances the notion that black women are now serving as role models for white women who are beginning to question such things as the institution of marriage, the concept of illegitimacy, and the general moral code traditionally associated with this society. It is interesting to note that much of the behavior characterized as deviant when practiced in the black community is now being sanctioned by the majority groups and thus becoming "legitimate."

The Japanese and the haoles of Honolulu: Durable Group Interaction. By Frederick Samuels. New Haven, Conn.: College & University Press, 1970. Pp. 206. \$6.00 (cloth); \$2.95 (paper).

T. Scott Miyakawa

Boston University

It should be noted at the outset that I have no first-hand knowledge of Caucasian American and Japanese American relations in Honolulu. I do have, however, some familiarity with the Japanese American experience in the United States and with certain racial and ethnic problems in several foreign countries and this interesting book is intended to be more than an empirical study of Honolulu. The author is concerned with developing a more adequate theory of group relations and with its application in understanding and ameliorating intergroup tensions everywhere.

In the first section, Professor Samuels briefly reviews the Hawaiian situation and some of the research techniques used in the earlier studies, and discusses his own findings on the relations between Caucasian Americans and Japanese Americans, the two largest racial groups in Hawaii. He relied on interviews, participant observation, demographic data, and documentary records, and partially capitalized on his previous experience with small-group research.

His results generally confirmed what we might have anticipated, but also showed some significant variations and opened the possibilities for better explanations. Thus, the Caucasian American long-term residents of integrated neighborhoods felt closer to Japanese Americans than they did to other non-Caucasian groups, while the Caucasian residents of all-Caucasian ghettos with few social contacts with Asian Americans preferred the Chinese Americans. Both Caucasian Americans and Japanese Americans commented that more should be done to enhance the quality of their relationships, and both were critical of their own group characteristics, as well as those of the other group, which they regarded as detrimental to

better relations. This self-awareness, Samuels believes, bodes well for the continuing relations.

It seems that, for the Issei, and perhaps to a less extent, for the Nisei, the feeling of distance from "darker" racial groups is as much a cultural as a racial bias. They consider persons of mixed Hawaiian and Asian ancestry, who had associated with Orientals as part of their primary groups, while they regarded others of identical parentage and physical appearance with only a few contacts with Asian Americans as outsiders.

The findings partly support the claim that Hawaii is racially more tolerant than the mainland. The long-term resident Caucasian Americans seem to be less prejudiced against most non-Caucasians than recent arrivals from the Continent, particularly the families of the military personnel. Samuels asks if the present relations can survive the severalfold increase in the numbers of mainland tourists anticipated in the near future.

In the second half of the book, Samuels attempts to develop a theory of interaction among durable ethnic, racial, and religious groups, drawing on his Honolulu empirical data and his earlier small-group research. The 13 variables he regards as basic were "conceptualized in four subsystems within a feedback system" (p. 144). The subsystems in turn include such familiar factors as the frontiers of group interaction, sex ratios, group image, and "reward-distribution group-types" (p. 144) which, he believes, provide a new perspective. The theory, as he observes, needs further testing and development.

For several reasons, parts of the study convey considerable naïveté. First, virtually neglected are mainland Japanese American materials which might have strengthened the Samuels's theory and the formulation of his hypotheses, some of which seemed tautological. Second, at least for this mainlander (and some Hawaiians), the colloquial use of "Japanese" in the text, and even in the title, is annoying. It is applied to Japanese, Issei, Nisei, and Japanese Americans as well as to cultural, national, and racial groups, and to language. Although this is done in Hawaii, where a partial understanding of specific meaning can generally be assumed, a scholarly work should avoid such confusion. For some years, social scientists and civic leaders have been trying to educate mainland Americans to distinguish between Asian Americans and Asians and for non-Americans to differentiate the American ideals of citizenship from the Old World traditions of permanent minority *nationalities* and of pre-nation-state *cultural subgroups*.

Readers may also wonder whether using the percentage of professionals in a racial group to determine its status does not seriously distort the Honolulu picture. Caucasian Americans dominate banking, industry, plantations, transportation, communications, and land-holding firms. This sheer power is only slightly modified by the growth of the Japanese American political influence. While multiracial patronage of professionals and interaction in integrated neighborhoods is extremely important, the role of power cannot be ignored.

Samuels found that the Japanese American battlefield experience in

Europe was a significant factor in their rising status, but does not mention their military service on the Pacific front and its consequences. Almost half of the Japanese American soldiers in World War II were on the Pacific and Southeast Asian fronts, especially in the front intelligence units.

Despite these and other limitations, this book should have a wide appeal to both laymen and students interested in race relations, social change, acculturation, group relations, and East Asian studies. It encourages both theorists and empiricists to pay closer attention to the links between the larger associations and the individual and to the social structure of reward distribution. It is a pleasure to note that the author writes clearly and effectively.

Mexican Americans. By Joan W. Moore with Alfredo Cuellar. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1970. Pp. xii+172. \$5.95 (cloth); \$3.50 (paper).

John H. Burma

California State Polytechnic College at Pomona

As one of the Prentice-Hall Ethnic Groups in American Life Series, this volume more than holds its own. The authors would be the first to admit that in 160 pages they could not produce a complete compendium of information on Mexican Americans. What they have done in eight short chapters is to give a superior overview and summary of the major aspects of the Chicano group.

This book includes an excellent section on the historical roots of Mexican American people, an inclusion which will be useful to Anglos and much appreciated by Chicanos. In addition to the historical aspects, there is a good "population profile" section which describes the demographic characteristics of the group. Economic and occupational problems and the resulting poverty are described, as are the religious patterns of the Chicano community. Surprisingly, there is a short, useful section on the Chicano and law enforcement. The Mexican American family is described, including a neat suggestion of special functions of the family in an "economy of destitution," and the suggestion that familism—usually highly praised—may be dysfunctional for some Chicano individuals and families by inhibiting both geographic and social mobility, and by encouraging nepotism in a nonfamilistic economic system.

The discussion on class and power is superior, and the description of the beginnings of real upward mobility are heartening. The section on Mexican American culture and values is insightful. It deals with familism, planning for the future, clannishness, ethnic solidarity (*la raza*), and significant generational differences, all of which are highly controversial topics. The selection on "The Politicalization of Mexican Americans" will be enlightening for Anglos and Chicanos alike.

The section on immigration also is strong, dealing with a topic on which both Chicanos and Anglos tend to be hazy, or to confuse partial truths with fact. Moore, writing in 1969-70, suggests that the Mexican American community itself may change and grow more restrictionist, as it sees the effect of illegal immigrants on the already poor job opportunities for its members. It is appropriate to report that on October 10, 1971 a conference of the six major Mexican American organizations voted to drop its support of Mrs. Bafuelos for Treasurer of the United States, according to newspaper accounts, because immigration agents found illegal aliens working at her Los Angeles food-processing plant.

The section on education is good, but too brief, although this handicap is partly overcome by a later discussion of the use and misuse of the Spanish language. Greater mention of attempts to "solve" the problem of Chicano dropouts should have been included.

The most important single aspect of Mexican American life today may quite possibly be the Chicano movement. This topic receives needed explanation in an unbiased fashion. Too frequently the impression is given that all Mexican Americans are an active part of the Chicano Movement, which is just as incorrect as reporting that all "hard hats" support the war in Vietnam. Regardless of such overstatements, nothing has so stirred such a large number of young Mexican Americans as "The Movement," and the relatively brief analysis found in this book is a distinct contribution to understanding the rise of *chicanismo* throughout the Southwest.

As supplementary reading for courses dealing with ethnic relations, this book is recommended.

Immigration and Race Relations in Britain, 1960-1967. By Sheila Patterson. London: Oxford University Press, for the Institute of Race Relations, 1969. Pp. xviii+460. \$10.50.

Robin Ward

University of Manchester

Sheila Patterson's recent book, together with the better-known report of the Survey of Race Relations in Britain (E. J. B. Rose and associates, *Colour and Citizenship*, London, Oxford University Press, 1969), provides a comprehensive account of immigration and race relations in Britain during the crucial years of the 1960s. The criticism of *Colour and Citizenship* by some sociologists in England that it provided a "white liberal" ideological interpretation of current events and relevant data is inapplicable to Patterson's book, which for the most part eschews analysis entirely. Basically, it consists of a kit of materials from which researchers and other interested persons can draw. It does not set out to answer many of the questions, but rather to document the wide range of empirical

materials that must be accounted for in any interpretive understanding of race relations in Britain. Few will "read" it in a conventional sense, but it should perform a most useful role in enabling theoretical analyses to be more empirically grounded.

Thus, American readers will find that it illuminates important themes to which it is not specifically addressed, and none more significant, or more frequently discussed in Britain, than the extent to which the present state of race relations in the United States can be taken as a foretaste of things to come in England. Thus, given that most black people in Britain are basically economic immigrants and their families, and that they have settled in areas of high employment demand which have a correspondingly severe shortage of housing, a crucial factor structuring race relations in Britain is the local authority's practices in its role as landlord. Where up to 50% of the local housing stock is controlled by a bureaucratic landlord and recognized as a destination not only for black families and those "known to the social services," but a large section of the traditional working class, there is considerable potential for local authorities, in line with the wishes of many black residents, to avoid the ethnic concentrations typical of cities in the United States. Patterson's coverage of this crucial factor neatly dovetails such empirical data as were available with successive recommendations for modifications in policy.

Other areas where the contrasting situation of black minorities in Britain and the United States can be seen are in the role of black police, in the structure of antiblack organizations, and in the development of educational policies. On the last point, Patterson notes that the Conservative Minister of Education was "undoubtedly impressed" by the U.S. Civil Rights movement's strong support for a quota system, whereby the children of certain minority groups should not comprise more than a fixed proportion in schools. However, the resulting policy recommendation of "spreading the children" where they (i.e., "immigrant" children) formed more than one-third of the school population, produced a rare unanimity among those opposing it, including almost all local education authorities, white liberals, and most shades of black opinion. Even in Wolverhampton, scene of Enoch Powell's outraged cry about the one white child in a class of black children, the authority decided against dispersal.

The period Patterson covers, 1960-67, is crucial in the development of race relations in Britain. The conveniently cheap policy of complete *laissez-faire* with regard to immigration and race relations gave way in 1962 to immigration control, and in 1965 to a vestigial Race Relations Act. Its extension to cover the vital areas of employment and housing under the stimulation of Roy Jenkins was already under way before 1967 was out, and so was the strengthening of provision for voluntary activities to secure stable race relations. What comes out clearly from Patterson's presentation is the range of possible interpretations that can be placed on the political and administrative actions of this brief period, which provided a severe control on numbers, a minimum of regulation of the nature

of racial contacts, and a network of local organizations to try and patch up specific problems as they cropped up.

Official policy during this period has been characterized as a series of racist measures whose effects were scarcely modified by a token gesture on behalf of black residents; or, alternatively, as the result of a genuine fear that the chances of integration of black people already in the U.K. would be jeopardized by the continued uncontrolled influx, particularly from India and Pakistan. Thus, Harold Wilson's justification for restricting immigration even further was to forestall "a social explosion in this country of the kind we have seen abroad" (p. 47). But whatever the stated aims, the administration of the policy was such that colored (but not white) applicants experienced the greatest possible difficulty obtaining forms required for the admission of dependents.

The degree of intended racism in these policies may be irrelevant. As Patterson shows, it is symptomatic of race relations in Britain that practices not racist in intention have led to the more extensive adoption of racial attitudes. The clash between the hardening of stereotypes about color and the systematic movement, especially of second-generation immigrants, into a wider range of jobs and better housing will provide material for a similar book describing race relations in the 1970s.

Race and Racism. Edited by Sami Zubaida. Foreword by T. B. Bottomore. Explorations in Sociology, no. 1. New York: Barnes & Noble, 1970. Pp. xiii+185. \$5.75 (cloth); \$3.00 (paper).

R. A. Schermerhorn

Case Western Reserve University

This volume consists of seven essays selected from a larger number of conference papers delivered at the British Sociological Association; all of them deal, in one way or another, with race relations and their proper analysis. Taken as a whole, they show that this is now a subject in British sociology which is no longer treated as a separate field of investigation, but one whose elements and explanatory propositions are derived from the total range of sociological theory. The liberal familiarity with American sociological writings displayed by the authors is one which, I fear, has little counterpart on this side of the Atlantic with respect to British social science. For Americans trying to correct this astigmatism, this book might serve as an elegant entering wedge. But its value is not only for the neophyte, or for the specialist in race relations, but for all those with a deep interest in social conflict and its myriad forms.

Tentatively, it is possible to subdivide these lectures into two main divisions: *theoretical* papers by Michael Banton, John Rex, David Lockwood, and Harold Wolpe; and *empirical* studies by John R. Lambert, Sheila Allen, and Nicholas Deakin.

Banton devotes much, if not most, of his attention to the historical antecedents of racism (e.g., the term was first systematically used by Robert Knox in 1850, even before Gobineau); in addition, he presents a useful distinction between racism and racialism—the former being “the doctrine that a man’s behaviour is determined by stable inherited characters deriving from separate racial stocks having distinctive attributes and usually considered to stand to one another in relations of superiority and inferiority,” while racialism is (in Margaret Nicholson’s phrase) simply “the practice of the doctrine” (of racism). John Rex takes an analytic stand and asserts that race relations problems presuppose three components: a stratification factor, an ascriptive factor applied to those who perform distinctive social roles, and a deterministic belief system. By the last he means a set of beliefs which, when “applied to the justification of a social structure, the social structure comes to be seen as inevitable and unalterable, and transition from one kind of role to another may be held to be impossible.” Rex contends that this deterministic belief system, like other belief systems, may have causal efficacy in the social world—a view considerably at variance with Harold Wolpe’s neo-Marxian approach in the last chapter where it is strongly implied, if not explicitly stated, that racism or race prejudice does not have the causal effects usually imputed to it; what really counts is the organization of economic power. Thus “the relationships between racial groups in South Africa must be analyzed in terms of the ongoing relationships of strata and classes, which are structured in terms of differential access to resources, organization, skills, status, and so on.”

In his imaginative discussion of “plural society” as distinguished from a society polarized by the conflicts described by the Marxians, David Lockwood concludes that the difference is a fundamental one. In the plural society, he declares, “conflict will tend to centre on the dominance of one group over another rather than on the system of domination as such. So that by contrast with class revolution, conflict in plural society, however violent, is not first and foremost directed at an alteration in the structure of power and deference but rather at the usurpation of power and deference by one section of the community to the disadvantage of the other.” This implies, of course, that a “revolution” of the Negro in western societies is “clearly untenable.”

In the empirical studies, John R. Lambert describes his research on the police and race relations in the light of earlier investigations from both Britain and the United States. He quotes an ex-policeman appearing in a talk show before a British audience as saying that color prejudice among the bobbies is close to 100%. On the other hand, he maintains that “the lack of hostility between police and the white community in this country is remarkable and valuable.” Some of his other evidence seems to show that this kind of solidarity is apparently a function of a widespread etiquette of civility traditionally practiced by British citizens interacting with the police. “How the offender responds to police authority, what *respect* he shows the individual policeman, will determine the action that

is taken." A challenging attitude on the part of a suspect frequently leads to peremptory arrest. Lambert explains much police conduct in racially mixed situations by pointing out how law officers, highly mindful of majority dislikes or fears of racial minorities, believe the majority is the public they must serve. Consequently, "many police administrators feel they have gone about as far as they can go without endangering their own relationships with the larger community."

The more localized studies of Sheila Allen and Nicholas Deakin are almost barometric indicators of the present state of race relations in medium-sized communities outside London. In her study of an industrial community (Bradford), Allen found that the unions made little effort to enlist colored workers into labor organizations; that the Pakistanis (whose living conditions she examined in some detail) had wages averaging 4 pounds less per week than the national average for men over 21; that most Pakistanis were convinced that the English workers and union leaders were indifferent if not hostile to them; that cleavage between immigrants and the management became so wide that a small group of Pakistanis struck independent of the union; and that one major source of frustration for the migrants was the paucity of housing which made it impossible for them to live comfortably. Viewing the entire community situation, Allen maintains that union leaders have little awareness of the increasing dissatisfaction of colored workers whose needs are consistently ignored, making it more likely that in time, "economic and social issues" will be "transformed into racial conflicts."

In Deakin's study of Wolverhampton, while immigrants from India, Pakistan, and the West Indies made up only 4.8% of the population, there were substantial minority groups such as the Irish and Poles. Deakin's respondents describe these European immigrants as being in conflict with colored immigrants or as treating the latter unfairly. Only half the leaders who acted as informants for Deakin thought that "minorities have obligation to integrate," and 65% felt that the racial situation was bound to deteriorate. This did not destroy their blandness or indifference since only 38% thought of the immigration issue as having any real importance. Deakin did not consider the need of such leaders to put a good face on their community; had he questioned a representative sample of the citizenry, the results might have been considerably different. Was Enoch Powell whipping up an issue that had low salience in Wolverhampton, or was he exploiting an already rising tide that bore him on to fortune? This question is not answered in an otherwise highly instructive public opinion study.

In spite of its many topics, the book as a whole has a remarkable unity, and this is captured well by the editor, Sami Zubaida, in his introductory chapter. Zubaida's coherent summary will repay rereading at the close. The table of contributors and the index are carefully done. As a whole, the book is a well-reasoned and mature guide for the analysis of race relations at the present stage of sociological knowledge. The only omission of consequence is Harry Hoetink's sociopsychological theory about the

origin of race attitudes—a viewpoint needed to supplement the earlier theoretical chapters. This, of course, does not detract from the solid contributions evident from the first page to the last. Especially welcome is the brevity with which the authors state their case. They show us it is not necessary to be voluble.

Classes et associations d'âge en Afrique de l'Ouest. Edited by Denise Paulme. Paris: Librairie Plon, 1971. Pp. 354. (Paper.)

Remi Clignet

Northwestern University

At a time when industrialized societies are confronted with an increased generation gap and with accentuated conflicts between age groups, it might be useful to examine how more traditional social systems have used age differentiation as a mechanism of social integration. Such was the theme of a symposium organized in 1969 by the Ecole Pratique des Hautes Etudes in Paris, the proceedings of which have been published under the direction of Denise Paulme.

Two preoccupations underlie the present volume. First, it is important to identify the conditions under which age classes appear in a traditional structure. Second, the problem remains to assess variations in the nature and functions served by this particular mechanism of social integration. The conclusions that the editor draws concerning the origin and the functions of age classes are derived from an examination of a limited number of traditional societies in Senegal, Dahomey, Togo, Ivory Coast, Niger, and Cameroun.

From a general perspective, age classes do appear among peoples simultaneously characterized by an absence of a highly integrated system of political authority, an absence of a marked system of social stratification, and finally, by the existence of heterogeneous kin groups. The main function of age classes is then to reconcile the diverging forces exerted on the loyalties that an individual owes to his familial group on the one hand and to the village on the other. The existence of such classes induces strong feelings of solidarity among the members of a same age group. It similarly insures a marked interdependence among the members of differing groups. The functions of these groups vary with the extent of the patterns of division of labor of the society at large. The lower this division of labor, the more numerous the functions of age classes. Finally, the number and the importance of age classes tends to increase with the severity of the conflicts opposing lineages to one another.

As indicated, the second preoccupation of the editor is to show the variety of age-class systems found in the African context. One system is cyclical in nature insofar as each group is comprised of the offspring of the immediately preceding group. Therefore, the number of groups is

limited and each one receives a name which remains the same throughout history. Depending on the total number of classes, the cyclical system induces an emphasis alternatively on the solidary or on the competitive ties between fathers and sons. By contrast, groups in the linear system are comprised of all individuals of an age to be initiated. Such a system is not associated with a limitation in the number of groups nor in the names that they are allowed to have. Its organization involves less significant forms of competitive relations than the organization of the cyclical system.

Since each society introduces variations in a general system of age classes, the book contains 11 descriptions of distinctive systems of age classes. Although each description is interesting in itself, and although many of them indicate how the organization of age groups has been affected by social change and modernization, it is still a pity that the sample of societies studied reflects more the relative knowledge acquired by anthropologists than a systematic range of variations in the phenomenon at hand. In this sense, such a study shows our ignorance as much as our knowledge. As the editor writes at the end of her introduction, it is only a first step toward more rigorous comparisons.

Pourquoi l'épouser? Et autres essais. By Luc de Heusch. Paris: Editions Gallimard, 1971. Pp. 331. (Paper.)

Rodney Needham

University of Oxford

Professor Luc de Heusch, the Belgian structuralist, has brought together a collection of 12 of his published and unpublished papers. They are here divided into three parts: "Structure and Praxis," "Religions," and "Politics." Those in the first part are conventional social anthropology and deal predominantly with kinship and marriage; those in the second are concerned mainly with myth, possession, and symbolic categories in Africa; and the third part consists of a eulogy to the late Patrice Lumumba. The volume is a most convenient enterprise, since it collects publications that were otherwise far dispersed and difficult to obtain, but the language in which they are published still leaves them fairly inaccessible to many students outside the French-speaking world. An English translation would be most desirable if attention is to be paid to an interesting and versatile anthropologist.

The essay which gives the volume its title—"Why marry this one rather than that?"—calls for a special scrutiny, not only because of its titular preeminence, but also because it gives a definitive impression of the author's style of argument. It is moreover a completely revised version of a paper originally printed in *Zaire* in 1955, and is thus especially telling as a deliberate example of method. It deals with Fr. Louis de Sousse's *Structures de parenté et d'alliance d'après les formules Pende* (1955) and with certain opinions on patrilineal cross-cousin marriage

that are ascribed to me. De Sousberghe argues that Pende marital institutions cannot be accounted for in Lévi-Straussian terms of exchange; I maintain that patrilateral prescriptive alliance cannot and does not exist. De Heusch wishes to show that we are both mistaken. He makes in fact a good case that the Pende ideology of marriage is open to a structural interpretation, but when he considers Pende institutions in connection with a patrilateral prescription he does not do so well. Citing only *Structure and Sentiment*, he ignores a whole series of relevant papers by Allan Coult, Robert Lane, E. A. Hammel, and others, dealing with this very issue, as well as a later report on the Pende by de Sousberghe himself; he ascribes to me the curious assertion that a patrilateral prescription is logically impossible, whereas my contention has consistently been that such a system, although logically possible, is nevertheless socially impossible. He credits Lévi-Strauss with the isolation of the patrilateral model, though Fortune and van Wouden did so long before; he crucially misconceives the implications of a genealogical specification of cross-cousins, despite the repeated attention that this matter has received in print; and he fails to realize that the Pende do not have a prescriptive terminology of any kind, let alone one that conforms to the hypothetical pattern of a patrilateral prescription. In a preliminary investigation, published at an earlier date, some of these defects would perhaps be understandable; but they strike one as retrograde and rather inadmissible in a paper rewritten and brought out following considerable public debate of the topic after a lapse of 16 years.

The long paper "Signes, réciprocité et marxisme," previously unpublished, is far more ambitious. It centers on a reexamination of Kachin society, and argues that the theoretical problem posed by the ethnography is nothing less than that of the emergence of social classes in an archaic economy. Here, too, there are unexpected failings when the author comments on the alliance system, but these points are subsidiary to a larger intention that finds expression in a peculiarly rhetorical vocabulary—for example, the incest prohibition is said to "constitute the fundamental level of real articulation between man and nature," by which is meant that "the rule of exogamy imposes socio-economic restraints on the biological law of reproduction." Again, the author seems not to understand his authorities correctly; when Edmund Leach writes that matrilateral cross-cousin marriage is "a correlate" of a system of patrilineages in a class hierarchy, de Heusch castigates him for holding a bizarre belief that the domination of superior lineages *explains* the type of marriage. Whether the argument as a whole really discloses the emergence of "a veritable consciousness of class" among the Kachin must await Leach's verdict, but the prosecution of the analysis of rank and affinal status does not instill an entire confidence in its conclusions.

It is hard treatment, perhaps, to settle on just two papers out of a dozen, and then to find so much fault in them, but they are both important and typical—and theoretical cogency depends on right particulars. The style and method of the collection, apart from the numerous enthusias-

tic references, show that de Heusch is still very much under the formidable spell of Lévi-Strauss; and his own work reproduces faithfully some of the invalidating deficiencies, as well as the grand cerebral visions, of the master. If this seems an ambiguous assessment, it is none the less a respectful recognition of the scope of de Heusch's interests, the fundamental intentions of his analyses, and the seriousness of his endeavors.

Kinship and Class: A Midwestern Study. By Bernard Farber. New York: Basic Books, 1971. Pp. xii+210. \$7.95.

Raymond T. Smith

University of Chicago

In 1968 Bernard Farber published a book entitled *Comparative Kinship Systems* (New York: Wiley), in which he presented the interesting discovery that there are marked differences in U.S. state laws regulating marriages between persons who are kin. Whereas in the majority of north-eastern and southern states the law permits marriage between first cousins and prohibits marriage to certain affines (such as the spouse's parent, sibling, grandchild, or parent's sibling), most western state laws prohibit the marriage of first cousins but place no restriction upon the marriage of affines. On the basis of this discovery of pattern in the legal codes he developed the hypothesis that there are two distinct kinship systems in the United States, which he termed the "Biblical System" and the "Western System." These were then analyzed in broad comparative perspective drawing heavily upon anthropological data and theories. Although that book seems to have received little attention from anthropologists, I do not intend to review it here. It is relevant only as the point of departure from which Farber set out to study a group of families in Champaign-Urbana, Illinois, with the intention of providing "knowledge about kinship among lower-class populations" (p. v). It is clear that the study was also designed to provide evidence for the existence of Biblical and Western systems of kinship, for by now Farber has developed the idea that the Biblical kinship system is found among higher status groups while low-status groups will tend toward the Western system. We have passed from the classification of legal codes to complex hypotheses about class differences in kinship behavior, norms, and values. The hypotheses are frequently interesting, but the evidence presented for their validity is seldom convincing.

It is argued that the Biblical System is conducive to the maintenance of a settled pattern of differentiated status groups, for it encourages the conservation of family estates (particularly, the symbolic representation of status) by incorporating spouses into each other's intimate kin group, thus stressing affinity and the marital linking of a small number of nuclear families which constitute a persisting kinship network. Parental interest in, or control of, mate choice is common; divorce is discouraged because the intimate kin group has a strong interest in the continuity and stability

of the union which has led to the full incorporation of the spouse. Cousin marriage is encouraged while marriage to affines would be redundant (since they belong to the same intimate kin group already), and may even be destructive of the solidarity of the group.

The Western System arose in an area of rapid change where the need was for the progressive integration of diverse populations. In this system, the individual does not modify his kinship identity upon marriage by joining his spouse's nuclear family; on the contrary, he remains tied to his paternal and maternal kinship networks and has optional and largely nominal relations with his affines. If a marriage is unstable, it does not matter too much since primary kinship affiliation is based on descent rather than affinity; cousin marriage is prohibited because first cousins belong to the same intimate kinship group and also because the marriage system encouraged widespread links based upon individual choice.

This is all very ingenious, but we are a long way from the geographical differences in marriage laws with which we started. Having developed these models of the different "systems," the author is now concerned to prove that they really exist. Space does not permit a full discussion of the manner in which he sets about this, but a few examples will suffice.

The first confirmation of the existence of two "systems" is found in a technical discussion of the utility of componential analysis by two anthropologists, Profs. W. Goodenough and D. M. Schneider (E. A. Hammel, ed., *Formal Semantic Analysis*, Special Publication of *American Anthropologist* [October 1965], pp. 259-308). Goodenough presented an analysis of "Yankee" kinship terminology using himself as informant, while Schneider criticized the soundness of the method of componential analysis based on his own knowledge of an extensive body of genealogical and cultural material collected from relatively high-status residents of Chicago. Farber is so anxious to find confirmation of the existence of his "systems" that he not only interprets Schneider's comments on Goodenough's article as a failure to recognize the difference between New England ("Biblical") and Illinois ("Western") kinship systems, but he accepts the word of an acquaintance that Goodenough is of higher socioeconomic birth status than Schneider, so that there is double confirmation of the real existence of the two systems! It is precisely this tendency to allow speculation (and idle speculation at that) to harden into conviction that mars what could have been a reasonable exercise in model building.

The book proceeds to further tests based on survey data pertaining to use of terms of address between relatives, measures of "feelings of closeness" and whether they persist after divorce, and measures of frequency of interaction between particular kin. Many of these data are interesting, but there are other ways of interpreting them than the ones presented here. For example, it is argued that if married persons address their parents-in-law as "mother" or "father" this is evidence for the existence of a "Biblical" kinship system because the individual is symbolically incorporated into the spouse's family of orientation. Some equivocal evidence is produced to show that such terminological usage is more frequent

among higher socioeconomic status groups. However, it could be that where there is greater joint activity on the part of married couples and less differentiation in their sex roles, then there is a more pronounced tendency for couples to treat each other's parents in the same way; that is, the relation to the spouse's parents could be a function of the relation between the spouses themselves and have nothing to do with the issue of "incorporation" into each other's families of orientation. I do not say that this is an explanation of Farber's findings—merely that it is as good an explanation as his in the absence of any extensive information on actual behavior.

While the earlier chapters are unfailingly interesting even when one finds complex anthropological issues oversimplified, the final chapters which seek to explain the relationship between kinship and poverty are distinctly irritating. We are given yet another version of the all-too-familiar theory of the "vicious circle" or "tangle of pathology" which argues that the lower class are the "rubbish" of society because they are incompetent. Since competence is *defined* as the ability to become middle class, it is clear that the argument has minimal explanatory power. It is true that Farber discounts differences between black and white members of the *lumpenproletariat*; that he enters the caveat that racial discrimination, stigmatization of the poor, economic exploitation, and other factors play a part in the persistence of poverty; and he even suggests that poverty is structurally necessary in the American social system; but we still end up with a picture of the lower class as "perennial adolescents," whose lack of "control" and general eroticism condemn them to hover forever on the fringe of the real (middle-class) American society. The contrasts he draws between middle- and lower-class kinship and general life styles are not inaccurate, and one can easily agree about the deleterious effects of poverty—bad housing, unemployment, poor schooling, and inadequate medical attention—but to suggest that the condition of the poor is inevitable because adherence to lower-class kinship forms automatically relegates them to superfluity in modern industrial society is, to say the least, unproven. Once again the author has fallen into the error of confusing his neat models with statements about reality; not only is the model inadequate but his account of the reality of lower-class life is without sympathy, without warmth, and ultimately without understanding.

Opportunity and the Family. By John H. Scanlon. New York: Free Press, 1970. Pp. vi+247. \$6.95.

Bert N. Adams

University of Wisconsin, Madison

Here is a study family sociologists should examine. John Scanlon's research in Indianapolis is the launch pad for an exchange-theory model of marital processes, and for certain functional assumptions regarding

articulation of the family and the opportunity structure of U.S. society. Ideas and hypotheses appear on virtually every page. Males may not mind their wives working, says Scanzoni, and may not mind if it reduces affiliation (shared activity), but *will* mind if it reduces affectional demonstration (p. 89). Affect is more important to marital cohesion than is affiliation, but empathy is more important than either affect or affiliation, especially in the later years of marriage (pp. 97, 134). An important reason for conflict in low SES homes is that these husbands are the most ideologically authoritarian, while their wives are the most equalitarian (p. 153).

Some of Scanzoni's ideas stimulate the reader to dialogue instead of agreement. The husband's, or else the family's, articulation with the opportunity structure is said by the author to be related to marital cohesion (pp. 13, 22, 33). Yet family articulation is not additive. The wife's working does not merely add to the husband's level of success. Furthermore, when the wife is more articulated (more successful) than the husband, difficulty instead of marital cohesion may result as long as a "male dominant" ideology remains; and Scanzoni hints this at one point (p. 22). Also, he states on page 83 that in popular lore women are the chief bearers of romanticism. Yet William Kephart had found women to be more realistic and men more romantic, indicating that Scanzoni's findings of greater male idealism should not have surprised him. Similarly, the author assumes that the husband exchanges high status for conjugal solidarity (p. 21), although it is equally plausible that he might exchange it for conjugal freedom or license. In short, whether the reader accepts, disagrees, or expands on themes introduced by the author, examination of this study is rewarding.

In any empirical study, problems arise, and the reviewer's task includes bringing these to the awareness of potential readers. One difficulty concerns Scanzoni's measures of several key variables. *Hostility* is measured by overt acts without evaluation. *Status estrangement* is based upon objective occupational position, although estrangement itself is an attitude (pp. 50-51). *Affiliation*, *affect*, and *empathy*—the expressive dimensions—are measured by evaluation only. When only evaluation is employed—for example, "Please tell me . . . how you feel about the love and affection you receive from your wife/husband," (p. 80)—two major problems are posed: First, much crucial information on the relation between behavior and attitude remains unknown. What of couples who do many things together, but are dissatisfied, or those who seldom engage in sex or confide, but are satisfied with this state of affairs? Varying relations between behavior and attitude are associated, as Mirra Komarovsky and others have noted, with such objective factors as SES. Second, there is a real danger that, after asking evaluations only, one will slip into a phraseology which sounds as though one has ascertained behavior. Scanzoni falls into this trap several times. In discussing empathy, for example, he says, "It seems that the presence of achievement and success generate a greater willingness to discuss the world of the job, their ab-

sence a corresponding reticence" (p. 121). Yet he does not know this, only about the respondents' satisfaction with such communication and understanding as they do have. The fact that in each chapter on a major expressive dimension of the family he slips into such behavior-oriented language (see pp. 78, 83, 89) means that he really wishes he knew about behavior, despite his lengthy justifications for evaluations only (pp. 27-28, 80-81). While this criticism borders on lamenting what the author might have studied or written about, in this instance it is both justified and inevitable—justified, because the author himself obviously is aware of and made uncomfortable by the problem, and inevitable because of the many unanswered behavioral questions which must arise in the reader's mind. The study is at its best when, as in the case of authority, the author has information on both decision making and evaluation of it—behavior and attitude.

Methodologically and stylistically, the study raises several additional questions. One damaging error is in Scanzoni's use of statistics to assert that empathy is more closely related to the opportunity structure, and distinguishes more between males and females, than either affiliation or affect. These conclusions are, however, a function of basing empathy on a two-variable index, while the other two are single-factor measures. The two measures which together comprise empathy, the author tells us, are highly correlated; yet they are simply added together, without weighting the two single-factor measures double to make them statistically comparable. In the following table, the means for husbands and wives on the three expressive dimensions as in the study and then with the first two doubled are presented, making them comparable with the two-factor empathy index:

| MEAN SCORE | IN THE STUDY | | AFFILIATION AND AFFECT WEIGHTED DOUBLE | |
|-------------------|--------------|-------|---|-------|
| | Husbands | Wives | Husbands | Wives |
| Affiliation | 2.70 | 2.51 | 5.40 | 5.02 |
| Affect | 2.77 | 2.70 | 5.54 | 5.40 |
| Empathy | 5.37 | 5.00 | 5.37 | 5.00 |

It can now be seen that differences between the sexes in evaluation of affiliation and empathy are quite similar. Thus, while Scanzoni's argument that empathy is the most important expressive dimension of modern marriage and distinguishes most between the sexes makes intuitive sense, it is not supported by his statistical evidence.

Again Scanzoni's methodology leads him to find high SES parents much less passive and a little less mastery-oriented than lower SES parents. The latter surprises him. However, certain items in the mastery index, especially items 1 and 3, are somewhat low-SES oriented. The clearest example is item 1: "Above all, parents should try to help their children get fur-

ther ahead than they were able to get." By stating it comparatively, the researcher biases the realistic high-SES parent toward disagreement, since he may perceive that his children can only equal, not surpass, his attainments. As a whole, this is more a striving scale than a mastery scale.

I was bothered somewhat by four additional aspects of Scanzoni's study: (1) broad labels such as "alienation" and "anomie" are used for specific measures of respondents' feelings about their current and future prospects within the occupational structure. (2) The lowest SES families are omitted from the study; they would have been difficult to include, but would have added much to our understanding. (3) The varying of independent variables from chapter to chapter is said to make for variety, and helps us "gain additional reliability" (p. 99). However, the reader is more likely to conclude that it makes for noncomparability, rather than reliability. (4) Realizing the exigencies of publishing, it is nevertheless much easier to follow an author's argument when tables are in the text, instead of clustered together at the back, as in this book.

It would be easy for such reactions to degenerate into quibbling. The author, on page 34, states that the major purpose of the book is to gain "theoretical leverage" on the family, and to propose exchange theory as useful for the sake of dialogue, not as a final answer. I would agree that Scanzoni has accomplished his aim, and would recommend that those interested in the modern family study this volume. It is a most stimulating and valuable contribution to the archives of family research.

Reciprocity Systems of the Rural Society of the Finnish-Carelian Culture Area with Special References to Social Intercourse of the Youth. By Matti Sarmela. Folklore Fellow Communications, no. 207. Helsinki: Academia Scientiarum Fennica, 1969. Pp. 347+15 maps. Fmk. 40 (paper).

Elina Haavio-Mannila

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The "getting together" institutions of rural Finland during the 17th through 19th centuries are examined in detail in this vast ethnosociological study. They include local festivals (parish and church holidays, berry fairs and harvest festivals, hiring fairs, *kihu* holidays, sheath proposals, and *prasnikas*); family festivals (proposal, betrothal, separation, wedding, and nameday festivals); work parties (bees and end-of-work events, handcraft parties, and watches); amusement events of the young (dances, play occasions, and play weddings); and night courting of the young. Each of these institutions could have been described in a special volume by itself, so rich is the material. The author's ambiguous frame of reference demands the inclusion of all these custom complexes in the same study, leaving the analysis partly fragmentary. Sarmela wants to present

a synthesis of reciprocal action—I would have preferred the term interaction instead of reciprocity—and to relate it to the ecological and economic system of the society. His basic approach is thus almost macro-sociological. He often makes use of sociological concepts and theories, even though he received his education in the fields of folklore and ethnology.

In the first part of the book, a general introduction to the reciprocity systems and individual institutions is presented: (1) institutions as ethno-sociological phenomena, (2) their spread, (3) the number of reciprocity situations, (4) behavior, (5) inner development and adaptation to value and attitude historical changes, and (6) age or period in which the diffusion of the institution may have begun. The second part deals with reciprocity as a social phenomenon, the formation and existence of youth groups, and the reflection of changes in the forms of reciprocity both in the reciprocity tradition and in Finnish society. The central concepts are rural-agrarian, dynamic-static and diffusion-acculturation-assimilation. "Rural culture" is used to designate a culture dependent on a natural economy in general. The term "agrarian" signifies a form of culture based purely on agriculture. In Finland, agrarian society was represented by a fixed peasant culture, characterized by village habitation based on a division of fields ("line village").

A central theme in the study is the examination of how the socializing youth became differentiated from the adult groups and how the periodicity of economic life ceased to be the basic determinant of the social interaction of youth. In this way, the study casts an interesting light on general social differentiation processes by showing how new roles and institutions develop.

Another basic theme is the comparison of the characteristics of different areas of Finland and their varying internal historical development. In studying the diffusion of culture traits, Sarmela discovers that customs have mainly spread from the south and west to the north and east, but that certain phenomena are of eastern origin. In the southern and western parts of the country, the central social unit was the village and the youth groups of this agrarian static society were independent and active. With the beginning of industrialization and urbanization in the late 19th century, the youth groups began to disintegrate. In the eastern part of the country, family and kin were more important social units than in the west. Socialization of the youth was limited to these groups. In eastern Finland with its scattered population, individualism and lack of integration prevailed, as has been shown in other studies, also.

The material consists of more than 10,000 separate items collected by ethnographers, historians, and laymen. It has been gathered (1) from several archives, mainly from the Folklore Archive of the Finnish Literature Society, (2) from literary sources such as local histories, and (3) by the author himself with the help of a newspaper inquiry. The data have only partly been statistically analyzed. The traditional note system gives the sources which support each statement in the text. The analysis

of this massive material mainly by hand and almost without any help was a tremendous job.

The courageous generalizations are made mostly on the macrolevel. The interacting groups—boys, girls, and adults—might also have been investigated as small social groups, and not only as carriers of tradition. Theories about youth culture and age classes, as well as some analysis of the functions of the institutions, might have been useful in the interpretation of the results. But as it is, the study provides a great deal of formerly unavailable information about the customs and rituals connected with special events in the religious, familial, and economic life in this remote European rural society, which has been influenced by diffusion from both east and west. Sarmela does not make many explicit cross-cultural comparisons with the same institutions in other countries, which would have been practically impossible with the resources available.

Social Networks in Urban Situations: Analyses of Personal Relationships in Central African Towns. Edited by J. Clyde Mitchell. Manchester: Manchester University Press, for the Institute for Social Research, University of Zambia, 1969. New York: Humanities Press, 1970. Pp. x+378. \$8.25.

Dan R. Aronson

McGill University

This collection is the first major work to explore systematically the utility of network analysis of sociological field materials. The editor, who is professor of urban studies at Manchester University, is clearly enthusiastic about the approach: he notes the "serious core" of a colleague's quip that "three things abideth—class, role and network—and the greatest of these is network" (p. 1). Readers may not be convinced that the concepts and methods of network analysis are as potent as predicted, but they will find a dramatic advance here over previous work in a field opened by John Barnes and Elizabeth Bott in the mid-1950s.

There are two roots of the current social anthropological interest in networks. First, as John Barnes notes, "The notion of network has been developed . . . to analyse and describe those social processes involving links across, rather than within, group and category limits" (p. 54). I have suggested elsewhere (*Canadian Review of Sociology and Anthropology* 7 [November 1970]: 222) that social anthropologists were constrained toward the elaboration of network analysis by several concrete but perplexing phenomena, among them the relatively atomistic behavior of urban migrants, and the interpenetration of personnel at rural, urban, and national levels of rapidly changing societies. In both these situations "groups" and "categories" were elusive at best. The second root of network analysis grows from a related but independent concern with choice making, the manipulation of norms and situations, and processual as well as structural abstractions from behavior.

After the editor's theoretical statement, to which I shall return, three short papers, two reprinted from other sources, set the context of the principal work here presented. Then, J. A. Barnes elaborates the thinking which informed his original paper on networks in 1954, with the aim of giving clarity to the terminology of network analysis. A. L. Epstein's 1961 paper on the diverse personal network of his research assistant is here too, together with a brief but fresh report on an incident of adultery which allowed the author to see how the communication of gossip helps to build and sanction the informal social order. Different sorts of networks are examined in Epstein's two pieces, but both provide insight into the structure of urban society which was not readily available through methods suited to rural communities.

It is, however, the new paper by B. Kapferer which is most lucid on network issues. In it he attempts to fully "explain" a brief dispute among electro-zinc plant workers in a mine in Broken Hill: an older worker accused a younger one of rate busting, the latter retorted that young men must beware of their elders' witchcraft, and as the day passes, other workers come to support the older one primarily on the *lesser* grounds that a junior should show greater respect than the younger man did. By careful stages, Kapferer sets out the structural framework which conditions relationships at work and the normative framework from which all parties to the dispute draw. But there are conflicting norms, and workers emphasize one issue or social relationship rather than another. So why does one elder rather than another accuse one junior rather than another over one sort of issue? Why does support mobilize in one of many possible ways for a lesser rather than the central issue? These intriguing questions of choice, strategy, and influence yield to Kapferer's scrutiny of the interactional and structural aspects of the complicated network of relationships among the workers involved. Kapferer does not substitute network (he prefers "reticulum") analysis for other modes; he demonstrates cogently its additive powers.

The three other long empirical papers by younger co-workers of the editor at what is now the Institute for Social Research of the University of Zambia are less broad than Kapferer's. P. D. Wheeldon analyzes the politics of a major voluntary association in the small colored community of a southern African town. By detailing the types and strengths of personal relationships and influence among the participants in a factional struggle, and by specifying the functions of certain kinds of information flow, she shows how the dispute is resolved enabling the community to endure cohesively. D. M. Boswell, using interesting material on the complex funeral arrangements for three people who die suddenly at a hospital, makes the point that recruitment to the effective portions of personal networks varies situationally, making the specification of situation analytically prior to relative closeness of ties. P. Harries-Jones casts fresh light on micropolitical processes (in Zambian political party formation) by using network and communication theory to explore linkages within and between groups of opponents in a small neighborhood and the recruitment

of support by higher-level political leaders. Harries-Jones finds that "home-boy" ties and critical points in communication flows are not as important to political mobilization as command of political skills and of actual resources.

Mitchell's introductory paper knits the book (how happy an event to have a collection of papers that is not a *non-book*!) together and explores the analytic potential of network methods thoroughly. After a review of the literature, he, like Kapferer, discusses the significance and measurability of various aspects of network morphology (anchorage, density, reachability, and range) and interaction (content, directedness, durability, intensity, and frequency). He urges the importance of direct observation of behavior and situation conceived in network terms, and suggests problem areas in current middle-range theory which network analysis may illuminate. Some will find his nonempirical treatise (along with Barnes's) flat; they might read Kapferer first. Others, who absorb all the distinctions and overlaps of terminological definition among these authors, will find yet further aspects of networks necessary for their own work.

Coming "out of Africa," this is not primarily an Africanist's book. There is no exoticism to excuse sociologists from attending to the analytical advance made here. But this strength could turn into weakness: class, role, and now network are neutral tools, and once forged, they are judged not by formal elegance but by function. If network analysis of field data is to abide and flourish, it must be turned to important tasks, imaginatively derived, both in Africa and elsewhere.

Organisation sociale des Peul: Etude d'ethnographie comparée. By Marguerite Dupire. Paris: Librairie Plon, 1970. Pp. 624. Fr. 47.40 (paper).

Nicholas S. Hopkins

New York University

The Fulani (*Peul* in French) are the most widespread of West African ethnic groups; numbering at least 6 million they are found in various ecological circumstances from Senegal in the west to Cameroon in the east. Some are nomadic cattle herders living in symbiosis with sedentary neighbors, as in Niger and Mali; some are seminomadic pastoralists, as in Senegal; others are sedentary agriculturalists for whom cattle retain largely symbolic values, as in the Fouta Djallon highlands of Guinea; still others form urban aristocracies in the theocratic states of Nigeria, Mali, Guinea, and Senegal. Such variation, combined with a distinctive physical type, has led to much unfortunate historical speculation.

Marguerite Dupire proposes a study of the Fulani which would be structuralist and synchronic rather than speculative and diachronic. This book is subtitled, "an essay in comparative ethnography," for it is Dupire's intent to arrive at some generalizations concerning all the Fulani through a systematic comparison of those she knows best. Her material is largely

drawn from her own research among nomadic Fulani in Niger, seminomadic Fulani in Senegal, and sedentary Fulani in Guinea. By "social organization," she means chiefly kinship and marriage patterns, and her ethnographic material on these topics is rich and ably presented.

The theme of Dupire's book is to seek the sociological origins and implications of the Fulani preference for endogamy. Drawing on alliance theory, she shows how marriage links are used to reinforce ties based on descent or common residence among all the Fulani groups she studies. Where descent tends toward the fission of groups, alliance can be used to reunite them. More importantly, alliance is used as a tactic by groups interested in gaining political power. In Dupire's analysis, all marriages have political meaning, although the nature of the politically advantageous marriage may vary from one situation to the next. Her most original point is to describe the cycle that ambitious descent groups go through: in the early stages they angle for support through the circulation of women, but once power has been gained there is a tendency to limit that circulation. When the group loses power and splits, the cycle begins again. Among the nomads, the ambitious group must give more women than it receives, for it is trading women for support; while among the sedentary Fulani those in power tend to accumulate women. In both cases, alliance is at least as important as descent for maintaining power. Dupire brings massive amounts of data to bear on this problem, but in the end the analysis is shallow and the generalizations inconsistent.

Partly this is so because she has not made up her mind why this comparison should be made: is it to show that groups in comparable ecological situations, such as the nomadic Fulani and the Bedouin of Cyrenaica, evolve similar institutions, or is it because she thinks that all Fulani groups are based on common principles that differentiate them from all other ethnic groups? Much of her argument seems directed toward the first question, yet her conclusions are framed in terms of the second. Consequently, she begs a number of historical questions, such as the relationship between the various Fulani groups and the relations that existed between the Fulani and their neighbors. She tends to find basic Fulani principles among the nomads, perhaps because they seem the simplest case; the other cases then show how the principles are transformed under different circumstances. Implicit here is the idea that all the Fulani were once nomads, yet nowhere is the evidence for this presented nor the implications for West African history explored. After a consideration of marriage patterns, she offers the idea that the Fulani have switched from a preference for mother-brother-daughter marriage to one for father-brother-daughter marriage without considering why the Fulani alone among West African groups, most of whom have at least a vague preference for mother-brother-daughter marriage, should have made the switch. Is it ecology, Islam, or what? She shows how the Fulani have been influenced by their neighbors, so that the Senegalese Fulani are a little like their matrilineal Serer neighbors, the Guinean Fulani have many Maninka traits, and the Nigerian ones have borrowed titles and

habits from Hausa and Kanuri; then she concludes the book by remarking polemically that the Fulani have never quite assimilated West African patterns, which hardly seems the point of the evidence presented in this book at all. This implicit suggestion that the Fulani have Middle Eastern origins shows that Dupire has not moved very far from speculations about Fulani origins, despite her brave talk about synchronic analysis.

Anthropological Perspectives on Education. Edited by Murray L. Wax, Stanley Diamond, and Fred O. Gearing. New York: Basic Books, 1971. Pp. xv+392. \$12.50.

Philip Foster

University of Chicago

How does a reviewer do adequate justice to published symposia arising from conferences? Having solicited manuscripts, editors are usually bound by the constraints of collegial loyalty to publish them and the result is usually predictable: along with pieces that would grace the pages of the most demanding of journals are to be found a larger number of essays whose contributions to scholarship is rather more obscure. The editors of this volume have done a valiant job in attempting to introduce some order and sequence into its very disparate contents but I emerged from reading it with very little idea of what contribution anthropology could make to the study of education.

Rather than undertake any detailed comment on the 20 or so articles contained in the book, let me join issue with what I conceive to be its main purpose—namely, to establish that there is a distinctive field that can be termed “anthropology of education” and that anthropologists by virtue of their special methodological training are in a position to make a unique type of contribution to the study of schooling. In particular, it is argued, they are singularly fitted to undertake microcosmic studies of schools as “social systems” or to analyze relations between the school and the local community. (I may add, in passing, that this does not prevent some of them from producing hortatory polemics on the macrocosmic problems of our time; I found pieces by Jules Henry, John Seeley, and Stanley Diamond, in particular, to be singularly out of place in a volume which is attempting to generate an atmosphere of academic credibility in what is a nascent field.)

The attempt to stake out this claim has, of course, been made several times before and I think that the earliest effort in this direction was *Education and Anthropology* (Stanford, Calif.: Stanford University Press, 1955), edited by George Spindler. On rereading that earlier volume and comparing it with the present one, I am struck by the relative absence of conceptual advance and significant fieldwork undertaken by anthropologists in the intervening 16 years. To be sure, I have profited from the work of Murray and Rosalie Wax and Manning Nash among others but

in general I am forced to the conclusion that most research undertaken by anthropologists in the field of education is rather thin stuff. I fear there is little in this volume to disabuse me of that view.

Indeed, judging from performance thus far, I am bound to reach the conclusion that anthropologists are singularly *unfitted* to conduct educational research. For all their disclaimers concerning objectivity and careful, controlled investigation, a large proportion of the authors are afflicted by a subliminal Rousseauianism which almost always enables them to see the school as an agency of repression governed by a conspiracy of middle-class power elites. (How weary I am of all this type of fashionable rhetoric!) I am always a little puzzled that anthropologists who can view the barbarities (I use the word advisedly) of Australian initiation ceremonies with sympathetic detachment and objectivity seem to become so outraged by some of the coercive aspects of western formal education. There is very little pretense at objectivity in much of this writing and some anthropologists are conscious of it. Thus, in one of the most perceptive essays in the volume, Harry Wolcott observes that as a result of his survey of the literature in the anthropology of education he "found few studies in which teachers or school administrators received either relatively compassionate or at least dispassionately analytic treatment, and I found fewer studies in which pupils and parents particularly ethnically different ones, did not receive special, almost "underdog treatment" (p. 101). So much for scholarship!

Moreover, I found some rather serious omissions in a volume that purports to give us the unique "lowdown" on what education is really all about. To be sure, Wilfred Wallers's seminal work receives two brief mentions and Durkheim all of one, but I find it hard to justify the absence of reference to Philip Jackson's more recent writings (surely an example of an essentially anthropological research in schools) and to Robert Dreeben's *On What is Learnt in School* (Reading, Mass.: Addison-Wesley, 1968). A perusal of the latter volume would, I am sure, be of immense profit to many of our authors.

I forbear to provide extended comment on some of the empirical research reported in the volume. It ranges from the respectable to the downright sloppy and makes me question the extent to which some authors are conscious of what is required in adequate research design. Some anthropologists never tire of telling us how superior their sensitive and qualitative techniques are to the crudities of quantitative research and general surveys. But they are not so conscious of the fact that many of the questions they themselves are asking can *only* be tackled by more rigorous adherence to a "tough-minded" methodology. I suspect that if they took the time off to read some writings produced by economists, political scientists, or sociologists in the field of education they could find observers perhaps no less sensitive than themselves but possessed of a much more formidable battery of research expertise.

Finally, what struck me about this book was its parochiality; nearly all the research reports are focused on the United States. Surely, the

editors could have rounded up a few anthropologists who might have written illuminatingly on research on education in the less-developed areas. Only two major items in the book really touch directly on this issue: Helen Icken Safa's useful piece on "Education, Modernization and the Process of National Integration" and Harry Lindquist's general bibliography at the end of the volume. The bibliography is especially illuminating; whether it is adequate or not (and I am not sure that it is), the perceptive reader will note that only a tiny proportion of the works cited are by authors who could even remotely claim to be anthropologists—a fair commentary, I think, on who is doing the substantive and significant work!

Learning to Be Rotuman: Enculturation in the South Pacific. By Alan Howard. New York: Teachers College Press, 1970. Pp. xii+184. \$7.25 (cloth); \$4.95 (paper).

Richard A. Shweder

Harvard University

Learning to Be Rotuman is one volume in the *Anthropology and Education* series edited by Solon T. Kimball, which has accepted the responsibility for presenting the anthropological point of view to educators and their students. An educator whose initial look at socialization and education from the anthropological perspective is this book will have reason to hesitate before coming back for a second look.

The book is a case study of socialization, formal education, and character formation on the Melanesian island of Rotuma. Based on Howard's fieldwork, it has three themes:

1. Rotuman "modal personality" is seen from the point of view of interpersonal relations, and explained in terms of values and socialization practices. The author argues that the Rotumans' dominant concern is with the maintenance of harmonious interpersonal relations, and that they eschew an interest in behavioral consistency, motives, or a stable self-image.

2. A contrast is made between the kinds of knowledge transmitted and the manner of transmission of knowledge in the formal school system of the island and the system of education in the traditional culture.

3. The author describes the ability of the Rotuman to become "bi-cultural," that is, capable of functioning in two cultures (the modern world of Fiji and his traditional home) by doing what is appropriate in each. Implications are drawn for subcultural groups in the American educational system.

The book has a number of weaknesses. First, the descriptions of Rotuman culture, personality, socialization, and formal education are based on aggregate impressions by the author. The data were not systematically collected (pp. x, 117). Second, there are major conceptual ambiguities

and confusions. Howard discusses the situational sensitivity which Rotumans exhibit in their social behavior. Yet he never distinguishes variations in culturally proscribed and prescribed behavior from variations of behavior in situations where there are no cultural maps for appropriate behavior. For example, differences in the degree of respect shown to certain categories of relatives is treated in the same framework as differences in childrens' reactions to frustrations when the frustrating agent is either an adult or a peer. Only the second type of variation is relevant to personality theory (pp. 37, 51, 52, 102).

The analysis of beliefs and values is confused with the analysis of actual behavior. For example, Howard is willing to make the highly debatable assumption that Americans are not as sensitive as Rotumans to their circumstances in the actual behavior they exhibit *because* Americans show a cognitive concern for consistency in their feelings, motives, and self-images, while Rotumans do not.

His theoretical view of "modal personality" is inadequate. He defines it as the similarities in the way individuals with similar learning histories respond to the same stimuli (p. 117). This is a necessary but insufficient definition of "modal personality." Howard must also show that there are differences in the way individuals with different types of social learning histories respond to the same stimuli. Thus, he does not explicitly distinguish differences in the properties of the social and physical environment of Rotuma and America from differences in the internalized response dispositions of members of these cultures as they result from child-training practices.

Third, there is a slightly disturbing flavor of "bongo-bongoism" and intellectual arrogance in the book. Howard tells us that he did not systematically collect data (p. x) and that his treatment of Rotuman "character" may reflect more his attachment to the culture than a commitment to scientific objectivity (p. 117). The bibliography of the book is brief and includes little of the theoretical and empirical literature on social behavior or on the education of "culturally disadvantaged" children.

Yet Howard is willing to use casual evidence from an exotic culture to challenge the notion that it is a universal psychological process for individuals to develop somewhat enduring and generalized self-images and identities. He may have a good point; but no one will be convinced with evidence of this sort. The same can be said for Howard's treatment of the concept "culturally disadvantaged," which he criticizes.

The book has some virtues. Howard is insightful in his discussion of difficulties faced by Rotuman children in the formal school system which can be directly related to a discrepancy between acceptable norms of conduct in the traditional culture and the school (pp. 56, 59, 134, 167). He dispels certain ethnocentric explanations of Rotuman interpersonal behavior and makes them understandable from the native point of view. Finally, he notes the extent to which reactions to frustration (e.g., withdrawal or aggression) vary in relation to the frustrating agent (e.g., adult or peer) (pp. 51, 52, 70).

The School Prayer Decisions: From Court Policy to Local Practice. By Kenneth M. Dolbeare and Phillip E. Hammond. Chicago: University of Chicago Press, 1971. Pp. xi+164. \$6.50.

Joseph H. Fichter

State University of New York at Albany

While I was reading this book, the Congress voted, 240 to 162, in favor of a constitutional amendment that would allow voluntary prayer in public schools. The majority was not large enough for passage—two-thirds were required—but the distribution of voting probably reflected the proportional preference for some minimum expression of religious devotion among public-school children.

Dolbeare and Hammond say as much in their first three chapters where they analyze studies previously made on this question. Before the Supreme Court prayer decisions of 1962 and 1963, 37 states either required or permitted Bible reading in the public schools. Regionally, the practice of some religious devotion was highest in the south and east, lower in the midwest, and lowest in the west. Negative reactions to the Supreme Court decisions came from many national leaders, churchmen, politicians, educators, and parents. There was widespread refusal to comply.

The research question proposed by the authors is How is national policy established by Supreme Court decisions transposed (or not transposed) into the lives of citizens? The general refusal of people to comply with the law was notorious in the case of Prohibition, for example. More recently, there were notorious instances of massive resistance to school desegregation by state officials in the southeast. In the matter of school prayer, say the authors, "strong public affirmation by state officials seems to promote compliance, and public negation seems to inhibit it" (p. 35).

It is an oversimplification to say—although it is often true—that people will obey the laws they like and disobey the laws they dislike. It is also an oversimplification to say—although it often seems to be the case—that public behavior (or morality) cannot be legislated. The fact that many parents and teachers want prayer and Bible reading in the schools is only one element in the situation. Neighbors are reluctant to bring court action. Local officials are politically unwilling to arouse conflict with constituents. State officials turn the other way when the ruling is being disobeyed, or argue that they do not have responsibility for enforcement.

The three chapters of part 2 analyze the content of the research study done by the authors in one midwestern state, here called "Midway." The research findings from five communities in Midway show that state officials and school superintendents in some instances opposed the Supreme Court ruling, in others ignored it, and in still others "did not know" that there was widespread noncompliance. Interviews in these five communities revealed that the issue of prayers and Bible reading was left pretty much to the discretion of teachers, but with the general stipulation that no child be "forced" to participate. At the level of the local citizenry, both

elite and nonelite, these communities "not only failed to comply with Supreme Court rulings; they failed even to respond" (p. 129).

The two basic principles involved in church-state relations sometimes become confused. People who agree that there should be no politically official establishment of religion in America also insist that there should be no official restriction of the free exercise of religion. The Supreme Court made it clear that the school prayer decision was based on the no-establishment principle. In their research, Dolbeare and Hammond found that many people interpreted the decision as an assault on the freedom principle. In other words, they support the separation of church and state, but they also support the right to the free exercise of religion—in this case, voluntary prayers by schoolchildren.

Does national policy result from the will of the people, whose demands percolate upward? One can argue that national leaders found it necessary to curtail the war in Vietnam when large numbers of people began to demand the restoration of peace. The movement for women's rights seems to promise success in proportion to the numbers who are supporting it. There are indeed instances when the courts and legislatures seem to delay social change until a groundswell of public opinion almost forces it upon them.

On the other hand, there are changes from the top down. The seventh and concluding chapter of this book generalizes the research findings and shows that this is a highly complex process. Social values and public attitudes centering around the school prayer decision had to be measured alongside the content of the policy decision, the nature of enforcement agencies, and the responsibilities of school and public officials, as well as the institutionalized patterns of people themselves.

This book is an excellent case study that may serve as a model of research procedure in the various areas where attempts are made to have national policy transformed to public behavior.

Crimes against Bureaucracy. Edited by Erwin O. Smigel and H. Laurence Ross. New York: Van Nostrand Reinhold Co., 1970. Pp. vii+142. \$2.45 (paper).

Ronald L. Akers

University of Washington

In this small (142 pages) collection of seven readings (all but one previously published), Smigel and Ross hope to engage our interest in what they believe is an unexplored type of crime—crimes against bureaucracy. The selections are well chosen, especially since there is not much literature to choose from beyond that on employee theft and shoplifting (indeed, four of the seven selections are on these topics). Three of the articles concern attitudes toward stealing from organizations: the first two by Smigel on public attitudes toward stealing from large or small organizations and

toward chiseling on unemployment compensation; and one by Donald Horning on blue-collar theft in an industrial plant. Two deal primarily with the kind of action taken against apprehended offenders: a reprint of part of Mary O. Cameron's well-known study of department store shoplifting and an article by Gerald Robin on the disposition of discovered department-store employee thieves. Donald Cressey's contribution is his often cited and reprinted analysis of the trust violator's vocabularies of adjustment. Only one deals primarily with actual violative behavior—Harold Grove's (an economist) report on income-tax evasion by landlords (of which there was very little in the city studied).

Space limitations preclude comment on each of the selections. Taken together, they provide some beginning documentation of what Smigel and Ross so clearly and succinctly point out in the introduction: The bureaucratic attributes of big business and government, their size, power, wealth, and impersonality, are often taken to justify or at least to diminish disapproval of their criminal victimization. For this and other reasons, people who commit crimes against large organizations are likely to define their actions as excusable, justifiable, or not really criminal. (One searches in vain, however, for an explicit recognition and analysis by Smigel and Ross of the rather obvious point that this constitutes a type of "definition favorable to law violation" in differential association theory.) Partly because of low visibility of offenses, not many offenders are detected; the ones who are apprehended are very likely to be handled informally by the private security personnel of the organization, rather than turned over for adjudication in the formal criminal justice system. Those who do become implicated in the formal system tend to be treated leniently.

In putting together these readings, Smigel and Ross have assembled a tidy package of interesting and stimulating literature under an engaging rubric. But they almost (although not quite) ruin it all both by claiming more for it than it is and by not doing enough with the concept. In the introduction, they present this editing job as following in the pioneering footsteps of Edwin H. Sutherland and Hans von Hentig in attacking oversimplified conceptions in contemporary criminology. But if the sociology of crime really is characterized by the stereotypes and assumptions which Smigel and Ross describe for it, then I have been reading the wrong books and journals. To say that contemporary sociology views the criminal as "proletarian, disreputable, and pathological" and that it continues to "search for pathological disturbances of the mind or of the social fabric that produce deviant personalities" (p. 1) is at best simply incorrect; at worst, it is patent nonsense. Also, the editors miss the opportunity afforded by the introduction to relate adequately concepts and research on crimes against bureaucracy to any of the three areas for which they claim the work is relevant—criminology, sociology of organization, and sociology of law. For instance, nowhere is the concept "crime against bureaucracy" defined or analyzed as an ideal type. "White-collar crime" is mentioned but there is no discussion of how the concept of "crime against bureaucracy" relates to it. There is no indication of the

distinctiveness, interrelatedness, or overlap between this and any other type of crime or crime typology (beyond simply listing some legal offense categories such as forgery, embezzlement, burglary, etc.). Neither the readings themselves nor simply bringing them together in one place will afford the challenge envisioned by the editors. They may stimulate some interest in doing more research along the lines of the two Smigel articles; but certainly, embezzlement, shoplifting, and employee theft are not radically new areas of research which would go unheeded without Smigel and Ross calling our attention to them. The challenge is afforded only by placing the readings in a significantly new perspective. All of this, of course, is too much to expect of a collection of readings which may have some chance of adoption for criminology (or perhaps complex organization) courses. It is not too much to expect of a work which claims to be a "treatise [which it is hoped] will stimulate further research which, in turn, will unearth new ways of looking at the criminal process" (p. 3).

Crime: An Analytical Appraisal. By Manuel López-Rey. New York: Praeger Publishers, 1970. Pp. xiv+277. \$9.00.

David R. Novack

New York University

This monograph stresses that the study of crime should be analytical and should emphasize the social and political aspects. López-Rey believes criminologists ought to stress the significance of crime and related statistics to assist political institutions to remedy social injustice and minimize criminal behavior. His sociopolitical concern emphasizes the way in which the social order both generates crime and reduces it. The sources include social disorganization and inequities; the resolution involves an application of knowledge gleaned from a somewhat unclear relationship between crime and the legal system. López-Rey states further that traditional explanations of criminal behavior (e.g., differential association theory) are not only inconclusive but demonstrate the futility of attempting a comprehensive causal approach. Instead, he believes that the legalistic perspective will be more fruitful by linking crime to the sociopolitical establishment, thereby creating new methods to develop a more just order.

Crucial topics discussed include statistical problems, juvenile delinquency, rural-urban distinctions, and socialist and capitalist approaches to crime. The overall perspective is a cross-cultural one, focusing on North and South America and Eastern and Western Europe. Such an approach is particularly informative, although most comparisons emphasize rate differentials with minimal focus on their substantive importance.

Excellent distinctions are made between known, detected, and officially reported crime. However, while the author notes epidemiological problems, he does not stress their social significance. In discussing juvenile delinquency, López-Rey asserts that the label of juvenile delinquent should

only be applied to youths who commit crimes, not to such individuals as truants and homeless court wards. Their inclusion, he correctly notes, leads to inflated delinquency rates. In this regard, he is especially critical of the English welfare approach. Further, he believes that youthful offenders should be treated as adult criminals and processed through the same system. Although this inclusion of youth with adult is defensible on civil liberties grounds, it ignores the social stigmatization that might prevent such individuals from subsequent involvement in legitimate social activity.

In examining the rural-urban dichotomy, López-Rey indicates the need for making finer distinctions in criminological analysis. He distinguishes between partly rural and partly urban areas and various-sized urban units. In addition, he notes the difference between urban areas and the potentially disorganizing process of urbanization.

One of the best points is López-Rey's contrast between capitalist and socialist criminological approaches. The former appear to stress behavioral causation whereas the latter emphasize a legalistic viewpoint and attribute crime primarily to remnants of capitalist systems.

The major drawbacks of this work are threefold: (1) insufficient documentation, (2) a number of contradictions, and (3) a failure to make clear exactly what is meant by a sociopolitical approach to criminology. The author asserts that an understanding of the association between crime and the political structure will provide a means of removing the conditions that lead to criminal behavior. In this light, he stresses the role of population increase. However, he fails to show more than a correlation between this variable and crime rates. To show an association is not to demonstrate a causal relation. Moreover, his treatment of the intervening factors of official corruption, leisure time, and societal insecurity does not involve sufficient documentation or an integration with the main variables. For example, he speaks of leisure and youthful offenses without supporting data and discusses insecurity without relating it to crime.

An important contradiction exists because, although he deals with the causal role of population increase in criminal behavior, he indicates elsewhere that criminology should not deal with behavioral causation. In this regard, he dismisses numerous traditional approaches with a cursory examination and without any attempt at integration. Hence, in his general approach to crime, López-Rey tends to minimize causal factors. Regarding actual treatment, however, he deals with these elements. Although he does identify sources, which are primarily structural, he fails to formulate an explicit criminological program.

Another difficulty is the author's ambiguous approach to the sociopolitical aspects of crime. While stressing the political significance of criminal behavior, he states that the social evaluation of such acts is not a concern for criminologists: "With respect to criminal behavior two explanations are possible: one [is] why a particular form of behavior has been 'promoted' to or sometimes 'demoted' from the condition of criminal, and the other [is] why the behavior performing a criminal offense has taken place. The

first explanation is outside criminal law and criminology. The second refers to something that by itself is not intrinsically criminal" (p. 136). However, elsewhere he appears more aware of the social significance of this label in noting that "the ultimate decision when dealing with the crime problem . . . belongs not to a causation process but to an evaluation process in accordance with the fundamental values incorporated in the legitimate order" (p. 101). Apart from this major inconsistency, an examination of the social evaluation of various acts would seem crucial given the author's political emphasis. To understand crime as sociopolitical, it is essential that the criminal label be politicized, thereby enabling one to see more clearly the linkage between criminals, crime, and the powerful groups whose values are imposed on others through legitimate legal channels.

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Toward the Sociology of Esoteric Culture¹

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Duke University

The point of departure of this paper is an examination of the sociological literature on the recent occult revival in modern societies. This phenomenon, which clashes with the image of secularization, is particularly notable among the youth of the counterculture. To further sociological analysis, a conceptualization of esoteric culture is proposed. It is further argued that esoteric culture has played a significant role in Western cultural change, in such areas as artistic expressions of reality, political ideas, and even scientific thought. Esoteric culture is thus treated as a source of ideational innovations in Western modernization.

Among other bewildering aspects of the kaleidoscopic cultural scene of Western societies in recent years is a complex of phenomena which, for lack of a more precise label, has generally come to be designated as the "occult revival." Receiving public exposure in this context have been a variety of forms of popular entertainment dealing with occult themes, for example, the musical *Hair* with its "Age of Aquarius" hit song, movies such as *Rosemary's Baby* and *The Mephisto Waltz*, and the television series "Bewitched." Clairvoyants such as Jeanne Dixon in the United States and Madame Soleil in France have become public figures and authors of best sellers, while self-designated practitioners of witchcraft or even devil worship (as in the respective cases of Sybil Leek and Anton LaVey) have gotten public exposure in the mass media, along with attention to the macabre Tate-Manson affair and other cases of purported ritual homicides and suicides in which figured weird occult themes. Bookstores specializing in occult works of various kinds, frequently located in proximity to college campuses, are flourishing; sales of items bearing a Zodiac sign are booming and have even received in France, since 1969, a quasi-official blessing in the form of a special monthly drawing of the National Lottery. Tottering ivory towers have been no refuge from the occult revival: seminars and courses, with or without credit, have taken up the new "in" study, with the logical extreme being the recent formation of an "Aquarian University" in Maryland, offering a full range of esoteric studies from alchemy to Zen Buddhism.

¹ Revision of a paper presented at the 1971 meetings of the American Sociological Association. I wish to thank Josefina Tiryakian for assistance in the preparation of the manuscript.

The sociological response to this new cultural development is just beginning to emerge. One of the aims of this paper is to see what sociological interpretations of this phenomenon have been recently advanced. At the same time, we will seek to carry the discussion further by outlining how the present occult revival poses broader considerations for sociological inquiry, regardless of whether the revival is destined to be a short-term affair of no significance or one with more durability. What we shall seek to develop in these pages, then, is an initial formulation of the sociology of esoteric culture and its relation to the larger social context.

THE SOCIOLOGY OF THE OCCULT REVIVAL

One of the earliest and most extensive sociological treatments of the occult revival is that of Marcello Truzzi (1970), providing, in particular, much information on the spread and organization of witchcraft in contemporary urban American society. The perspective within which the materials are presented is that of the sociology of popular culture, with modern occultism taking on the significance of a "pop religion" (we might even say of coven adepts, "Have broomstick, will travel!"), which Truzzi regards as a "demystification process of what were once fearful and threatening cultural elements." Persons playing the role of witches, for example, are attacking some of the last cultural frontiers of Western psychic inhibitions; to engage in role taking of parts formerly publicly branded as odious and the object of extreme social repression is, in a sense, to demonstrate the final liberation of Western man (and woman) from traditional cultural prohibitions dealing with the supernatural. The occult revival, at least in terms of the receptivity of witchcraft among segments of the middle class, could thus be seen as another step in the modernization of Western society, in this context as a secularization of the demonic. Such a perspective would be consonant with the secularization hypothesis concerning the relation of religion to modern society (Wilson 1966).²

Supplementing Truzzi's linking of witchcraft to popular and mass culture, Marty (1970) has examined a variety of publications dealing with astrology and psychic phenomena, on the basis of which he differentiates an occult establishment, responsible for most of the widely circulated publications in this area, from an occult underground press. Marty gives most of his attention to the former and notes that it is predominantly intended for an audience of middle America. In this literature there is an absence of a social message, so that "like some forms of conservative orthodoxy, the occult establishment concentrates almost entirely on individual life and often on 'other-worldly concerns'" (Marty 1970, p. 228).

² For a recent treatment of the secularization thesis, see Robertson (1971).

Evaluations contrasting with the above have been entertained by Staude (1970) and Greeley (1970*a*, 1970*b*). Staude views the current interest of youth in occult practices and mysticism as essentially a search for meaning and identity because "they feel alienated and disillusioned with the liberal progressivist ideology of their parents and with totalistic ideologies" (1970, p. 13); in this respect, he suggests, today's cultural setting is similar to the religious and cultural renewal of the Renaissance. More directly than Staude, Greeley has seen the import of current youth interest in occult behavior in its implications for the sociology of religion. Whereas Marty (1970, p. 228) had pointed to an absence of a communal impulse in the literature of the occult establishment, Greeley stresses such an impulse in the broad neosacral movement of today: "The young people who are involved do . . . assert that their sacred, or mystical, or occult interests do indeed provide them with meaning, with community, with a contact with the transcendent, and with norms by which to live" (1970*a*, p. 6).

Greeley, like Staude, sees the appeal of occult behavior on the college campus as symptomatic of the alienation of youth from the scientific-rationalist ethos of modern society, with the invocation of new gods: superstition, ecstasy, and "groupism" (Greeley 1970*b*).³ He notes an affinity between those engaged in occult practices on the campus and the new left, not only in terms of the rejection of the dominant institutional ethos but also in terms of certain value orientations: the affirmation of the inherent goodness of human nature (an echo of both Rousseau and the much older Pelagian heresy) and the concern for transcendent power in human interaction (1970*b*, p. 208). Further characteristics of the new faiths, he suggests, are that they are millennialistic, charismatic, and liturgical. The thrust of Greeley's argument is that the occult revival is to be seen as a neosacral movement in contemporary culture, which by the significance of its presence is further evidence against the proposition that increasing secularization accompanies or is an integral aspect of modernization.⁴

Greeley's writings on the significance of occult behavior among youth, as well as his earlier empirical research (1968), constitute a major critique of the secularization hypothesis.⁵ Additional testimony in the occult

³ A justification for Greeley's terms "tribal Gods" and "tribal consciousness" is the self-designation of some communes as "tribes."

⁴ The late Howard Becker had developed, in an unfortunately neglected communication to the American Sociological Association (1960), a theoretical critique of the secularization model. Becker would have no difficulty in perceiving today's occult revival and the neoevangelical movement as instances of "normative reactions to normlessness."

⁵ Like Greeley, I see this hypothesis as having been derived from a short-sighted reading of Durkheim and Weber on the modernization process—or else as a correct reading of the mistaken notions of Spencer.

revival literature comes from a recent article of Shepherd (1972), who observes "a new mysticism emerging among the young in the developed countries, one not constrained by an already well-defined religious context" (p. 8). Arguing that the new religious life-style is analogous to the aesthetic experience of music—a theme which echoes the contention of Roszak (1969) that today's dissenting youth have returned to the archaic aesthetic vision of beauty of the shaman, a vision communally shared—Shepherd (1972) proposes that "the counter cultural young among Westerners . . . may be the agents in a process of 'reorientalization' occurring within our own culture, and that they may be the harbinger of spring, of a new value consensus" (p. 8).

In this vein, one might argue that a function of occult practices is to provide a position against what is perceived as "Establishment" mentality with its structural apparatus of modern societies: The oppressive "technocracy," "reductive rationality," and "objective consciousness," to borrow terms from Roszak. Occult practices are appealing, among other reasons, because they are seemingly dramatic opposites of empirical practices of science and of the depersonalization of the industrial order. The appeal of the occult may thus be related to the new appeal of artisan work to many college youth, since it is nonindustrial work, affording a reintegration of personality with the product of one's labor.⁶

One last sociological study of the occult in modern society to be noted here is a recent French collaborative work on contemporary astrology (Defrance et al. 1971). It is not easy to present in a few words the contents of this work which, taken as a whole, is a sociology of astrology, but several of the themes discussed earlier are elaborated here. Edgar Morin (Defrance et al. 1971, pp. 110–25) sees the appeal of astrology among youth today as stemming from the cultural crisis of bourgeois society, with astrology offering symbols of identity as a science of subjectivity. Paradoxically, modern astrology has an antithetical function in modern society: in mass culture, the popularization of astrology in the mass media plays an integrative role in bourgeois civilization by reconciling individuals with their situation. Morin suggests that among counterculture youth astrology is also part of a new gnosis which has a revolutionary conception of a new age, the Age of Aquarius.

In his chapter "Astrology and Society," Fischler (Defrance et al. 1971, pp. 69–81) considers other latent functions of astrology. Modern society multiplies fragmentary contacts between strangers, without traditional norms to guide comportment; in the face of making an increased number of decisions, especially of an interpersonal kind, resort to astrology and

⁶ It is tempting to think that both artisan labor and occult work among today's college-trained youth may indicate a new nonindustrial form of inner-directed achievement orientation.

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other means of divination reduces the ambiguity of intersubjective conduct. Since astrology is subject oriented, it is a means for self-apprehension and self-grasping. Fischler also presents data initially gathered in 1963 by the French Institute of Public Opinion, which examined in a cross section of the French public just how widely diffused is adherence to astrology. The survey sought the social distribution of those respondents possessing three attributes, namely, they (a) knew their own Zodiac sign, (b) read their horoscopes fairly often, and (c) thought there was some truth to astrological character traits. The results are shown in table 1.

TABLE 1
SOCIAL CHARACTERISTICS OF BELIEVERS IN ASTROLOGY (FRANCE)

| Category | Percentage in Total Sample |
|---|----------------------------|
| Total believing in astrology | 30 |
| By sex: | |
| Men | 21 |
| Women | 39 |
| By occupation:* | |
| Professional, managers, and executives | 34 |
| Small business owners and artisans ("commerçants") | 36 |
| Clerical and sales ("employés") | 46 |
| Manual workers ("ouvriers") | 29 |
| Farmers and farm workers | 15 |
| Not in the labor force or retired | 30 |
| By locality: | |
| Under 2,000 inhabitants | 21 |
| 2,000-5,000 | 27 |
| 5,000-20,000 | 37 |
| 20,000-100,000 | 34 |
| Over 100,000 | 40 |
| By age (years): | |
| 20-34 | 38 |
| 35-49 | 33 |
| 50-64 | 24 |
| 65 and over | 20 |

SOURCE.—DeFrance et al., *Le retour des astrologues*, p. 74, from survey data gathered in 1963 by the Institut Français d'Opinion Publique, at the request of *France-Soir*.

* Since occupational classification in France differs in some cases from that commonly used in the United States, we have given in parentheses the French category in those instances where the translation is approximate.

Noting the high incidence of astrological adherence among women and those under 35, Fischler suggests (DeFrance et al. 1971, pp. 80 f.) that as these formerly segregated strata enter more and more in the modern *polis*, having been less stamped by the belief systems of the dominant culture, they are more prone to recourse to astrology in relating to the larger society.

What is of further interest are additional data presented by Fischler (Defrance et al. 1971, p. 75) indicating the social distribution of two groups: (a) those having consulted at least once a card reader, a clairvoyant, or a person predicting the future; and (b) those having consulted at least once an astrologer. On the whole ($N = 6,000$ in the national sample), the results were consistent with the first survey (the more populated the community, the higher incidence in each group, etc.), save that the category of those 18–25 years of age had a lower incidence (9% and 1.5%, respectively, in the two groups) than the other age categories (which all had practically the same incidence, about 13% and 3%). Although Fischler sees in this that older persons go in for more applied astrology and the young for a more speculative curiosity, it may also be the case that the young are their own practitioners of astrology (and other divinatory practices), for whom going to professional practitioners (part of the occult establishment) would defeat the purpose of a personal quest for meaning and certainty in an ambiguous world.

Although more suggestive than conclusive, the French data point to a redrawing of a certain sociological image of superstition/rationality as distributed in society. For the greatest spread of adherence to astrology is not to be found in the countryside among farmers or among the lower rungs of the occupational structure, but rather in the most densely populated urban centers and among white-collar workers. Moreover, although the French materials are silent on this, my own observations suggest that there is a higher incidence of belief and interest not only in astrology but also in other occult sciences among those of high educational achievement—college and university level—than among those who have not gone beyond high school or those who are still in high school. Obviously, this is a terrain for further empirical studies.

In examining various sociological writings dealing with the occult revival, a number of themes have been touched upon. Most significant, perhaps, is that this phenomenon has to be seen in terms of a broader social context of cultural change. As a spiritual reaction against the rationalistic-industrial-bureaucratic ethos of modern society, it is part of the counterculture. Similarly, it may also be seen as part of a new religious cultural revitalization, having an affinity with both the exuberant neoevangelical movement crossing denominational lines and the equally vigorous political movement of the new left in developed societies.

To take the sociological interpretation of the occult in modern society further along, there is need to develop a more articulated conceptualization. For one thing, we need to differentiate basic elements of the occult, and for another, to reexamine how the occult relates to modernization in its cultural aspect. This will be undertaken in the following section.

COMPONENTS OF ESOTERIC CULTURE

The previously discussed sociological literature on the occult revival has related it, on the one hand, to the sociology of mass society, and on the other, to the sociology of religion. Since we posit that the sociology of the occult is also germane to the study of cultural change and the dynamics of modernization, it may be located more broadly as part of the sociology of culture. It is at this point that I wish to explicate some basic terms of central importance to this essay: "occult," "esoteric," and "secret society." These are familiar terms, yet ones which have had little currency in standard sociological reference works.⁷ It is necessary, before conceptualizing these terms, to have a preliminary understanding of "culture." The sense of culture which is followed here is, so to speak, that of a collective paradigm which provides the basic interpretations and justification of ongoing social existence. As the anthropologist Ward Goodenough has expressed it: "Culture then consists of the 'concepts' and 'models' which people have in their minds for organizing and interpreting their experiences." It should be noted that Goodenough takes into account non-linguistic aspects of culture (which have a bearing on the significance of symbols in esoteric culture, since symbols and imagery are primary modes of relating to reality for the latter) when he adds: "nonlinguistic forms have systematic relationships to each other in paradigms" (quoted in Singer 1968, p. 538).

Equally pertinent here is the conceptualization of Parsons viewing culture as that integral component of systems of social action which provides the fundamental symbolic grounds of expression to the existential problem of meaning inherent in social existence. Although "meaning" has a cognitive and rational orientation, it also has a complex moral one as well, one involved in the evaluation of social action. As Parsons states: "The highest level of the problem of meaning is that of the conceptions of ultimate reality, in the religio-philosophical sense. This concerns the major premises in which the nonempirical components of a culture's total belief system are rooted" (1961, p. 970).

⁷ In the sociological literature on the occult revival, I have found commonly known instances of the occult (astrology, witchcraft, etc.) but no analytical definitions or conceptual classificatory scheme subsumed under this rubric. The *International Encyclopedia of the Social Sciences* has no entry for "esoteric" or "occult" and only passing mentions of "secret societies" in a few substantive articles. The UNESCO-sponsored *A Dictionary of the Social Sciences* does have the following comment (by Kenneth Little, an anthropologist) under "Secret Society": "It is difficult [in this context] to discuss the nature of modern institutions, such as Freemasonry, but in primitive cultures secret societies generally constitute an integral part of the social system concerned" (in Gould and Kolb 1964, p. 624). Why the difficulty is not made clear.

There is one qualification that needs to be introduced to the above conceptualization of culture, since it has a crucial bearing on the central thesis of this paper. It is that a given social complex, as in the case of modern Western society, may have more than one set of major premises present in its cultural matrix; that is, that there are several cultural paradigms offering the ground of meaning of social action, albeit one set may have dominance in the institutional fabric of society while another set remains covert or latent.⁸ The cultural paradigm which is manifest in public institutions, a set of cognitive and evaluative orientations publicly recognized and legitimated in the network of social institutions, is what I propose to call "exoteric culture." Exoteric culture provides the ground of meaning and orientation for the everyday social world. It is the social basis of what the phenomenologist Husserl designated as "the natural attitude," in terms of which actors take the existence of the world for granted, that is, in a sociological sense they take as nonproblematic the institutionalized structures of the social world. It is implicitly in reference to exoteric culture that sociologists and anthropologists, for the most part, have formulated their conceptualizations regarding culture and society.

I wish to propose that a unitary conceptualization of the cultural system of Western civilization in its historical development via modernization has to be modified, and that to arrive at a more sophisticated understanding of cultural systems and societal change there is need to consider what, for heuristic purpose if nothing else, I will call "esoteric culture." It is at this juncture that we need to specify three major components of this esoteric culture, elements common to any cultural whole but taking specific forms in esoteric culture: a set of beliefs and doctrines (cognitive and moral orientations), a set of practices oriented to empirical action, and a social organization within which action is patterned or structured.

Although "esoterism" and "occultism" are often used interchangeably, and although there is no standard agreement as to their referents, it may still be fruitful to venture an analytical differentiation. Both, of course, refer to something which is not immediately given to the senses or to perception, to the nonempirical in this sense.

By "occult" I understand intentional practices, techniques, or procedures which (a) draw upon hidden or concealed forces in nature or the cosmos that cannot be measured or recognized by the instruments of modern science, and (b) which have as their desired or intended consequences empirical results, such as either obtaining knowledge of the empirical course of events or altering them from what they would have been without this intervention. Obviously, I exclude for the purpose of my analysis some

⁸ The conceptualization and research of Florence Kluckhohn (1950, 1961) on dominant and substitute value orientations is highly relevant here.

broadier considerations of occult phenomena, such as extrasensory perception and *déjà vu* experiences, because these are harder to integrate within a sociological scheme. To go on further, insofar as the subject of occult activity is not just any actor, but one who has acquired specialized knowledge and skills necessary for the practices in question, and insofar as these skills are learned and transmitted in socially (but not publicly available) organized, routinized, and ritualized fashion, we can speak of these practices as occult sciences or occult arts.⁹

Commonly recognized occult practices include a variety of phenomena, such as those designated as "magic"¹⁰ and divinatory practices,¹¹ which are very numerous cross-culturally (astrology, the Tarot, and the I Ching having been particularly noted in the contemporary occult revival); they also include practices which are oriented to changing the physical nature of nonhuman objects by the active participation or ego involvement of the subject, as in the case of alchemy.

By "esoteric" I refer to those religiophilosophic belief systems which underlie occult techniques and practices; that is, it refers to the more comprehensive cognitive mappings of nature and the cosmos, the epistemological and ontological reflections of ultimate reality, which mappings constitute a stock of knowledge that provides the ground for occult procedures. By way of analogy, esoteric knowledge is to occult practices as the corpus of theoretical physics is to engineering applications. But a crucial aspect of esoteric knowledge is that it is a secret knowledge of the reality of things, of hidden truths, handed down, frequently orally and not all at once, to a relatively small number of persons who are typically ritually initiated by those already holding this knowledge. Moreover, it should be added, this knowledge is not of a detached or objective sort about an outer reality which stands against the observer as this page stands against the reader; esoteric knowledge is of a participatory sort, namely a knowledge (or gnosis) of the meaning of the world to human existence, in the progressive realization of which the subject develops internally and liberates himself from the strictures of everyday life.

Since esoteric knowledge is taken as knowledge of the real but concealed nature of things, of ultimate reality (a knowledge which therefore is divine, theosophic in the generic sense), it is important that its recipient

⁹ It is worth noting that occultism has both a scientific and an aesthetic aspect, which relates back to our earlier remarks on the meaning of the occult revival for youth. We need not dwell on how much of scientific creativity has a pronounced aesthetic dimension, and conversely.

¹⁰ Parlor tricks or stage magic is not meant here, but rather such practices as sorcery, witchcraft, ritual or ceremonial magic, and the like.

¹¹ For a succinct anthropological statement on divination, see Victor Turner (Sills 1968, 13: 440).

be demonstrated worthy of receiving it; that is he must meet criteria of acceptance into the inner circle of true knowers. Hence the need of the candidate to submit at various stages to a series of trials and ordeals, in the course of which the adept becomes increasingly socialized into the esoteric culture and increasingly desocialized from the natural attitude of the exoteric culture.¹²

At the heart of esoteric knowledge is its concealment from public dissemination, from the gaze of the profane or uninitiated. To shield it from vulgarization, it is presented to the adept not directly but, typically, symbolically or metaphorically, so that it has to be deciphered progressively by the neophyte who uncovers layers of meaning in stages of initiation. As a correlate of this, the social organization and handling of esoteric culture tends to take the form of secret societies, societies whose modes and codes of organization and membership lists are not publicly disclosed and which may even be incompletely known to members who have not attained the highest stages of initiation and spiritual perfection.

The sociology of secrecy and secret societies, which is essential for any consideration of esoteric culture, has largely lain fallow since the seminal essay by Georg Simmel (1905). Yet secrecy is of general sociological interest since, as Simmel was quick to note, it is constitutive of social structure and social interaction. Even the most democratic countries, de jure or de facto, organize much of their affairs secretly and have agencies which specialize in concealed activities; all information about the doings of formal organization is not for public consumption but only to qualified insiders, some more than others. This leads to other outsiders seeking to obtain this information in specialized ways (private and governmental espionage activity seeking industrial, military, and state secrets), with such information-gathering activities being themselves secret.

To be sure, not all secret societies pertain to esoteric culture, nor are all social organizations that have secrets, secret societies. But those secret societies which are social forms of esoteric culture have common features. They typically have rituals of initiation and are hierarchically structured in terms of strata which correspond to different degrees of initiation. Leadership and authority are functions of stages of acquisition of esoteric knowledge, at least in theory. At the top echelon is a very small elite designated variously as "Magi," "Grand Masters," or other appropriate terms, and a council responsible for ultimate internal and external policies of the

¹² The academic world may be seen, through one perspective, as routinizing and secularizing the esoteric acquisition of gnosis. At lower levels, students progress through neophyte levels known as grades. At higher levels we have *rites de passage* (qualifiers, Ph.D. orals, and the like) as we initiate students into higher stages of the academic fraternity. The Phi Beta Kappa ceremony is a symbolic accolade of those who have shown particular aptitude for academic mysteries, albeit few of its recipients may be aware of its esoteric background.

organization. The council is somewhat along the lines of an executive committee of formal organizations; an enlarged board of directors may include, as notably in the case of Freemasonry, persons holding high ranks in exoteric society, even including heads of state.¹³ Although the hierarchic principle of organization is basic to secret societies, the higher rungs are accessible to all. Moreover, fraternal solidarity is greatly emphasized: irrespective of social standing in the larger society, all members are brothers (or sisters), and when in a situation of distress, a member is to be given every assistance possible by any other member at hand, even on the battlefield, if they belong to opposite forces.

These, then, are major components of esoteric culture. It would go beyond this essay to articulate specific esoteric belief systems or to discuss the details of organization of specific secret societies. More relevant is to indicate that esoteric culture is not concretely disjoint from exoteric culture, that it coexists, albeit unobtrusively with the latter,¹⁴ or stated differently, that there are many interchanges between them.

The esoteric culture often uses as referents publicly known cultural materials such as religious texts and figures (e.g., the Torah, the Book of Revelations, Adam, Christ, etc.), but it considers that the meaning of these is not exhausted by their public definition and recognition; rather, the esoteric group sees itself as the true repository of the ultimate reality manifest in the materials in question. Only a select few, initiated into the mysteries can decipher and pass on in an unbroken chain the real meaning of what underlies the figurative. Only a few, thus, can be permitted to attain knowledge of the secret name of God which Moses learned on Mount Sinai and which has been transmitted orally to those who can correctly understand the Kabbalah; only a few can learn the secret teachings of Christ to His chosen disciples, etc.

Since these secrets are those that reveal the ultimate nature of reality, the concealed forces of the cosmic order, it means that esoteric knowledge is an ultimate source of power, which must be shared and utilized by a relatively small group of initiates. Such power is never justifiable in terms of the enhancement of the material conditions of the esoteric knowers but rather in terms of broad impersonal ends, humanitarian ideals, etc. Consequently, much of the parlance of esoteric culture is necessarily

¹³ This is institutionalized in Sweden and Great Britain where the monarch or a member of the ruling family, respectively, is the titular head of Masonry in his country; though not institutionalized in the United States, most presidents have been 33° Masons or of high rank.

¹⁴ The cultural traditions of all the major complexes of civilization and high religions have an esoteric side. Thus, Islam, Christianity, Hinduism, and Judaism have their esoteric components in Sulfism, theosophy (in various forms: Illuminism, Rose Croix, and more modern versions such as those of Blavatsky, Gurdjieff, R. Steiner), Tantric Yoga, the Kabbalah, etc.

obscure, that is, designed to put off members of the larger society; this is in some ways similar to the argot of the underworld or even to the language of some psychotics used as a shield from public deciphering. Exoteric language glosses over the esoteric source of such expressions as "third degree" (from Masonic initiation), "magnum opus" (from alchemy), or "sub rosa" (from Rose Croix), just as other items originating in the esoteric culture have been absorbed, for example, card games (the major suits trace back to the Tarot, the esoteric depiction of human existence seen as a dialectical process).

ESOTERIC CULTURE AND SOCIOCULTURAL CHANGE

It is the relation of esoteric culture to various facets of Western modernization that is of particular interest to this article. We wish to argue that much of what is modern, even the ideology of modernization at its source, has originated in esoteric culture; paradoxically, the value orientation of Western exoteric society, embodied in rationalism, the scientific ethos, and industrialism, has forced esoteric culture into the role of a marginal or underground movement. That is, modern Western civilization (dating it back to the Renaissance and Reformation) has increasingly given to esoteric culture the mantle of a counterculture, while at the same time co-opting many of its values and products.

I would like to suggest that the conceptions of ultimate reality in the esoteric tradition may be conceptualized as part of the latency subsystem of Western society, following Parsons's model of structural differentiation of social systems of action (Parsons 1969). Further, we contend that in the Western case, at least, (a) such esoteric conceptions and modes of interpreting reality form a cultural paradigm which provides leverage against the institutionalized paradigm, hence function as a seat of inspiration for new systems of social action; and (b) that at various historical points these conceptions and modes have come into play in the larger society so as to provide vehicles of social and cultural change.

Moreover, albeit to document this point would require much more space than here available, the basic cognitive model of hidden, underlying reality central to esoteric thought, is that of a reality moved by forces, by energies constantly in motion and in tension with one another; it is a model opposed to the static, stable, or harmonious view of things inherent in the natural attitude. Hence, we suggest, at the very heart of the ideology of modernization, or modernism, is an esoteric influence. This ideology, which positively evaluates what is new as against what is old, which sees the unfolding present as a time of liberation and freedom from the yoke of the stagnating past, is an ideology of a new order of things to come and of a this-worldly salvation by human means—an ideology which at least one

author (Voegelin 1952, p. 133) has seen as a product of esoteric (gnostic in this instance) symbolism manifested in the development of the Puritan Revolution.

Of relevance here is to discuss the relation between esoteric culture and avant-garde culture, for the esoteric apprehensions, depictions, and interpretations of reality find a ready-made terrain of expression in artistic products (literature, painting, architecture, even music) whose esoteric significance escapes the larger public. To the initiates such symbols are more than artistic: they also convey a message, and hence are expressive symbols in the fullest sense.

In a recent article pointing out the antithetical relation of the social structures of industrial-technological society and modern culture, Daniel Bell (1970) has noted in the latter the prevalence of a dominant impulse "towards the new and the original . . . so that the *idea* of change and novelty overshadows the dimensions of actual change" (1970, p. 17). This ideology of change, of modernism per se, is one which Bell locates in the cultural tradition of avant-garde art that appeared in the 19th century as a counterculture to the rising bourgeois culture of industrial society.¹⁵

Bell does not examine any linkage of occultism and the formation of avant-garde in the 19th century, although he proposes in passing (1970, p. 34) that what avant-garde values stand for—antistructure, antihistory, radical freedom, in brief, values of nihilism and anarchism—are part of an older Western tradition, that of gnostic esotericism.¹⁶

In a complementary article, Vytautas Kavolis has explored recently (1970) the sociopsychological nexus between avant-garde culture and what he calls "Satanic" and "Promethean" personality modes, the former characterized by a "resentment-destruction mechanism," the latter by a "sympathetic concern for the needs of others."¹⁷ These personality orientations are manifested in the activist and nihilistic aspects of avant-garde culture, oriented to both the destruction of the established order of things and to a perpetual innovation and renovation of forms. Moreover, Kavolis sees avant-garde culture as giving positive value to the symbols of the

¹⁵ For a fuller discussion of the rise and characteristics of avant-garde literary culture, see Irving Howe's essay, "The Idea of the Modern" (Howe 1967, pp. 11-40).

¹⁶ In a similar vein, Edgar Morin has spoken of the revival of astrology today as a manifestation of a new gnosis (Defrance et al. 1971, p. 123). See also the remarks of Hans Jonas (1963) on gnostic features of some modern political tendencies, including the theme of alienation.

¹⁷ Students of astrological traits might note with amusement the congruence of the Promethean and the satanic with the two sides of the Aquarian-born (today's *enfants du siècle*): "A negative Aquarian is one who . . . demands licence under the guise of liberty, and shouts for public service while serving none but himself. The positive Aquarian is . . . the humanitarian, seeking liberty not for self, but for others, dealing in the larger affairs of the country or planet, ever urging humanity upward and onward, and running ahead to show the way" (Keane 1967, p. 34).

satanic psychological mode of orientation, symbols treated as the epitome of antisocial values in the established order of society: "To some extent, the avant-garde culture could be interpreted as an attempt to legitimate much of what used to be illegitimized in the Satanic mythology" (1970, p. 27).

The influence of occult themes, particularly those dealing with the demonic and the satanic, is a striking aspect in the historical development of modern avant-garde culture, which is viewed here as both a literary and a political protest against the institutionalization of a rationalistic-industrial bourgeois social order. This protest against modernization was a major common denominator in the Romantic movement, and esoteric culture provided much of the materials for the protest against the new social order, though neither esoteric culture nor avant-garde culture sought a return to the *ancien régime*. Ritual magic, "forces of darkness," Satan himself, became symbols of identification, rallying points, in brief, revolutionary forces drawn from the counterculture of the occult,¹⁸ in the fight against bourgeois values. The occult as a source of inspiration abounds, then, in writings of well-known and lesser-known Romantics, such as Goethe (Lepinte 1957), Novalis, Gautier, Nerval, Byron, Lautréamont, and Baudelaire (Bays 1964).¹⁹

Such themes of the occult were continued by later generations of the avant-garde culture,²⁰ notably the "accursed" symbolist poets such as Rimbaud and Verlaine (Senior 1959), but also Yeats and Thomas Mann, and finally, André Breton, the crucial figure of surrealism, a movement of particular sociological significance since it represents a clear articulation of artistic protest and political radicalism.²¹ Many of the surrealists (Breton himself, Aragon, Eluard, Naville) were or are politically committed to the radical left, and the influence of surrealism is even to be

¹⁸ For a fuller statement on the social significance of Satan in the last century, see the cogent remarks of Eugen Weber (1964).

¹⁹ Note here the observation of Howe (1967) in commenting upon a passage of Baudelaire: "[This seems] the report of a desire to create . . . the very ground of being, through a permanent revolution of sensibility and style, by means of which art could raise itself to the level of white or (more likely) black magic" (p. 17).

²⁰ To do full justice to the role of esoteric culture as an inspirational source for avant-garde culture, one should also examine esoteric influences on earlier literary and artistic innovators, such as Shakespeare (Arnold 1965; Reed 1965) and Rabelais (Masters 1969), to say nothing about esoteric currents in painting and architecture (Van Lennep 1966).

²¹ In commenting on "Surrealism and Revolution," Camus stated: "The essential enemy of Surrealism is rationalism. Breton's method . . . presents the peculiar spectacle of a form of Occidental thought in which the principle of analogy is continually favored to the detriment of the principles of identity and contradiction. . . . Magic rites, . . . alchemy . . . are so many miraculous stages on the way to unity and the philosopher's stone" (Howe 1967, p. 218).

found in the writings of A. Césaire, one of the founders of *Négritude*, the cultural arm of black liberation. Breton drew upon various sources of inspiration (including Marx, Freud, and occultism) to formulate a revolutionary consciousness aimed against the bourgeois world (of utility, reason, realism, and technological society), a consciousness whose intention "is always to allow the *irruption* of 'wild' images that will disturb the sensibility by shattering the coherence of those 'stable' images that make up, for each individual, the objective world (Willener 1970, p. 224).

The contemporary significance of surrealism as a source of inspiration for the May 1968 revolutionary movement in France has been fully discussed in an excellent sociological study by Willener (1970).²²

In the political development of modern society, both in the West and in the Westernization (including imperial domination) of the "Third World," esoteric culture has also had an influence in avant-garde political movements and ideologies which have pitted themselves against established regimes. A major social vehicle of such protest have been secret societies: Weishaupt's Bavarian Illuminati in the 18th century, Freemasonry in France in the 18th and 19th centuries, the Carbonari in Italy and France, Mazzini's Young Italy, the Sinn Féin in Ireland, and many others. For the most part, the ideology of such secret societies was nationalistic, republican, anticlerical, even internationalistic. All these drew upon the imagery and expressive symbols of esoteric conceptions of reality, especially symbols of the liberation of man from the yoke of darkness (politically interpreted as the yokes of traditional political institutions or alien oppressors) into the realm of light, of freed humanity. For the most part, these movements succeeded in establishing political regimes which at least partly satisfied their aspirations.²³

Assuredly, not all secret societies drawing upon esoteric symbols, rituals, and interpretations can be classified as progressive, for there is the minority case of those having a reactionary image, such as the Ku Klux Klan in the United States, the Cagoule in the France of the 1930s, and even, in the light of the discussion provided by Pauwels and Bergier (1968), there were esoteric influences operative in secret societies (such as the Thulé order)

²² The hexing of the Pentagon and the Stock Exchange (to say nothing of an Establishment department of sociology) and the adoption of the acronym WITCH attest in their own modest way to the appeal of occult symbolism in some sectors of radical students. To my knowledge there was no manifest influence of surrealism in these instances.

²³ Thus, the very symbols of the Great Seal of the United States (the luminous delta, the pyramid) are esoteric symbols of Freemasonry, to which belonged most of the founding fathers; similarly for the symbols of the French Republic (especially that of 1848), whose motto "Liberty, Equality, Fraternity" represents cherished ideals of Masonry formulated in lodges before the French Revolution. Simón Bolívar, the liberator of South America from Spanish rule, was also imbued with Masonic ideals, as was Garibaldi in Italy, etc.

that played a covert role in the formation of nazism. This is perhaps indicative of the fact that esoteric culture, with its fantastic wealth of imagery and symbolism, is multivalent in terms of political expressions that can be derived from it. Yet, whatever the specific instance, it may be said that esoteric culture provides leverage against the existing order by grounding political reflection and action in a reality that transcends that of everyday life,²⁴ but which is a reality that may become actualized in the historical future by reversing the present order of the world.

One other aspect of the political expressions of modern counterculture to be noted is that of Marxism. Its crucial leverage against bourgeois mentality is its formulation of dialectical materialism, which provided Marx and Engels the key with which to unlock the hidden laws of the historic process. The intellectual progenitor of modern dialectics is commonly taken to be Hegel, but the recent study of Benz (1968) has demonstrated the extent to which esoteric and mystical sources figure in the German intellectual background of Hegelian and Marxist thought. Notable sources are theosophy, philosophical alchemy, and the Kabbalah, drawing much inspiration from the writings of earlier mystics such as Meister Eckhart and particularly Jacob Boehme, and reinterpreted in early 19th-century German theosophic and evangelical circles, as well as philosophical circles which also partook of the Romantic movement.²⁵

A last consideration involving the outputs of esoteric culture to the development of modern Western civilization is its relation to scientific thought. The latter in its empiricist and positivistic image of an objective reality, measurable by empirical means and existing independently of the human subject, is perhaps the key mode of thought in the modernization process which has illegitimated and devalued the practices of occult science. Yet, paradoxically, esoteric influences in the form of symbolisms, imagery, practices, and cosmologies, have been in much of the background of the rise of scientific disciplines.

In the case of modern depth psychology, Bakan (1958) has brought out

²⁴ S. N. Eisenstadt's evaluation of the ability of Chinese Communists to draw upon various threads of protest in Chinese society and wield them into a common cause is highly germane here. He signals in particular that such a linkage "enabled some gentry groups . . . some secret societies, some warlords and some peasant rebels to go beyond their own restricted social orientation and to find a wider basis and forge out new, broader orientations" (1971, pp. 49 f.). It is our contention that these orientations are likely to have originated, at least in part, in esoteric doctrines formulated in secret societies. In the case of China, of noteworthy mention in the rise of national consciousness is the case of the Taiping (Shih 1967).

²⁵ Among other things contained in the esoteric conceptions of the theologian Oetinger, a generation before Marx, are such themes as an eschatological view of history and the coming freedom of man in a "Golden Age" in which the state, private property, and a money economy will disappear in communal equality and love (Benz 1968, pp. 32-53).

various elements of the Jewish mystical tradition which frame much of Freud's psychoanalysis; the latter's theory of the libido and the significance of symbols in the psychic process (including the analogic linkage of symbols) may be seen as a scientific formulation of elements of the Kabbalah and the Zohar. His one-time heir apparent, Jung, made intensive studies of medieval and Renaissance alchemy (Jung 1968*a*, 1968*b*) which he linked to personality development. Freud was early influenced by Charcot's experiments in hypnotism, but the foundations of hypnotism had been laid nearly a century before by the occultist Mesmer's demonstrations of magnetism (Mesmer 1971).

Even in the natural sciences esoteric influences have played a not insignificant role, which can only be mentioned in passing. If today the alchemical symbols of chemical elements remain a glossed-over vestige, the development of chemistry from its matrix in late medieval alchemy deserves mention (e.g., Stillman 1960). Alchemy and astrology may also have been of importance in the rise of modern medicine, with Paracelsus being another key figure mediating between esoteric culture and modern scientific thought—the innovation of operating on human bodies may have been guided by an attempt to establish the correctness of astrological views that different parts of the body, and consequent pathologies, are under the influence of different signs of the Zodiac. Astrology and theosophy were also part of the cultural baggage which was utilized rather than rejected by modernizing scientists, such as Kepler and even Newton (Hutin 1960). The very social organization of modern science, in the form of academies of science, owes much inspiration to Francis Bacon's *New Atlantis* with its "scientists' Paradise," a work said to come out of the "Hermetic-Cabalist" stream (Yates 1964, p. 450).²⁶

So much for some indications, necessarily brief and incomplete, of the range of esoteric influences in the historical process of modernization.

CONCLUSION

Marginal as the occult revival may initially appear to sociological pre-occupations, the study of the esoteric in fact touches on many facets of our discipline, such as the sociology of knowledge, the sociology of art, the sociology of religion, and the sociology of deviance.²⁷ For heuristic

²⁶ Once organized, the scientific community broke formally with esoteric culture. This was symbolized in 1666 upon the establishment of the French Academy of Sciences, membership in which excluded the practice of astrology. A few years before France had still had an official state astrologer, Morin de Villefranche, professor of mathematics at the prestigious College de France, who had sought to modernize and rationalize astrology with mathematical precision.

²⁷ In the context of the latter, see the study of differential reactions to witchcraft

purpose, this essay has mainly placed the emphasis on esoteric culture as an important promulgator of a counterculture of long standing in the West. Essentially, we have viewed the function of esoteric culture to be that of a seed-bed cultural source of change and wide-ranging innovations in art, politics, and even science, analogous to the functions of "seed-bed societies" discussed by Parsons (1966) in the case of Israel and Greece.²⁸

In discussions of social change elsewhere (Tiryakian 1967, 1970), I have proposed that important ideational components of change (i.e., changes in the social consciousness of reality) may often originate in non-institutionalized groups or sectors of society whose paradigms of reality may, in certain historical moments, become those which replace institutionalized paradigms and become in turn new social blueprints. Relating this to the present essay, I would propose that esoteric culture, and groups of actors mediating esoteric to exoteric culture, are major inspirational sources of cultural and social innovations.

To document and validate this model is obviously no easy matter. It involves demonstrating the meaningful sociohistorical affinity between seemingly heterogenous social spheres of action, a methodological problem of exactly the same nature as the one involved in Max Weber's study of the affinity of ascetic Protestantism and the ethical basis of modern capitalism. The magnitude of the problem involved in relating esoteric culture to social innovations in exoteric culture is even greater than the Protestant case. It involves developing tools of analysis which will enable us, as sociologists, to make sense out of esoteric texts and documents, many of which depend on being able to decipher the meanings of expressive symbols which are by their nature qualitative expressions of reality, and not subject to quantitative measurement in their presentation. A promising avenue here may be the techniques of linguistic and structural anthropology, for example those used in deciphering mythologies, which have similarities to esoteric conceptions of reality. The increasing applications of phenomenology (which is, after all, oriented to the inner grasping of structures of consciousness) in the social sciences (Natanson 1972), such as those being developed by ethnomethodology, may be particularly fruitful in this vein.²⁹

The problem of indicating linkages between esoteric symbolisms, imageries, and conceptions of reality to purposive social behavior, that is, the

in England and on the Continent by Currie (1968). For other materials on witchcraft, deviance, and social structure, see the New England study of Erikson (1966).

²⁸ We might note in passing that Western esoteric culture has deep historical roots in cultural aspects of both Israel and Greece, notably the prophetic and Kabbalistic traditions of the former and the rituals of the mystery cults of the latter.

²⁹ A student of Harold Garfinkel, Trent Eglin, is presently preparing an ethnomethodological study of alchemy.

question of how conceptions of the structure of reality translate into paradigms of social action and social imagery (e.g., how the esoteric notion of the androgyne, Adam Kadmon, is linked today to fashion designers of unisex clothes) is also laden with serious methodological difficulties. And this is rendered even more serious by the fact that major mediating groups between the two cultures tend to operate in socially invisible or secret social organizations, which means a paucity of readily available informative documents, or in some cases, an undue reliance on records of governmental and other institutional agencies which have sought to repress these esoteric groups.

Nonetheless, these methodological vexations should be seen as more of a challenge to the sociological imagination than in principle insoluble. The very framing of theoretical questions concerning the relation of esoteric to exoteric culture, particularly in the area of conceptualizing the dynamics of social change, may suggest new methodological developments to the sociological enterprise.

A final note on the occult revival may be apposite in reconsidering the crucial notion of the modernization process. It may be fruitful to view modernization in a broader historical context than just that of the past 200 years or so, to view it instead as stretching back to antiquity, in the course of which modern ideas have dislodged previously institutionalized paradigms during "crucial periods"³⁰ of social change—a recurrent feature in the development of Western society. The net effect has been, to venture an analogy, a stochastic process of change rather than one of continuous development, one punctuated by adaptive mutations in the cultural code of Western civilization, to propose yet another analogy.

In the historical unfolding of Western civilization, occult revivals have attended such crucial periods of transition from one cultural matrix to another. The waning period of the Roman Empire is a case at hand, with a great flourish of esoteric culture and symbols (much of which was absorbed in primitive Christianity prior to its institutionalization under Constantine). The Renaissance/Reformation period is another major one of shifting cultural paradigms, representing a rejection of the rationalism of medieval scholasticism and of established ecclesiastical authority, having consequences in a variety of social domains. It is during this period, and not the antecedent medieval period, that there was a major occult revival, with esoteric culture becoming a major vehicle of new expressive symbols and belief systems, a source of new value orientations.³¹

³⁰ I have taken this suggestive term from Balandier (1971a, p. 202) in preference to the overworked "periods of crisis." In this and other writings (1970, 1971b), Balandier has been developing a theory of modernity which seems particularly fruitful for reformulating the dynamics of change.

³¹ For an outstanding historical study of this cultural context, see Yates (1964).

In both of these instances the particular thrust of efficacy of esoteric culture lay, I would suggest, in the exoteric culture having what may be characterized as a loss of confidence in established symbols and cognitive models of reality, in the exhaustion of institutionalized collective symbols of identity, so to speak. There was what may be called a "retreat from reason into the occult" (Yates 1964, p. 449), a retreat not in the sense of a total "leaving of the field," to borrow from Kurt Lewin, but rather in the sense of a religious retreat, a temporary withdrawal for inspirational meditation which provides a restoring of psychic energy to be used in re-entering the everyday life with greater vigor.

The occult revival of today is, in this perspective, comparable with previous such phenomena, even including the contemporary attacks against institutionalized rationality, attacks which take irrational forms in the now generation. To make sense of the irrational, the modes and conditions in which it occurs, as well as possible societal consequences deriving from it, is a basic concern of the sociology of the occult. If we come to perceive the occult revival of today not as an ephemeral fad of mass society but as an integral component in the formation of a new cultural matrix, more likely international than national in scope, if we see it, in brief, as an important vehicle in the restructuring of collective representations of social reality, we will see (with or without the third eye) the Age of Aquarius as a major sociological happening.

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Two Sources of Antiwar Sentiment in America¹

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Opposition to the Vietnam war has been manifested both in university protest actions and in cross-section public opinion surveys. But the college-related protests and the wider public disenchantment have sharply different characteristics: they have peaked at different points in the war; they are discontinuous in educational and age basis; and a substantial part of the antiwar public is also extremely hostile toward college protesters. Together these findings suggest a distinction between moral criticisms of the goals and nature of the war and pragmatic disillusionment over failure to win it. This hypothesized distinction is investigated using thematic analysis of open-ended responses from a cross-section sample of 1,263 Detroit adults who had indicated opposition to American intervention in Vietnam. A small classroom sample of University of Michigan students is also used for comparison purposes. The themes emphasized by the Detroit sample as a whole, and by most subcategories defined in terms of race, sex, age, and education, are generally consistent with the moral-pragmatic distinction. Other related factors (such as traditional isolationism) are also shown to contribute to broader public disenchantment with the war. The moral-pragmatic distinction, while somewhat oversimplified, is useful in considering public reactions to future wars of the same general type.

Two distinct measures of opposition to American involvement in Vietnam can be traced over the past seven years. One is the intensity and scope of college-related protests against the war. The other comprises the results of Gallup polls and similar opinion surveys based on cross sections of the entire American adult public. Both measures show increasing disenchantment with the war, and it is easy to treat them as simply two aspects of the same thing. There is some truth to this, but even more error. The college-based protest has focused on moral objections to the use of American military power in Vietnam. The general public disenchantment, however, seems to have been largely practical, springing from the failure of our substantial military investments to yield victory. Confusing these two sources of opposition to the war leads to serious

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miscalculations about the relationship between mass opinion and presidential action in matters of war and peace.

THREE DIFFERENCES BASED ON PAST SURVEY FINDINGS

The first major campus protest occurred in March 1965 at the University of Michigan. A handful of faculty members and students created the "teach-in" as a way of arousing opposition to the recently initiated bombing of North Vietnam. Several thousand members of the university community attended the all-night series of lectures and discussions about the war, concluding on an emotional note with songs and oratory. Above all, there was a strong tone of moral indignation in the teach-in. The United States was pictured as intervening in what was essentially a civil war, supporting a corrupt and unpopular government, and now extending the war dangerously by sending modern bombers to pound the countryside of North Vietnam. The main issue presented was not whether the United States could win the war but rather the devastation that such a victory would entail.

Similar protests spread rapidly in the next months to other major campuses. Later, they expanded beyond the campus and led to massive demonstrations in Washington, New York, and other cities. This expansion was characterized by the same moral emphasis seen in the first teach-in. A whole generation of students, especially at major universities, is learning to criticize our involvement in Vietnam as not merely unsuccessful but also morally wrong.

It is commonly assumed that the public slept until awakened by the college protests. Since then, public opinion is thought to have moved in much the same direction, though more slowly, and with uncertainty and occasional backtracking. This movement of national opinion can be traced by the one question that the Gallup organization has repeated regularly since 1965:² "In view of the developments since we entered the fighting, do you think the United States made a mistake in sending troops to fight in Vietnam?" Results for this question show a large and unmistakable trend in growth of opposition to the war over the seven-year period. In August 1965, only 24% of the population believed our intervention a mistake; by May 1971 that figure had climbed well past a majority to 61%.

So far it appears that public opinion has followed much the same course as the college protest. But a closer look at dates and events reveals some

² These and other Gallup findings reported came from the monthly publication *Gallup Opinion Index*. Further descriptive analysis of these results and of related data from the University of Michigan Survey Research Center and other sources can be found in Converse and Schuman (1970), Davis (1970), and Mueller (1971).

important differences. The first teach-in, for example, was created to protest a major new employment of military force by the United States—the bombing of North Vietnam. The countrywide campus strikes in May 1970 were directed in good part against still another example of expanding American military power—the thrust into Cambodia. These two examples reflect the fact that university protests have been provoked primarily by anger and dismay over *offensive* military actions by the United States.

When we search the polls for similar turning points in the trend of general public opinion, we find them at different locations. The most dramatic change in survey trends on the war is best reflected by a “hawk-dove” question that Gallup administered at several points in 1968 and 1969: “People are called ‘hawks’ if they want to step up our military effort in Vietnam. They are called ‘doves’ if they want to reduce our military effort in Vietnam. How would you describe yourself—as a hawk or a dove?” Just before the Tet attacks in January 1968, with American leaders confidently predicting victory, the number of self-described hawks outnumbered doves by two to one. But two months after Tet the proportion of doves in the country slightly exceeded that of hawks, and by the end of the same year, doves outnumbered hawks by nearly two to one. The shift in a space of just 60 days represents probably the largest and most important change in public opinion during the entire war.³ (The “mistake” question quoted above also reveals a sharp drop in support for the war over the first eight months of 1968; the slope is less precipitous probably because the question was less fitted to immediate policy directions.) These transformations came, it will be noted, not in reaction to expanding American power, but in response to a widely advertised American defeat.⁴ It is almost as though a sizeable number of Americans had

³ The figures for several key dates are:

| | Hawks | Doves | No Opinion | Total |
|-------------------------|-------|-------|------------|-------|
| (a) January 1968 | 56 | 28 | 16 | 100 |
| (b) March 1968 | 41 | 42 | 17 | 100 |
| (c) April 1968 | 41 | 41 | 18 | 100 |
| (d) November 1969 | 31 | 55 | 14 | 100 |

The Tet Offensive occurred between the (a) and (b) measurement points, while President Johnson's announcement of a partial bombing halt (and his decision not to seek reelection) occurred between (b) and (c). Thus the decisive change in early 1968 seems attributable to the offensive itself, rather than to the presidential policy announcements. The subsequent decline in number of hawks over the next year and a half is no doubt more complicated and represents the basic acceptance by both political parties of the bombing halt, the Paris talks, and later the troop withdrawals. By the end of 1968, almost all political leaders had become “doves,” at least in rhetorical expression.

⁴ Oberdorfer (1971) offers a persuasive case for the official American interpretation of Tet as a military defeat for the Viet Cong and North Vietnamese. But he also

suddenly concluded that the war was not about to be won by being "stepped up," and that hence the only alternative was to step it down. More generally, the curve of broad public disenchantment with the war seems to reflect not offensive actions but news of defeats, casualties, and frustrations.⁵

A second divergence between campus-related protests and general public opinion appears in the reaction of the public to antiwar demonstrators. The gathering of more than a quarter million people in Washington in November 1969 drew heavily from colleges and universities but was intended as evidence of the extent of opposition across the country to the administration's prolongation of the war. Great pains were taken to keep the demonstration peaceful, so that it would appeal to the public at large. Yet the following month the Gallup Poll showed a 6% *rise* in public approval "of the way President Nixon is handling the situation in Vietnam." The president's speech a few days before the march undoubtedly had some influence in rallying public opinion to his side. But there is also reason to think that the demonstration itself had a negative effect on parts of the public unhappy with the war but even unhappier with demonstrators.

Poll data show clearly that open protest against the war is not well regarded by the great majority of American adults. In 1968 the University of Michigan Survey Research Center asked a national sample to indicate their "feelings" toward "Vietnam war protesters" on a scale ranging from zero (very unfavorable) to 100 (very favorable), with a neutral midpoint of 50. It is perhaps no great surprise that seven out of every 10 adults placed protestors on the negative half of the scale. What may seem strange is that extreme dislike of war protestors was shown by many people who on other questions indicated their own opposition to the war. For example, one question in the survey asked "Which of the following do you think we should do now in Vietnam?" Three alternatives were given:

1. Pull out of Vietnam entirely.
2. Keep our soldiers in Vietnam but try to end the fighting.
3. Take a stronger stand even if it means invading North Vietnam.

documents graphically the way in which the Communist offensive was interpreted by the American public as new and dramatic evidence that the war was far from over and far from being won.

⁵ Mueller (1971) suggests that both the Korean and the Vietnam wars can best be explained in terms of an initial "rally round the flag" enthusiasm, followed by a drop in support as the costs, frustrations, and length of each war became clear. The Chinese intervention in Korea had an impact even greater than Tet in shattering expectations of a quick and decisive victory. Mueller seems to argue against the importance of any single similar traumatic event for the Vietnam war, but he does not deal explicitly with Tet, or with the hawk-dove question we have reviewed here.

Of those who chose the first response, calling for unilateral withdrawal, more than half nevertheless placed "Vietnam protestors" on the negative side of the feeling scale. One out of four of these extreme doves placed protestors at the absolute bottom of this 100-point scale. These findings raise serious questions about the effect of massive antiwar demonstrations. When the television cameras focus on the protestors themselves, rather than on the object of protest, Vietnam, the demonstrations probably lead many people who are against the war toward support for the president.

One more important set of findings from national surveys points to the divergent types of antiwar sentiment. As we have already noted, the most forceful dissent over Vietnam has come from students and faculty in leading universities. These articulate opponents of the war have tended to assume that their potential allies in the general public are the most educated and informed segments of the population. Such is indeed the case on many issues—for example, questions involving civil liberties—where the universities provide forward positions which then find public support in direct proportion to the education of those questioned (Stouffer 1955).

Contrary to common belief, however, this has not been the case with the Vietnam war. Analysis of poll data shows more educated sections of the public to have generally provided the greatest support for continuing American involvement. In February 1970, for example, Gallup asked its national sample: "Some U. S. Senators are saying that we should withdraw all our troops from Vietnam immediately—would you favor or oppose this?" Of those having an opinion, more than half the grade-school-educated adults favored immediate withdrawal, about two-fifths of those with high school backgrounds, and only 30% of those with at least some college. This was not a fluke. In May 1971, 66% of those college-educated persons with opinions claimed that the war was a mistake, but the figure rose to 75% among the grade-school-educated. In general, a careful review of public opinion data over the last seven years shows that on most war-related issues, the greatest opposition to continued American involvement in Vietnam has come from the least educated parts of the population.⁶

A related finding is that when it comes to Vietnam, the "generation gap," at least in a simple form, has been largely a myth. Age differences in the general public have been neither great nor consistent. On the

⁶ The socioeconomic findings from survey data are supported by the results of census tract analysis using cities and towns that have carried out referenda on the Vietnam war (Hahn 1970). More detailed analysis *within* the college category reveals that opposition has been great in high-quality college groups, exactly as one would expect from the fact that the protest movement began at such places as Columbia, Harvard, and Michigan (see Converse and Schuman 1970; Robinson and Jacobson 1969).

question about immediate withdrawal mentioned above, those under 30, those 30-49, and those over 50 all show much the same pattern of responses. More recently, young people do call for a faster rate of withdrawal, but older people continue to be more likely to regard the war as a mistake.

What sense can we make out of these poll results, especially when they contradict what our eyes and ears reveal about the intensity of antiwar feeling among youth on college campuses? The first thing to realize is that college students comprise less than half the college-age population in the United States. More particularly, it would be quite possible to have every student in the major universities in complete opposition to the war, yet find the total college-age population showing strong support. To this we must add the obvious but easily forgotten fact that in national surveys, "college"-educated persons are primarily adults who are well past college age at present, so that we cannot expect them to reflect recent changes on campuses.

Once we realize that students (and faculty) at Columbia, Michigan, or Berkeley cannot tell us much about the degree of public opposition to the war, we must also recognize a more subtle point: the *basis* for disenchantment with Vietnam need not be the same in the general population as on the campus. Why, indeed, is there public opposition to the war? This is such a simple-minded question that it may seem absurd even to raise it. The fact is, however, that we do not know the answer. Gallup and other polls have documented well the growing negative sentiments on the war, but almost no effort has been made to explore the reasons behind these sentiments. Such exploration requires open-ended questions that ask people to state in their own words why they hold a particular policy position.⁷ Questions of this sort, however, are expensive to include in interviews and complex to analyze and report. Unfortunately, their omission has a more dangerous effect than simply leaving us ignorant, for in the absence of knowledge of public opinion we all have a tendency to project our own views onto the population as a whole. This is particularly true when one tries to interpret the reasons behind a position with which one agrees. If one feels that American involvement in Vietnam was a mistake and then reads that two-thirds of the population also says that it was a mistake, it is quite natural to assume the reasons are the same in both cases. But of

⁷ The other alternative is structural analysis of a large set of interrelated closed questions, as is done insightfully by Modigliani (1972) on poll data from the Korean War period. Ideally both approaches should be used. In the present case, where we are attempting to discover basic frames of reference, it seems to me, as it did to Robinson and Jacobson (1969), essential to be able to draw on relatively unprompted verbalizations by respondents. Of course, both these approaches deal with overt "reasons" and "goals," and still further analysis of social and psychological motives is possible; some steps in this direction are indicated later.

course this need not be true, any more than it is true that votes for a Democratic presidential nominee by Mississippi whites and New York blacks spring from the same motives and concerns.

A BROAD HYPOTHESIS

In the summer of 1971 the Detroit Area Study included an open question to provide some insight into the basis of public thinking about Vietnam. Before reviewing those data, let us summarize in the form of a general hypothesis the argument developed thus far. We have seen that public opinion has been turned against the war mainly by reversals such as the Tet offensive. It is also clear that a good part of the public now opposed to the war is also opposed to the antiwar protests and, presumably, the beliefs that they symbolize. Finally, we have found that the larger public opposition to the war includes a substantial proportion of people who are low in education, not very interested in the war, and about as likely to be older as to be younger.⁸ What these several pieces of evidence suggest is that much of the disenchantment with the war registered in public opinion polls is of a purely pragmatic character, with little or no concern over the morality of employing American power in Vietnam. Disenchantment with the war is based on our visible lack of success in winning it. Mounting American casualties, the failure of so many optimistic predictions by American leaders, the surprising resilience of enemy forces—all these factors have taken their toll in supporters of the war. More and more Americans now think our intervention was a *military* mistake, and want to forget the whole thing. This explains why the Tet offensive had such a disastrous effect on public opinion, while the My Lai massacre caused hardly a ripple in the polls.

We can also understand in these terms why almost *any* administration action that seems to point toward an end to the war wins at least a brief rise in public support. When American planes first crossed the North Vietnam border, the public hoped that a serious blow had at last been struck against enemy sanctuaries. We lack adequate survey data from that period, but what data we have suggest wide support for the bombing. Five years later, when American troops crossed the Cambodian border, a less optimistic public still showed initial acceptance of another president's claim to be reaching important enemy sanctuaries.⁹ In between and subsequently,

⁸ On "interest" in the war, see Converse and Schuman (1970) and, using earlier data, Verba et al. (1967).

⁹ At the time of the Cambodian incursion (May 1970), about 50% of the public regarded our original intervention in Vietnam as a mistake. But a very similar question asked by Harris (1971, p. 124) about Cambodia in May 1970 produced only 39% who thought this new military action was a mistake. By July 1970, even this

almost every action that promised to hasten the end of the war, retreats as well as offensive actions, has won support from a large part of the public.

The distinction between moral and pragmatic opposition to the war is blurred by the policies of troop withdrawal, since these tend to be supported by both viewpoints. But the invasion of Cambodia could appeal only to the pragmatic opponent of the war. Provided he accepted the president's assurance that "cleaning out the sanctuaries" would speed troop withdrawals, the pragmatic objector could support the invasion. To the moral opponent of the war, however, the Cambodian intervention, no matter how limited in time or successful in outcome, meant that the destruction of war was now to be brought to Cambodian villages. On this issue, on the more recent American bombing, and perhaps on issues still to come, the different sources of opposition to the war lead to sharply different stands even among the majority who want an early end to American involvement.

METHOD AND SOURCE OF DATA

Let us turn now to the reasons that the general public gives for opposing the war. From April through August 1971, the Detroit Area Study interviewed an area probability cross-section sample of 1,881 persons, 21 years old and over, in the metropolitan Detroit area.¹⁰ The interview included the Gallup "mistake" question discussed earlier; it was followed by open probe questions to those who said they thought our sending troops to Vietnam was a mistake. The probes were simply: "Why would you say it was a mistake?" and "Is there any other reason why you think it was a mistake?" The instruction to interviewers was to be completely nondirective but to encourage full responses. The resulting verbal data from the 1,263 respondents who said "yes" to the closed question have been coded in terms of 10 broad themes developed partly on the basis of theoretical expectation and partly after carefully reviewing 100 responses chosen at random. In the final coding, each of the 1,263 responses was coded zero for a particular theme if that theme was not mentioned; if the theme was mentioned, the response was further categorized in terms of the way the theme was treated. Table 1 presents the five themes that are most relevant for our analysis, along with the marginal percentages for the metropolitan

39% had shrunk to 24%, presumably on the basis of reports that the incursion had ended and had been relatively successful. Thus doubts about Vietnam did not prevent hope for the success of a similar military operation in another country.

¹⁰ A report on the sampling design for the 1971 survey can be obtained from the Detroit Area Study, University of Michigan, Ann Arbor. The geographic area covered is the Detroit Standard Metropolitan Statistical Area (SMSA), minus the city of Pontiac and the outlying semirural areas of the three-county area; it includes about 85% of the SMSA population.

TABLE 1
REASONS AGAINST U.S. INTERVENTION: MAJOR THEMES CODED
(WITH PERCENTAGES FOR DETROIT SAMPLE)

| Theme | % |
|--|---------|
| I. U.S. Not Winning War: | |
| 0. Theme not mentioned | 66 |
| 1. The war is unwinnable | 10 |
| Example: "It can't be won militarily; it's guerilla warfare, not like World War II or Korea." | |
| 2. We are not trying to win the war | 8 |
| "Win or get out." | |
| 3. We are not winning (stated as a fact with no additions) | 1 |
| "We're just getting beat like crazy." | |
| 4. The war is not ending (low priority relative to 1, 2, 3) | 16 |
| "The war just goes on and on." | |
| Total | 101 |
| N | (1,263) |
| II. People Killed or Injured by the War: | |
| 0. Theme not mentioned | 58 |
| 1. American soldiers killed or injured | 28 |
| "So many boys being killed." | |
| 2. American soldiers hurt in other ways | 2 |
| "All those soldiers getting the dope habit." | |
| 3. People killed or injured: identity ambiguous | 7 |
| "So many innocent lives have been taken." | |
| 4. Both Americans and Vietnamese explicitly mentioned; objec- tions to all war | 3 |
| "I hate violence." "Too many Americans and Vietnamese killed." | |
| 5. Vietnamese killed or injured (includes references to any Viet- namese, on either side, either civilian or soldier) | 0 |
| "Thousands of Vietnamese killed by the mass bombing." | |
| 6. Vietnamese people hurt in other ways | 1 |
| "We make racketeers out of the people and prostitutes out of the women." | |
| Total | 99 |
| N | (1,263) |
| III. Loss of U.S. Resources: | |
| 0. Theme not mentioned | 80 |
| 1. U.S. resources wasted: no mention of alternative social uses ... | 9 |
| "It's ruined our economy." | |
| 2. U.S. resources wasted: explicit mention of alternative social uses | 3 |
| "We send money there and there's poverty here." | |
| 3. War causes polarization in U.S. | 4 |
| "All the young people are turning against the country." | |
| 4. We have enough problems of our own to take care of (vague; low priority) | 4 |
| "Enough problems here at home." | |
| Total | 100 |
| N | (1,263) |
| VII. Vietnam War Is Internal Conflict | |
| 0. Theme not mentioned | 54 |
| 1. It is a civil war (codes only clear references to <i>civil</i> war) | 5 |

TABLE 1 (*Continued*)

| Theme | % |
|---|---------|
| 2. Vietnamese don't want us there "The Vietnamese don't care who wins, just want to be left alone." | 5 |
| 3. The war is the Vietnamese responsibility, not our war "Let them fight their own war." | 16 |
| 4. Our intervention worsened the conflict (low priority) "We changed a small war into a bigger one." | 1 |
| 5. Shouldn't meddle in other people's business (low priority) "Too messed up . . . we should not get involved in other people's troubles." | 19 |
| Total | 100 |
| N | (1,263) |
| VIII. U.S. Goals Morally Questionable: | |
| 0. Theme not mentioned | 89 |
| 1. U.S. motives wrong or questionable "Our efforts at world domination are subject to question." | 3 |
| 2. We shouldn't force our way of life on Vietnam "Who are we to say what is the right way there." | 6 |
| 3. North Vietnamese or Vietcong justified "North Vietnamese form of Communism is the best way for them." | 1 |
| 4. The war is immoral or wrong; no further explanation (low priority) | 1 |
| Total | 100 |
| N | (1,263) |

Detroit sample.¹¹ The themes were not mutually exclusive; hence a response could be coded other than zero on as many of the 10 themes as were appropriate, although *within* a given theme a response could be coded in one subcategory only.

The Detroit marginal percentages in table 1 can be inspected in light of the theoretical expectations developed earlier in this paper. Content analysis of speeches and writings by antiwar protest leaders would also be useful as a basis for comparison. For our present purposes, however, we will make use of responses to the same closed and open questions by students in three sociology classes at the University of Michigan: an intro-

¹¹ The remaining five themes, and percentage coded as mentioning each, were: IV. Vietnam Not Important to American Interests, 28%; V. Okay to Intervene, But Handling of War Incorrect, 15%; VI. Entry into War Not Procedurally Correct, 15%; IX. The War Is Confusing, 11%; X. Problems with South Vietnamese Government or People, 6%. In general, these themes tend either to duplicate or to provide additional support for the results presented in the text. A copy of the complete set of coding instructions for all 10 themes, together with the results of check coding, can be obtained from the Detroit Area Study. Coding was carried out by professional Survey Research Center coders.

ductory class consisting almost entirely of freshmen; the same course but with a majority of sophomores; and a more advanced concentration course for juniors. The combined sample (initial $N = 278$, reduced to 236 who answered "yes" to the closed question), while obviously not an adequate representation of the university, provides a useful contrast, as we will see. In one sense it is too conservative a sample, since it seriously underrepresents juniors, seniors, and graduate students who have been most fully exposed to the prevailing university views on the war.¹² Exactness is not absolutely essential here because we use the sample mainly to bring out broad differences between the general population and at least a core of students at the university. It can safely be assumed that the students and faculty members who actually engage in antiwar protests would show more sharply the same trends as our classroom samples.

It would be misleading to proceed as though the open-ended responses and code summaries can provide a definitive, completely objective, or simple test of the moral-pragmatic distinction I have proposed. Instead we will use the data in table 1 on an exploratory basis both to evaluate important aspects of that distinction and to illuminate public thinking about the war. Let us begin with the evaluative emphasis, looking at the themes that provide an indication of the degree of moral concern about the war in the general (Detroit) population. The results for the student and Detroit samples on the three most relevant themes are summarized in table 2.¹³

¹² All the variables reported below that have been examined *within* the student sample show an accentuated difference for the more advanced classes. This is also reflected in answers to the initial "mistake" question: 85% of the freshman class regard the war as a mistake, 88% of the mainly sophomore class, and 98% of the junior class. On Theme VIII, the percentages offering moral criticisms of the United States are 22%, 38%, and 49%, respectively, for the three classes.

¹³ Student responses were coded nonzero on an average (mean) of 3.0 themes, as against an average of 2.3 for the Detroit sample. By multiplying the Detroit nonzero percentages by 1.3, they can be adjusted upward to a level that "corrects" for this difference in total nonzero responses. The correction is relevant only where the zero category is included in a comparison (table 2B below, but not 2C or 2A), and even then has only minor effects on our results. Since the correction itself involves some questionable assumptions and other problems, our tabled data are in their original form. However, "corrected data" were looked at, and χ^2 's calculated on the basis of such data, in all relevant cases below; in none did an appreciable change result, as indeed is evident from the very large size of most of the χ^2 statistics presented below. We should also note that the student responses differ in several other ways from the Detroit interviews. They were obtained at the beginning of 1972, in self-administered form in classrooms, without the context of other questions, and at the request of a faculty member (rather than a more neutral interviewer). It is doubtful that any of these differences affected the *content* of the responses seriously, though the differences may account for the greater quantity. We will see below that the main Detroit vs. student differences are replicated to a significant degree *within* the Detroit sample when younger college-educated respondents are singled out for attention. Other checks on the results will also be presented.

TABLE 2

COMPARISONS OF GENERAL (DETROIT) AND STUDENT SAMPLES ON THREE THEMES

| | Students (%) | Detroit (%) |
|--|-----------------|----------------|
| A. Theme II: Identity of People Killed or Injured: | | |
| Americans only (1, 2) | 15 | 73 |
| Both (3, 4) | 75 | 24 |
| Vietnamese only (5, 6) | 10 | 3 |
| Total | 100 | 100 |
| N | (147) | (525) |
| B. Theme VIII: U.S. Goals Morally Questionable: | | |
| Theme not mentioned (0) | 65 | 89 |
| Theme mentioned (1-4) | 35 | 11 |
| Total | 100 | 100 |
| N | (236) | (1,263) |
| C. Theme VII: Vietnam as Internal Conflict: | | |
| They cause us trouble (3, 5) | 43 | 84 |
| We cause them trouble (2, 4) | 57 | 16 |
| Total | 100 | 100 |
| N | (90) | (518) |

NOTE.—For full description of themes, see table 1. Note that A and C here are based only on those who mention a particular theme, while B compares those who mention a theme and those who do not. All three panels show relationships statistically significant at $P < .001$, using χ^2 and 3, 2, and 2 df, respectively.

COMPARISONS OF GENERAL POPULATION AND STUDENTS

Theme II, People Killed or Injured by the War, is one of the two most frequently mentioned by both samples, perhaps partly because of the wording of the original closed question, but even more likely because of the salience of the theme to any question about the war. Students show this concern to a greater degree than does the general public (62%–42%, $\chi^2 = 34.6$, 1 df, $P < .001$), but the more important differences have to do with the types of mention, as shown in table 2A. A primary focus of the college-related antiwar movement has been on the destruction wrought on Vietnam and the Vietnamese by American military technology. Even those moral critics of the war who grant some legitimacy to American political goals argue that the costs to the Vietnamese have long since exceeded any possible gain to them. From this standpoint, the American military effort is well symbolized by a U.S. officer's explanation at Ben Tre during Tet in 1968: "It became necessary to destroy the town to save it" (Oberdorfer 1971, p. 184). Our research question, then, is the extent to which concern for Vietnamese suffering shows up in the answers of those members of the general public who oppose the war. We see in table 2A that, of those Detroit respondents who oppose the war and who mention lives

lost or injured as a reason for their opposition, nearly three-quarters refer *only* to American soldiers. The students, on the other hand, are much more likely to refer to both Americans and Vietnamese and also more likely to refer to Vietnamese only. Again we must note that even the students reflect only imperfectly the emphasis of the humanitarian part of the antiwar movement. But they reflect it to a much greater degree than does the general public.

Theme VIII covers a more political type of antiwar criticism, one which centers not on the destructive nature of the war, but rather on the motivations for, and goals of, American policy in Vietnam. The category includes accusations of imperialism, support for the North Vietnamese, and more general criticisms of the war as "immoral." We would not expect anything approaching consensus on this among students, but in fact more than a third do touch on such a theme, as shown in table 2B. In the Detroit sample, however, only one out of nine persons gave a response classified anywhere under this theme. For the general public, opposition to the war seldom entails a political-moral criticism of American goals in Vietnam.

Theme VII provides a more subtle distinction between moral and pragmatic concerns. The theme as a whole deals with emphasis on the Vietnam war as an internal conflict, but there are two ways of looking at this. The one represented by categories 3 and 5 focuses on our staying out of "their troubles." "Let them fight their own wars" is the epitome of this outlook. The other point of view, categories 2 and 4, carries the assumption that either the Vietnamese do not want American involvement or such involvement only makes the war worse for Vietnam. (Category 1, "civil war," probably belongs with the second point of view, but we omit it as somewhat ambiguous.) In other words, the first perspective on the war is strictly in terms of American interests and concludes that "they cause us trouble"; the second perspective is at least partly in terms of Vietnamese interests and concludes that "we cause them trouble." Summarized under these rubrics, table 2C shows that Detroiters who mention this theme at all do so overwhelmingly in terms of "they cause us trouble." Students, however, are much more likely to place the emphasis on "we cause them trouble."

Thus on all three of these themes we find sharp differences between the general population and the student sample. If we are correct that a sample of students who actually participate in antiwar demonstrations would be still more different from the Detroit population, we begin to get some measure of the gap between the campus-based protests and the public disenchantment with the war reflected in national surveys. It is interesting that this gap itself is validated in a sense by our code, as can be seen from Theme III, category 3: "War causes polarization in U.S." This category is mentioned infrequently by the general population (4%), but 16% of the students refer to the fact that the war has created polarization within

America.¹⁴ Perhaps for much of the Detroit population polarization is not salient because protesters are perceived as deviants rather than dissenters. Students, on the other hand, personally experience the conflict between the university climate of opinion and that in their homes or hometowns.

Some modifications in the moral-pragmatic distinction are required by findings on other themes. Theme I, U.S. Not Winning the War, represents the pragmatic position in perhaps its purest form, namely, that the war is a mistake because we are not winning it, cannot win it, or have not tried to win it (see table 1).¹⁵ Approximately a third of the Detroit respondents give this response, three times more than offer a specifically moral critique (Theme VIII), it is true, but still far less than unanimity. Moreover, 29% of the students are also coded in Theme I categories, indicating that students are nearly as likely to give such a pragmatic response as are members of the general population. It is probable that this is the case generally: what are distinctive are moral types of responses, while pragmatic reasons are given by all groups who oppose the war. Only when the two positions are incompatible within a particular theme will students and the general population differ greatly. One other theme given frequently by *both* students and general public is Theme IV-4: "We gain nothing from the war" (not shown in a table). This is coded for 20% of the students and for 22% of the Detroit sample, and indicates the general lack of clarity about American aims and purposes in Vietnam. Finally, an unexpected finding occurs with Theme X-1: "Negative characteristics of South Vietnamese government" (not shown in a table). We had expected this to be mentioned with some frequency by the general population, since it is often referred to in the mass media, but in fact it is hardly mentioned at all (3%). Students are significantly more likely to focus on negative characteristics of the South Vietnam government (10%), suggesting that this complaint about our involvement tends to appeal mainly to those influenced by a general political-moral criticism of the war.

¹⁴ This category against all others (including zero) yields a χ^2 of 49.8, 1 df, $P < .001$, for students vs. Detroit. Unlike the comparisons in table 2, this comparison is strictly post factum, but its significance is so high as to make replicability likely.

¹⁵ The subthemes obviously differ greatly both in their sophistication and in their implications for action. "The war is unwinnable" includes the type of judgment finally made by those Pentagon officials and advisers who, having first participated in the escalation of the war, later sought to deescalate it (see Hoopes 1969). "We are not trying to win the war," on the other hand, is a pure hawkish response identified with the military's push for more extreme bombing and related measures. The remaining two categories suggest less a policy point of view than a matter-of-fact if weary observation. Despite these important differences, all four categories must be described as pragmatic in terms of our present frame of reference.

Two Sources of Antiwar Sentiment

ANALYSIS WITHIN THE GENERAL POPULATION: SEX, RACE, AND SES

The Detroit sample can be further broken down in several useful ways. As was mentioned earlier, public opposition to the war in surveys over the past seven years has been associated with lower education and to some extent with older age. Opposition has also been more characteristic of women than of men by a small degree, and of blacks than of whites by a substantial margin. As table 3 shows, both the Gallup national data of

TABLE 3

PERCENTAGE BELIEVING THE UNITED STATES MADE A MISTAKE IN SENDING
TROOPS TO VIETNAM, BY SEX, RACE, EDUCATION, AND AGE

| | Gallup National Results (May 1971) (N = 1,500+) | Detroit Area Study Results (April–August 1971) (N = 1,881) |
|--------------------|---|--|
| Sex: | | |
| Male | 65 | 66 |
| Female | 72 | 72 |
| Race: | | |
| White | 67 | 68 |
| Black* | 83 | 82 |
| Education: | | |
| College | 66 | 70 |
| High school | 67 | 68 |
| Grade school | 75 | 73 |
| Age: | | |
| 21–29 | 63 | 68 |
| 30–49 | 67 | 66 |
| 50 and over | 73 | 74 |
| Total | 68 | 69 |

NOTE.—Percentages calculated after removing missing data (11% reported by Gallup, 3.3% by Detroit Area Study).

* Includes other nonwhites for Gallup only.

May 1971 and our Detroit sample of summer 1971 continue to display these sex and race relationships with almost identical percentages. Gallup also shows small but clear age and education relationships, while our Detroit sample reveals less consistent associations for both these variables.¹⁶ In any case, it is useful to know how all four of these basic back-

¹⁶ When race is controlled, the Detroit sample reveals the same relationship to age for whites that Gallup reports for the nation as a whole. This is a reasonable control, since the percentage of blacks in our Detroit sample is twice that at the national level and therefore prevents an exact comparison. (From this the reader will infer correctly that the relationship of age to opposition to the war is reversed for blacks: younger blacks in the Detroit sample are more likely than older blacks to regard our intervention as a mistake.) However, with or without the control for race, we do not obtain for Detroit the usual (or any other) association between support of the war and education.

ground characteristics relate to reasons for being against the war.¹⁷ At least three, it may be noted, identify groups excluded from political dominance: blacks, women, and low-educated persons.

Let us begin with education and age, two variables that are usefully treated together, and with the focus on the white subsample where the number of cases is large enough to allow for more detailed analysis. The main findings here, as shown in table 4, fit well with those reported earlier

TABLE 4
A. PERCENTAGE MENTIONING U.S. MOTIVES MORALLY QUESTIONABLE
(THEME VIII) BY EDUCATION AND AGE

| AGE | EDUCATION | | | | |
|-------------|-----------|---------|---------|---------|---------|
| | 0-8 | 9-11 | 12 | 13-15 | 16+ |
| 21-29 | ... (5) | 4 (28) | 3 (67) | 12 (49) | 28 (36) |
| 30-49 | 0 (21) | 4 (75) | 8 (147) | 17 (76) | 24 (66) |
| 50+ | 8 (90) | 5 (100) | 12 (89) | 13 (47) | 21 (38) |

B. PERCENTAGE MENTIONING AMERICAN SOLDIERS KILLED OR INJURED
(II-1, 2) BY EDUCATION AND AGE

| AGE | EDUCATION | | | | |
|-------------|-----------|------|----|-------|-----|
| | 0-8 | 9-11 | 12 | 13-15 | 16+ |
| 21-29 | ... | 50 | 40 | 14 | 17 |
| 30-49 | 38 | 28 | 35 | 21 | 20 |
| 50+ | 40 | 33 | 26 | 25 | 26 |

NOTE.—Results are for whites only. The figures in parentheses indicate the base *N* on which each percentage is based. The same bases apply in both panels of the table. The cells based on only five cases are omitted as unstable; the frequencies are zero in A and one in B.

for students versus the general population. For example, Theme VIII, U.S. Goals Morally Questionable, produces a strong positive relationship to education, with a quarter of the college graduates voicing some moral criticisms of U.S. motives or actions, but decreasing proportions doing so at lower educational levels. Age may act as a conditional factor here, with the gap in mention of this moral theme somewhat greater between high and low educated for the young than for the old. That is, among young people 21-29 who oppose the war, those with college education are fairly

¹⁷ There are slight differences in the average number of codable (nonzero) responses given by different population subgroups. At the extreme, those with grade school education (0-8) give an average of 2.16 nonzero responses; those with some college (13+) give an average of 2.44 nonzero responses—a ratio of only 1.1. This difference is too slight to affect the results presented below. Black-white, male-female and age differences are even smaller on this response count.

likely to do so in terms morally critical of government actions, while those with less than high school education are very unlikely to do so. Complementing this, we find that on Theme II, People Killed or Injured, those young opponents of the war without college education are considerably more likely to mention American soldiers being killed or injured (and less likely to mention Vietnamese) than are those with at least some college training—perhaps reflecting the fact that there is not only a social class difference in sensitivity to moral issues but also a social class difference among young men in the risk of entering the army and being sent into combat. These data support our earlier finding of a gulf between college students and the general population in their reasons for opposing the war, and also reinforce the point sometimes made that the gulf is not only between young and old but between those young people with college education and those without it.¹⁸ We are also reminded that moral reasons for opposing the war (as well as principled support for it) may be easier to elaborate when one is not directly in the line of fire.

The less educated, therefore, and especially less educated youth, are particularly likely to interpret their opposition to the war in terms of the danger to American lives. Since older Americans tend to be low in education, this also helps explain the special opposition to the war of older people. However, age as such is associated with still another type of opposition, labeled earlier as “they cause us trouble” (Theme VII-3, 5). This

¹⁸ The methods developed by Goodman (1969, 1972) for analysis of multivariate contingency tables were applied to the results in table 4B. For full table (including the five cases omitted in percentaging), only the relationship between education and mention of American soldiers is significant ($\chi^2 = 21.2$, 4 df, $P < .001$; with age partialled out, $\chi^2 = 32.5$, 4 df, $P < .001$). Age is not significantly related to mention of American soldiers ($\chi^2 = 0.8$, 2 df), nor is there a significant three-way interaction ($\chi^2 = 11.6$, 8 df, $P > .10$). However, since the interaction expected on the basis of our earlier student versus Detroit findings was specified in terms of young college-educated persons, the problem was run again with age and education each reduced to two categories. The table below presents the observed percentages on the basis of this specification, along with, in brackets, those expected on the hypothesis of no three-way interaction. (The appropriate base N 's can be constructed from table 4.)

| AGE | EDUCATION | |
|-------------------|-----------|-------------|
| | 0-12 | 13 and Over |
| 21-29 | 42 [36] | 15 [22] |
| 30 and over | 33 [34] | 22 [20] |

The predicted interaction does occur ($\chi^2 = 4.7$, 1 df, $P < .03$ for rejection of two-variable model). The largest discrepancies between observed and expected frequencies of mention of American soldiers are located among those 21-29 years old, indicating that the young college-educated differ not only from the rest of the population generally but especially from the noncollege portion of their own cohort. I am indebted to Otis Dudley Duncan for pointing out to me the particular relevance of Goodman's approach for this problem; Davis's (1972) exposition also proved helpful.

stance should perhaps be relabeled "traditional isolationism," for it focuses on the avoidance of intervention into troubles elsewhere. The percentage giving this response is directly associated with age, but in a single-step threshold fashion:

| AGE IN DECADES | | | | | |
|----------------|-------|-------|-------|-------|-------------|
| 29-29 | 30-39 | 40-49 | 50-59 | 60-69 | 70 and Over |
| 32 | 29 | 31 | 42 | 40 | 42 |

Those over 50 are a third again as likely as younger persons to voice this sentiment, but there is little variation within either of the age categories created by the division at 50. The relationship continues to hold when education is controlled, except that the rejection of isolationism is strongest in the 30-49 age range. This is exactly the generation that came to maturity between the beginning of World War II and the beginning of serious frustration over Vietnam—that is, the generation most exposed to what might be called successful "military internationalism" on the part of the United States. Presumably it is this age group that finds especially resonant administration appeals referring to "Munich" and to the early episodes of the Cold War. In any case, whether or not this generational interpretation is correct, we see that the *older* age groups that have been disproportionately opposed to the war have often been drawn to that position on the basis of traditional isolationist sentiments. We assume that the failures of intervention in Vietnam have reinforced these sentiments, although we lack trend data to demonstrate such reinforcement.¹⁹

The persistence of both sex and race differences in poll data on the war over the last seven years has been interpreted as evidence that at least two groups—blacks and women—have special reasons for their opposition. It is easy to hypothesize that these reasons fit under the several categories that operationalize moral reservations or criticisms. In the case of women this could involve a less aggressive and greater humanitarian attitude. Black opposition, on the other hand, could reflect disenchantment with American society generally and therefore a greater willingness to criticize the war in moral or ideological terms. We find some support for both these expectations in our thematic data, but important qualifications are needed as well.

Considering sex differences first, table 5 shows that women are more

¹⁹ One other result involving education that is worth noting is a positive association between number of years of schooling and belief that the war is "unwinnable" (I-1). Percentages coded into the latter category are: 4% (grade school), 7% (some high school), 10% (high school graduates), 15% (some college or above). The finding supports our earlier note about the sophistication of this point of view. The association involves only education and not age.

Two Sources of Antiwar Sentiment

TABLE 5

PERCENTAGES BY SEX ON SELECTED ANTIWAR THEMES AND SUBTHEMES

| | Men (N = 535) | Women (N = 728) | Difference |
|--|------------------|--------------------|------------|
| Theme I: U.S. Not Winning War | | | |
| We are not trying to win the war (2) | 11 | 5 | 6* |
| The war is not ending (4) | 13 | 18 | -5* |
| Theme II: People Killed or Injured | 33 | 48 | -15** |
| Americans only (1 and 2) | 22 | 37 | -15 |
| Both (3 and 4) | 9 | 10 | a |
| Vietnamese only (5 and 6) | 2 | 1 | a |
| Theme VIII: U.S. Goals Morally Questionable .. | 15 | 7 | 8** |

^a Less than 2% difference.

* $P < .05$, using χ^2 for category I-2 versus remainder of Theme I (df = 1). $P < .01$ for category I-4 versus remainder of Theme I (df = 1).

** $P < .001$, using χ^2 for zero versus nonzero categories (df = 1).

likely than men to mention "People killed or injured" as a reason for opposing the war, but that this difference is entirely accounted for by the "Americans only" category. Men and women do not differ at all with regard to mention of Vietnamese deaths or suffering. Thus the greater concern of women for the pain of war seems to be channeled wholly along national lines. On other themes, our general finding is that men are more critical of the war effort in *all* ways, both moral and pragmatic. Men are more likely to complain that "we are not really trying to win the war"—a "hawk" type of response—but men are also more likely than women to question the morality of U.S. motives and actions in Vietnam.²⁰ Women are more apt to phrase their opposition in more passive ways, for example, that "the war goes on and on" (I-4). These findings help explain why the sex difference in opposition to the war has never seemed to translate well into political actions. Despite their concern for American lives lost in war, Detroit women are less rather than more critical of the policies that support and guide the war effort.

The black-white difference on the basic "mistake" question is the largest in table 3, and this finding of greater black opposition has held consistently over the course of attitude surveys on the war. Moreover, a number of student responses, self-identified as black, offer criticisms of the war in clear racial terms: that the war is racist and genocidal, that the money should be spent at home on urban problems, etc. Despite this indirect evidence that blacks may be ideologically more opposed to the war than whites, the Detroit black sample of adults gives only a little evidence

²⁰ These conclusions are *not* changed when age and education are controlled. For example, considering only the youngest and most educated respondents—those 21–29 with 13 or more years of school—26% of the 53 men question U.S. motives or actions (Theme VIII), as against only 6% of the 32 women.

TABLE 6

PERCENTAGES BY RACE ON SELECTED ANTIWAR THEMES AND SUBTHEMES

| | Black (<i>N</i> = 322) | White (<i>N</i> = 941) | Difference |
|---|----------------------------|----------------------------|------------|
| Theme I: U.S. Not Winning War (1-4) | 20 | 39 | -19* |
| Theme II: People Killed or Injured | 42 | 42 | a |
| Americans only (1 and 2) | 31 | 30 | a |
| Both (3 and 4) | 10 | 10 | a |
| Vietnamese only (5 and 6) | 1 | 2 | a |
| Theme III: Loss of U.S. Resources | 22 | 20 | a |
| Explicit mention of alternative uses (2) | 4 | 3 | a |
| Theme VII: Vietnam War Is Internal Conflict | 52 | 44 | 6 |
| They cause us trouble (3 and 5) | 42 | 32 | 10* |
| We cause them trouble (2 and 4) | 5 | 7 | a |
| Theme VIII: U.S. Goals Morally Questionable | 10 | 11 | a |
| Theme IX: The War Is Confusing (1 and 2) | 18 | 6 | 12* |

a Less than 2% difference.

* $P < .001$, using χ^2 for categories shown versus all other categories of same theme (*df* = 1 in each case).

of being more highly motivated by moral sentiments than are whites. As table 6 shows, blacks as a group do not differ from whites in their distribution of responses to Theme II, People Killed or Injured, or to Theme VIII, U.S. Goals Morally Questionable. They also do not differ significantly on a theme (III-2) which concerns alternative social uses of money and resources now being spent in Vietnam, although this is a point often made by black leaders. Blacks do differ, however, quite substantially on Theme I, U.S. Not Winning War, with 19% fewer responses here than reported by whites, the reduction being distributed evenly over categories 1, 2, and 4. In other words, blacks are much less concerned than whites about the lack of "victory" in Vietnam, and therefore in this sense black opposition to the war seems less pragmatically based than is the case for whites. But this deemphasis on pragmatic opposition does not appear to be translated into a more positive critique of the war. The 19% difference on Theme I is not compensated for elsewhere in any single category; it generally appears to be reversed in the categories we have called, "They cause us trouble" (Theme VII-3, 5), as well as on other categories indicating confusion over what the war is about (especially Theme IX). Together these findings suggest a picture of the war as a distant and unclear set of troubles belonging to someone else—an isolationist trend of thinking based on low interest in the war, rather than on conscious moral opposition to it.²¹

²¹ Detailed internal analysis of the black sample is difficult because of the small number of cases. However, certain findings can be noted. On the initial "mistake" question,

DISCUSSION

When the evidence from both past opinion surveys on Vietnam and the present Detroit study is drawn together, the broad distinction between

blacks at *all* age and educational levels are more apt than comparable whites to consider intervention to have been a mistake; however, the differences are greatest among persons in their twenties regardless of education. This may suggest that younger blacks have a distinctive set of reasons for opposing the war, but in fact nothing of this sort emerges. On the contrary, we do find that older and less educated blacks report more "confusion" about the war (Theme IX) than do comparable whites, but there are no uniquely emphasized reasons given by younger blacks in our sample. In sum, we are able to "explain" the greater opposition to the war among both older and less educated blacks, but there seem not to be equally salient patterns among younger, more educated blacks that distinguish them from comparable whites. The clearest trend is for young college-educated blacks to mention the waste of money in Vietnam (III-1), but this is a very small subsample ($N = 19$) and the import of the response is not clear; Theme III-2, dealing with alternative uses for the money at home, is *not* emphasized. When sex is controlled, differences emerge for blacks similar to those for whites. Unfortunately we are not able to control age, education, and sex simultaneously within a cross-tabular framework allowing a search for interaction. It is also possible that black responses are being obscured by race-of-interviewer effects (Hyman 1954; Schuman and Converse 1971). Our sample design included a systematic variation by race of interviewer, and we do find that black respondents are significantly ($P < .05$) more likely to question U.S. goals (Theme VIII) when being interviewed by blacks (13%) than by whites (6%). The association is not strong, however, and even if we substitute the higher percentage (13%) into table 6, the difference between blacks and whites on this theme remains trivial. No other themes show significant differences by race of interviewer; there are some trends that are of theoretical interest (the largest involving greater use to black interviewers of Theme IV-4: "We gain nothing from the war"), but again they are too slight to change the basic picture presented in table 6. Moreover, a special recoding of a subsample of 110 black responses to uncover subtle racial references possibly unnoticed in our thematic analysis produced only one such reference. One final piece of information that is suggestive in interpreting the racial differences comes from an additional coding of the open Vietnam responses. When respondents referred to the U.S. government indirectly by use of a pronoun, we coded whether the pronoun chosen was "we" or "they." Blacks are considerably less likely to use "we" and more likely to use "they" than are whites:

| | Blacks | Whites |
|--------------|--------|--------|
| "We" | 59% | 85% |
| "They" | 41% | 15% |
| | 100% | 100% |
| | (219) | (709) |

NOTE.— $\chi^2 = 67.9$, $df = 1$, $P < .001$.

This racial difference holds up well when age and education are simultaneously controlled (mean racial difference over nine categories: 22%). However, it is greatest among young, well educated blacks; indeed, use of "we" generally increases with increased education among both blacks and whites, *except* among blacks 21-29, where the direction is reversed. The exact meaning of these findings is not entirely clear, but they probably signify a sense of distance from the exercise of governmental power on the part of blacks, though how much of this is passive and unconscious, as against actively alienated, remains to be determined.

moral and pragmatic types of opposition to the war remains a persuasive one. The college-based protest has been led almost entirely by spokesmen presenting the moral critique, but much of the public opposition to the war flows from quite different sources. These have to do primarily with the long and frustrating nature of the war but also draw on other closely related themes, of which the two most important are probably the costs in American lives and the lack of clarity about the goals of the war. A very pragmatic current of isolationism is also involved, symbolized by the phrase: "Let them fight their own war."²²

The moral-pragmatic distinction does not, of course, correspond exactly to the difference between major universities and the rest of the population. One will find both types of opposition in both settings—though, as we have shown, their proportions differ sharply. Nor is it necessary or wise to assume that the distinction represents characterological differences between the campus and the city. We are dealing here with ideology, not with personality. While it is probably true that some of the leaders and participants in the college-based protest movement are motivated by deeply held ethical principles, it would obviously be a mistake to infer individual character directly from verbal reasons for opposition to the war. College students provide moral criticisms primarily because they are exposed to, and learn, such criticisms on campus. In addition, they are intellectually equipped to elaborate their sense of dissatisfaction with the war, and to turn personal concern about participating in it into a critical examination of its goals—what Weber meant by rationalization, which is far more than merely "explaining away" something distasteful. Our aim here has been an analysis of the content and social bases of antiwar sentiments and ideology, not an attempt at delineation of personality differences.

From a policy standpoint, the main overall implication of our argument is that the president has never had much to fear directly from the college antiwar movement, because the latter does not speak the same language

²² One must expect emphases to change somewhat over time as the impact of the war itself changes. Several months after our main field interviewing, we conducted brief telephone reinterviews with a random subsample of 198 respondents. Nine of the 10 themes showed a drop in mention, perhaps merely a function of the telephone context, but one showed a rise. This was Theme VII: Vietnam as an Internal Conflict, and especially the subthemes (3 and 5) we have labeled "they cause us trouble" or traditional isolationism, which increased from 32% to 46% for the relevant reinterview subsample ($N = 119$). The largest single drop in mention (from 42% to 27%) occurred for Theme II: People Killed or Injured. Since the salience of the war itself was decreasing at that point (for example, U.S. casualties had declined to a relatively tiny number), it makes sense that such specific objections were disappearing and being replaced by a hardening of general isolationist sentiment toward a more and more remote "nuisance." These findings reinforce the value of the broad abstractions "moral" and "pragmatic"; the distinction carries the danger of oversimplification, but it also points to more enduring stances toward the war than do most of the specific themes and subthemes.

as the general public. Public disillusionment with the war has grown despite the campus demonstrations, not because of them. The president's primary enemy is and always has been the Viet Cong and the North Vietnamese, for it is their resilience and success that undermine larger public support for the war. The antiwar movement is not wholly ineffective: it influences commentators and columnists, who in turn (but in different words) influence the public. And it provides energy and money in political campaigns. But attempts by moral spokesmen against the war to proselytize the general public directly are likely to fail or even prove counterproductive unless carried out with more skill and less righteousness.

There is another long-term implication to the moral-pragmatic distinction. Our Detroit questionnaire included a question on a possible future Communist-inspired revolution in South America (preceding the Vietnam question by several items). As might be expected, those who regard the present Vietnam war as a mistake are more likely to resist intervening in such a future situation. But our thematic code proves useful in distinguishing further. Of those who are opposed to the Vietnam intervention simply because we are not winning (Theme I), 50% would still intervene in a new South American war. But of those who criticize the Vietnam war on moral grounds (Theme VIII), only 25% would intervene in a new war. The *reasons* people give for thinking the Vietnam war a mistake are linked to their willingness to become involved in future wars of the same general type. We need not pretend to be entirely clear on cause and effect here, but we can insist on the value of understanding more thoroughly not only pro and con positions, but the reasons for them.

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Alienation and Action: A Study of Peace-Group Members¹

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The relation of social-psychological orientations to overt action is analyzed by examining the pattern of association of a number of scales to membership, degree of activity, and degree of radicalism in peace groups. Four types of alienation are found to be significantly related to social-action patterns, but in different directions. Mode of explaining social events is found to be a crucial underlying orientation. But analysis of recruitment patterns indicates interpersonal and status factors also determine peace-group membership. Biographical data suggest that orientations predisposing to peace-group membership take shape in youth and early adulthood instead of being internalized in childhood.

A fundamental issue in social psychology is the relation of social-psychological orientations to overt social acts. Too often, tests of reliability are tacitly taken as evidence for the predictability of action from personal orientations. However, studies such as those on prejudice and discrimination raise considerable doubt about the direct relation between social-psychological orientations and action (Brookover and Holland 1952; Dean 1958; Linn 1965). Deutscher not only cites empirical evidence but raises a range of theoretical questions about the expectation of a direct relation between expressed attitudes and action (Deutscher 1966). The most recent review of the literature, covering a number of areas, concludes that the review "provides little evidence to support the postulated existence of stable, underlying attitudes within the individual which influence both his verbal expressions and his actions" (Wicker 1969). Situational factors, of both structural and episodic origin, play an important role in shaping action.

But the healthy corrective to the simplistic assumption of the direct predictability of action from attitudes may be carried too far. Sociologists have long insisted that people act in terms of their definition of the situation. While writers such as Goffman show that definitions and norms are often constructed as specific to the immediate episode of interaction (Goff-

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man 1963), Mead's conception of the "I" and the "me" as a miniature system of social interaction implies an autonomy of the self from immediate situational determinants that suggests that the part played in the defining process by more or less stable personality orientations may be considerable (Mead 1934). Moreover, people are not thrust into situations at random. A person selects and is selected for situations in part on the basis of his presumed social-psychological orientations. Continuity in orientation may thereby be reinforced in a way that masks the relative influence of personality and situational factors.

Because of the relative fluidity of social movements, study of recruitment and personal orientations of participants affords an opportunity to sort out some of the influences operating to bring about various kinds of action. In the study reported here, data were collected on the recruitment and on a range of social and attitudinal characteristics of peace-movement participants. Comparable data were also obtained on a sample of non-participants. The form of sampling of peace-movement participants also permits examination of intensity of activity and degree of radicalness of activity among peace-group members. Hence, attitudinal data can be related to several dimensions of action. I shall first examine a body of relationships between attitudinal data and peace-movement action and then consider the manner in which situational and structural factors enter into recruitment into peace groups.

PROCEDURE

The samples (with one exception, to be discussed later) were drawn from a suburban university community of about 11,000 during one of the peak periods of peace activity, late summer and fall of 1962. Three organizations in the community were considered peace groups but varied distinctly in degree of radicalness of ideology and action. One was a chapter of the American Association for the United Nations (AAUN), which represented a type of peace activity closest to conventional attitudes and practices.² The AAUN supported an international organization of which the United States is a member and, though espousing disarmament and world law, did not strongly criticize American policy or participate in the protests and demonstrations of more radical peace groups. The local chapter considered its function educational and limited its activities to sponsoring rather bland speakers and recruiting members for the national organization.

The second peace group was a chapter of the United World Federalists

² The AAUN has subsequently merged with the U.S. Committee for the United Nations, a quasi-governmental organization, and has adopted the name the United Nations Association. However, this merger came well after the present study, and I will use the AAUN name.

(UWF), which focuses on world government but supports most of the principles of the peace movement short of pacifism. While national UWF has had more support from Republicans and businessmen than have other peace groups, the local members and leadership were overwhelmingly Democrats and professionals. Though nationally only slightly more radical than the AAUN, the local UWF group was considerably more so—probably partly because the UWF was for some years the only peace group in the community and hence drew members from the whole spectrum of peace-movement-oriented people. The local group was often vocally critical of U.S. policy and brought in speakers representing a fairly radical criticism. Though operating as a conventional interest group, the UWF chapter employed much more vigorous action tactics than the AAUN—organized telegram and letter writing campaigns, conducted workshops on protest tactics, wrote letters to the editor, etc.

The third group was an unaffiliated group, which will be referred to as the Community Peace Group (CPG). Largest of the three groups, CPG represented a considerably more radical position than the other two. Though leadership was largely nonpacifist, most pacifists in the community belonged to the CPG. The group had the widest range of activities: a newsletter, speakers' bureau, public protest meetings, newspaper ads and petitions, and support of a political lobbyist. Sponsored speakers were often identified with the more radical criticism of American policy. Members often engaged in demonstrations and vigils. Despite considerable membership overlap between UWF and CPG, the former rejected joint activities, implying that the radical image of the CPG would be detrimental to the effectiveness of the UWF.

Thus, the three groups could clearly be arranged by degree of radicalness of ideology and action, from the AAUN, to the UWF, to the CPG. There is no quantitative measure of degree of radicalness, but at least the rank order is very clear. However, because the three community peace groups all represented the relatively moderate portion of the peace movement, a small sample (30) of radical pacifists was obtained from outside the community. In the absence of a definable universe, no systematic sampling procedure was used. However, all the radical pacifists were persons known to have engaged in such tactics as civil disobedience, protest boardings of Polaris subs, lengthy peace marches, vigils, and sailing protest ships into nuclear testing zones. Most subjects were members of one or both of the two then most radical peace groups: the War Resisters League (WRL) and the Committee for Nonviolent Action (CNVA). About half were living on a CNVA training farm developing nonviolent resistance tactics. These activities indicate a radical ideology challenging a major part of American life and/or foreign policy. These radical pacifists had, for the most part, given up on conventional tactics or institutional channels

for attaining their goals. Most had been subject to jail sentences or police harassment. More than half of the radical pacifist sample were full-time peace workers, and all were devoting a higher proportion of time to peace work than almost any member of the three community groups. This sample thus enables us to carry the analysis to a more radical extreme than was afforded by the peace groups in the subject community.

Within the community, two samples were drawn. One consisted of all known members of the three peace groups. Although membership of the three groups totaled 131, because of overlapping membership the sample contained only 95 persons. Only one member refused to complete the questionnaire (she had just disaffiliated). The second sample was a random sample of nonmembers. Approximately one household in every 10 was sampled, with male and female spouse taken alternately (students were eliminated from the sampling procedure). This sample produced 234 usable questionnaires, an 83% return. Areal analysis and known characteristics of those who refused or could not be contacted indicate that elderly people, persons of low income, and, probably, conservatives are somewhat under represented. However, as our purpose is not a characterization of the general population but a comparison with the peace-group sample, these deficiencies are not a disadvantage, since peace-group members were overwhelmingly middle-aged, college educated, and politically liberal.

FINDINGS

We should first examine the sample relations on two scales specific to peace activity: political attitudes³ and attitudes about foreign policy.⁴ Initially, I

³ The political attitudes scale contained seven items, four of which are given here. Each could be answered on a five-point scale from "strongly agree" to "strongly disagree":

2. The freedom riders have worsened the lot of the Negroes in the South.
3. The drinking water in (university town) should be fluoridated.
4. The U.S. government should provide completely for the medical needs of every citizen over age 65.
6. Atheists should be prohibited from teaching in our public schools.

Gammas were calculated for the relation between the scores on each item and the total scale scores. The γ 's for the above items were: (2) .84, (3) .65, (4) .63, and (6) .85.

⁴ The foreign-policy attitudes scale contained nine items, four of which are given here. These items could be answered on a five-point scale from "strongly agree" to "strongly disagree":

1. Communist China should be admitted to the United Nations.
5. The U.S. government should permit the sale of our surplus grain (at world market prices) to Communist China.
6. The United States should undertake nuclear disarmament unilaterally, regardless of what Russia does.
7. The United States should commit itself to developing the United Nations in

will limit attention to the sample of peace-group members and nonmembers drawn from the same community. The comparisons are given in tables 1 and 2. Although the peace-group sample was markedly more liberal in political attitudes and had a markedly more internationalist-prodisarmament orientation in foreign-policy attitudes than the nonmember sample ($\chi^2 < .001$ for both scales), there was considerable overlap between the two samples. On the political attitudes scale (with step 9 being the most liberal), 20% of the peace-group sample were in the sixth or a more conservative step, while 33% of the nonmember sample were in the sixth or a more liberal step, 29% of the nonmembers being in steps 6 or 7, which also contained 39% of the peace-group members. On the foreign-policy attitudes scale (step 9 being most internationalist-prodisarmament), 28% of peace-group members were in step 6 or lower, while 22% of nonmembers were in step 6 or higher, with 20% being in steps 6 and 7, which contained 39% of the peace-group members. Thus, it appears that political and foreign-policy attitudes alone are not sufficient to account for peace-group membership, though their influence, particularly when taken in combination, is very considerable. But we are justified in looking to further influences.

Alienation

Alienation, especially in Marxist theory, has often been held to predispose people toward radical movements. If true, we should expect to find degree of alienation positively associated with degree of radicalness of groups to which people belong, a proposition the present research design can test, since we have five samples running from nonmembers through four increasingly radical peace groups. However, the empirical literature is sparse and, if anything, in conflict with the Marxian hypothesis. Rotter (1963) found that Negro students in a southern Negro college who saw themselves as the determiners of their own fate were more likely than those who saw themselves externally controlled to sign statements committing themselves to more personal and decisive forms of civil-rights action. McDill and Ridley (1962) found that anomic and politically alienated low-status people were less likely to vote in a metropolitan election and, if voting, were less likely than unalienated voters to favor metropolitan government. Horton and Thompson (1962) similarly found that the politically alienated were more likely to vote against local issues. Rose (1962) found that group

the direction of taking over more and more national sovereignty up to the point where the United Nations has sufficient power to eliminate war. The γ 's for the relation between the scores on these items and the total scale scores were: (1) .90, (5) .94, (6) .57, and (7) .63.

TABLE 1
DISTRIBUTION OF SCORES ON POLITICAL ATTITUDES SCALE^a

| SAMPLE | PERCENTAGES IN STEPS (NINE-STEP SCALE) | | | N | PERCENTAGE IN MEDIAN CATEGORY AND BELOW | COMPARISON WITH PEACE-GROUP SAMPLE | |
|-----------------------------|--|-------------------|------------------|-----|--|---------------------------------------|-----------------|
| | 1-4 (Conservative) | 5-7 (Moderate) | 8-9 (Liberal) | | | Q ^b | χ ^{2c} |
| 1. Three peace groups | 2.2 | 45.2 | 52.7 | 93 | 8.7 | ... | ... |
| 2. Nonmembers | 23.5 | 73.1 | 3.6 | 222 | 67.2 | .91 | * |
| 3. Radical pacifists | 0.0 | 50.0 | 50.0 | 30 | 0.0 | ... | ... |

^a The items in this scale and in each of the subsequent social-psychological orientation scales devised for this study were subject to the criteria in the Murphy-Likert technique for scaling. All of the scales are Likert-type scales, with the scores on the individual items added together to produce the score for the scale. The resulting scores were then divided into equal-interval steps.

^b Yule's Q, calculated on proportions of samples above and below the median category.

^c χ² computed on basis of 50/50 assumption, using proportions of samples above and below median category; *df* = 1.

* *p* < .001.

TABLE 2
DISTRIBUTION OF SCORES ON FOREIGN-POLICY ATTITUDES SCALE

| SAMPLE | PERCENTAGES IN STEPS (NINE-STEP SCALE) | | | N | PERCENTAGE IN MEDIAN CATEGORY AND BELOW | COMPARISON WITH PEACE-GROUP SAMPLE | |
|-----------------------------|--|-------------------|--------------------------------|-----|--|---------------------------------------|-----------------|
| | 1-4 (Nationalist) | 5-7 (Moderate) | 8-9 (Inter- nationalist) | | | Q ^a | χ ^{2b} |
| 1. Three peace groups | 3.3 | 48.4 | 48.4 | 91 | 13.2 | ... | ... |
| 2. Nonmembers | 42.3 | 55.3 | 2.4 | 215 | 78.1 | .92 | * |
| 3. Radical pacifists | 0.0 | 20.0 | 80.0 | 30 | 0.0 | ... | ... |

^a Yule's Q, calculated on proportions of samples above and below the median category.

^b χ² computed on basis of 50/50 assumption, using proportions of samples above and below median category; *df* = 1.

* *p* < .001.

leaders are less likely to be alienated than the general population—but only a fraction of the general population were significantly alienated. Seeman and Evans (1962) found that hospital patients low in alienation have more objective information than alienated patients about the hospital and treatment procedures. Seeman (1963) also showed that alienated prisoners learn less about parole information in reformatories. However, a number of conceptualizations of alienation are found in the literature, with little assurance that the various proposed indexes tap any common vein of alienation (Bell 1966; Nisbet 1966). Seeman (1959) sought to order the many variants of alienation under five types: powerlessness, meaninglessness, normlessness, isolation, and self-estrangement. Since the present study included indexes of these five types, the data provide a test of whether these five conceptions are simply variants of a general psychological orientation or are dimensions that may vary independently. Seeman's definitions and the scales used are as follows.

Powerlessness.—Powerlessness, according to Seeman (1959, p. 784), is “the expectancy or probability held by the individual that his own behavior cannot determine the occurrence of the outcomes, or reinforcements, he seeks.” The scale used in the present study involves two aspects that bear on man's power to influence outcomes. The first concerned whether human beings can collectively control their fate or are subject to some natural or supernatural destiny they can little influence. The other dimension deals with powerlessness in the face of controls exercised by elites. The eight questions (to be answered on a five-point scale from “strongly agree” to “strongly disagree”) used in the powerlessness scale were:⁵

1. Human nature being what it is, there will always be wars.
2. A person should try to lead his own personal life in as decent a way as he can, but there isn't much one can do about the big problems of the world.
3. There's an order of things in life, and there's not much we can do about our fate.
4. Science has its place, but there are many important things that can never possibly be understood by the human mind.
5. History shows that fundamentally people have always behaved about the same way, and there's not much you can do to change them.
6. The important matters are decided by a small group of powerful people, and there's not much of anything the average person can do about it.
7. It's sometimes worthwhile to try to influence things in your local

⁵ The γ 's for the relation between each item and the total scale scores were: (1) .78, (2) .79, (3) .84, (4) .67, (5) .85, (6) .60, (7) .77, and (8) .76.

community, but it's a waste of time to try to influence events or policy on the national scene.

8. The people who make the decisions which really matter in our world are beyond the influence of the common man.

Meaninglessness.—Meaninglessness, according to Seeman (1959, p. 786), occurs "when an individual is unclear as to what he ought to believe—when the individual's minimal standards for clarity in decision-making are not met." Meaninglessness, then, is the condition where the individual cannot predict with confidence the consequences of acting on a given belief (Seeman 1959, p. 786). The scale used as an index of meaninglessness is Srole's anomia scale (Srole 1956). Srole uses a 1-0 scoring technique, with only the extreme answer on a five-point range being scored as anomic. Where only extreme responses (strongly agree) are scored as anomic, Srole's scale is, as Meier and Bell (1959) say, a measure of "personal demoralization, utter hopelessness, and discouragement." But responses of "uncertain" or even "disagree" (as distinct from "strongly disagree") are more suggestive of uncertainty about predictions of outcomes on items from Srole's scale like: "Nowadays a person has to live pretty much for today and let tomorrow take care of itself," and "These days a person doesn't really know whom he can count on." Thus, in the present study a 1-2-3-4-5 scoring technique was used for the five possible responses. Scored thus, Srole's anomia scale seems a good measure of the degree to which respondents perceive the social environment as unpredictable.⁶

Normlessness.—"The anomic situation, from the individual point of view, may be defined as one in which there is a high expectancy that socially unapproved behaviors are required to achieve given goals" (Seeman 1959, p. 788). Because of the political focus of the study, the scale indexing normlessness was a measure of alienation from the political-economic structure of American society. The six items in the normlessness scale were:⁷

1. It is not so much "what you are" as "whom you know" that determines who gets ahead.
2. America is a land of unlimited opportunity, and people get pretty much what's coming to them in this country.
3. The American system of life is quite sound; the danger lies in extremists of both the Left and the Right.
4. Unfortunately, most people with whom I have discussed important

⁶ Though Srole's scale had been already standardized on other populations, it is of interest that the items scaled well on the population used in the present study, with γ 's for the relation between individual item and total test score all being between .85 and .98.

⁷ The γ 's for the relation between individual items and the total test scores were: (1) .65, (2) .64, (3) .68, (4) .47, (5) .74, and (6) .67.

social and political questions just don't understand what's going on.

5. The truth of the matter is that it is big business which wants to continue the cold war.
6. There isn't much hope for reducing the crime rate in the United States as long as we have so much materialism and a competitive economic system.

Isolation.—"The alienated in the isolation sense are those who, like the intellectual, assign low reward value to goals or beliefs that are typically highly valued in a given society" (Seeman 1959, pp. 788-89). Whereas powerlessness and normlessness have reference to the *means* by which goals may be achieved, isolation has reference to the degree of acceptance of the socially approved values or goals in the society. Nettler's (1957) alienation scale was used as a measure of isolation. As Seeman notes, Nettler's scale reflects the individual's degree of commitment to popular culture, including the general values of American society (Seeman 1959, p. 789).⁸

Self-estrangement.—Though expressing dissatisfaction with his conceptualization, Seeman suggests that self-estrangement refers to the individual's feeling of alienation from some explicit or implicit human ideal condition, reflected in the individual's inability to find his activities self-rewarding (Seeman 1959, p. 790). The index for self-estrangement in the present study was an item which asked, "Do you feel that you have fallen short—or might fall short—of your hopes and ideals in any of the following . . . ?" There followed a list of activities, such as occupation, marriage, being the kind of human being one wanted to be, enjoying life as much as one hoped, etc. A composite score of approximation of ideals was thus obtained. However, none of the comparisons, even when controlled for various factors, produced significant differences among the various samples. Both because of the absence of differences and because Seeman's conceptualization is not clear, I shall not consider self-estrangement in the remainder of the discussion.

I will first compare the total sample of members of the three community peace groups with the sample of nonmembers. However, because of the great difference between these two samples in political and foreign-policy attitudes, and in order to control for education, social status, and tendency to group affiliation (peace-group members being relatively high in all three), subsamples of nonmembers were also compared with peace-group members,

⁸ The γ 's for the relation between individual items and the total test score for Nettler's alienation scale had the greatest variability of any of the scales used on the population of the present study, running from .26 to .90, with four of the 17 items having γ 's below .50. However, since the scale is widely accepted in the literature and consistently discriminated among the samples in the present study, it was retained in its original form.

using subsamples of persons whose characteristics overlapped those of the bulk of peace-group members. It was not possible to generate a subsample sufficiently large for statistical analysis when controlling all five factors or by using a matching procedure. Hence, four subsamples are reported in the tables that follow. Subsample I controls for political attitudes, containing nonmembers who scored in the most liberal four positions on the nine-step scale, in which 91% of the peace-group members also fell. Subsample II controls for foreign-policy attitudes, containing nonmembers who scored in the most internationalist four positions on the nine-step scale, in which 87% of the peace-group members also fell. Subsample III controls for education (at least some college), political attitudes (steps 6-9), and foreign-policy attitudes (steps 5-9)—80% of the peace-group members falling into the range of all three characteristics controlled for in this nonmember subsample. These three subsamples have the deficiency of having a considerably higher sex ratio than the peace-group sample. Subsample IV controls for social status (by an index using education, occupation, and income), number of group memberships in the community, and foreign-policy attitudes—containing nonmembers who were in the upper three of a five-level social-status scale, who belonged to two or more voluntary organizations, and who were in the 5-9 range on the foreign-policy attitudes scale. Seventy-seven percent of the peace-group sample also met all these criteria. This fourth subsample nearly perfectly matches the peace-group sample in sex ratio, .71 compared with .70. This subsample has the deficiency that its members were more conservative on the political attitudes scale than peace-group members.

Table 3 presents comparisons for the four kinds of alienation. In all four types of alienation, the peace-group and nonmember samples differ significantly, with χ^2 s beyond the .001 level. While controlled subsamples somewhat reduce the differences, all differences are significant at the .05 level, and 14 of the 16 comparisons of peace-group sample and subsamples of nonmembers are significant at the .01 level or greater. But a crucial point for the analysis of alienation is that peace-group members and nonmembers do not have the same relative positions with regard to the four kinds of alienation. Relative to the nonmember samples, peace-group members are high in normlessness and isolation but low in meaninglessness and very low in feelings of powerlessness. Most striking is the fact that peace-group members are high in normlessness—that is, alienation from the economic-political structure—but very low in feelings of powerlessness to influence that structure. Indeed, efforts of these people to influence political decisions go beyond simply peace-group membership. A measure of political activity covering a wide range of political action (mostly conventional party activity) showed peace-group members far more active than any nonmember sample or subsample.

TABLE 3

DISTRIBUTION OF SCORES ON FOUR ALIENATION SCALES

| ALIENATION SCALE | PERCENTAGE IN STEPS | | | N | COMPARISON WITH PEACE-GROUP SAMPLE | | χ^2 COMPARISONS OF PEACE-GROUP SAMPLE AND SUBSAMPLES ^c | | | |
|--|---------------------|-----------------|----------------|-----|---------------------------------------|-------------|---|-----|-----|-----|
| | 1-4 (Low) | 5-6 (Medium) | 7-10 (High) | | Q^a | χ^{2b} | I | II | III | IV |
| Powerlessness scale: | | | | | | | | | | |
| Three peace groups | 61.1 | 30.5 | 8.5 | 95 | ... | ... | ... | ... | ... | ... |
| Total nonmembers | 18.1 | 40.0 | 42.1 | 233 | .78 | *** | *** | *** | ** | ** |
| Meaninglessness scale (Srole's anomia scale): | | | | | | | | | | |
| Three peace groups | 24.1 | 52.6 | 23.2 | 95 | ... | ... | ... | ... | ... | ... |
| Total nonmembers | 13.7 | 44.4 | 41.8 | 234 | .47 | *** | *** | *** | ** | ** |
| Isolation scale (Nettler's alienation scale): | | | | | | | | | | |
| Three peace groups | 25.4 | 36.9 | 37.8 | 95 | ... | ... | ... | ... | ... | ... |
| Total nonmembers | 59.2 | 33.6 | 7.3 | 232 | .62 | *** | ** | * | * | *** |
| Normlessness scale: | | | | | | | | | | |
| Three peace groups | 23.1 | 43.2 | 33.7 | 95 | ... | ... | ... | ... | ... | ... |
| Total nonmembers | 37.8 | 45.1 | 17.2 | 233 | .46 | *** | *** | *** | ** | *** |

^a Yule's Q , calculated on proportions of samples above and below median category.^b χ^2 computed on basis of 30/70 assumption, using proportions of samples above and below median category; $df = 1$.^c All differences are in the same direction as in the comparison between peace-group members and the total nonmember sample. Number of asterisks designates same level of significance as in footnote b. Subsample I controlled for political attitudes. Subsample II controlled for foreign-policy attitudes. Subsample III controlled for education, political attitudes, and foreign-policy attitudes. Subsample IV controlled for social status, group membership, and foreign-policy attitudes.** $P < .05$.*** $P < .001$.

The data depicting the types of alienation in which nonmembers show greater alienation than the members of peace groups (powerlessness and meaninglessness) reveal that the most marked difference is in powerlessness. Even in the subsamples controlling for political and/or foreign-policy attitudes and for education or social status, 62%–70% of nonmembers scored in the medium or high portions of the powerlessness scale, contrasted with less than 40% of the peace-group members, with the great bulk of that difference being found in the high-powerlessness column. There thus appear to be a considerable number of people of comparable social status in essential agreement with peace-group members on political and international questions but who feel powerless to influence political affairs and do not become involved in organizations such as peace groups or political parties.

Why do people in the same status levels vary so much in feelings of powerlessness? It is a plausible hypothesis that there is an underlying orientation to social phenomena which plays an important role in political action versus inaction: the frame of reference for understanding causal processes in social affairs. Within American culture—and extending deep into academic culture as well—is a fundamental dichotomy in mode of explaining social phenomena. The traditional mode, supported on the one hand by the free enterprise and social Darwinist traditions and on the other hand by much of the humanities, biological sciences, instinct psychologies, and psychiatry in the intellectual world, holds that the biopsychological individual is the causal factor in social occurrences. An armory of instincts, drives, needs, greeds, and passions are assumed to be generated within the individual by his biopsychological makeup. The flow of social phenomena, from this viewpoint, may be attributed to some essentially unalterable biopsychological human nature shared by all members of the species; or the flow may be viewed as resulting from the competitive struggle of individuals differently endowed with variable biopsychological forces.

Alongside this historic theory of social causation has been a sociological mode of conception, rooted in the humanitarian movement of the 18th and 19th centuries and carried on by social-welfare movements, the social-science movement, the politics of the Left, and pragmatic philosophy. The sociological mode sees social causation as lying in the relationships and interactions between people and conceives of the individual as the calculating animal—often misguided by social myths but at least potentially capable of controlling his own destiny in cooperation with other members of his society. The sociological philosophy thus lends itself to optimism about people's ability to influence the course of social events and to rid society of its ills. Biopsychological individualism inclines the believer to pessimism about the possibility of collective intervention in the social process. Whereas the sociological philosophy holds that human—or, more accurately, social—nature can be altered by changing the patterns of social

relations that channel individual action, biopsychological individualism holds that social relations—and in turn the evils of society—are products of the makeup of individuals, and hence intervention in the course of affairs must take the tedious form of direct reformation of the individual.

Of course, these two philosophies could not have persisted side by side in Western culture for so long without interpenetration. Most people probably have an amalgam of elements from each philosophy. But it was hypothesized that most people tend toward one or the other.

To test the hypothesis about the importance of mode of explaining social phenomena, a scale was constructed consisting of six items which could be answered on a five-point scale from "strongly agree" to "strongly disagree":⁹

1. Considering the way Negroes are treated in this country, you can hardly blame them when they sometimes become aggressive or join groups like the Black Muslims.
2. If we had more cooperation and planning in our society, we could produce better human personalities.
3. Most of our social problems would be solved if we could somehow get rid of the immoral, crooked, and feebleminded people.
4. If a person really wants to straighten himself out, he can solve most of his problems by himself.
5. Individualism, the competitive struggle for survival, is the law of nature; so the free-enterprise system is the only way to motivate people to work hard enough to assure progress.
6. There is not much point in trying to solve social problems by monkeying with the social and economic system, because you just can't change human nature.

Table 4 gives the distributions of the various samples on the social explanation scale. The differences are all very large, the association between the peace group and nonmember samples being $Q = .85$, while the range of Q for the subsamples is from .52 to .85 (incidentally, the Q of .61 for the difference between the peace-group sample and the subsample controlling for political attitudes shows that the scale does not simply measure political liberalism, since both samples scored high in the liberal segment of the political attitudes scale). It appears that political and foreign-policy liberals who tend toward a biopsychological mode of social explanation have a sense of powerlessness and manifest a low level of political activity. Political and foreign-policy liberals who tend toward a sociological mode of social explanation have a sense of social power and are likely to be

⁹ The γ 's for the relation between the individual items and the total scale scores were: (1) .79, (2) .46, (3) .66, (4) .77, (5) .84, and (6) .76.

TABLE 4
DISTRIBUTION OF SCORES ON SOCIAL EXPLANATION SCALE

| SAMPLE | PERCENTAGE IN STEPS (NINE-STEP SCALE) | | | N | PERCENTAGE IN MEDIAN CATEGORY AND BELOW | COMPARISON WITH PEACE-GROUP SAMPLE | |
|--|---------------------------------------|----------------|---------------------------|-----|--|---------------------------------------|-----------------|
| | 1-2 (Sociological) | 3-5 (Mixed) | 6-9 (Biopsychological) | | | Q ^a | χ ^{2b} |
| 1. Three peace groups | 57.9 | 34.7 | 7.4 | 95 | 86.3 | ... | ... |
| 2. Nonmembers | 12.5 | 36.1 | 51.6 | 233 | 34.0 | .85 | ** |
| 3. Subsample I, controlled for political attitudes | 28.4 | 43.3 | 28.4 | 74 | 58.2 | .61 | ** |
| 4. Subsample II, controlled for foreign- policy attitudes | 19.1 | 53.2 | 27.7 | 47 | 46.8 | .69 | ** |
| 5. Subsample III, controlled for educa- tion, political attitudes, and foreign- policy attitudes | 33.3 | 48.9 | 17.7 | 45 | 54.4 | .52 | * |
| 6. Subsample IV, controlled for social status, group memberships, and for- eign-policy attitudes | 20.7 | 45.3 | 34.0 | 53 | 49.0 | .74 | ** |

^a Yule's Q, calculated on proportions of samples above and below median category.

^b χ² computed on basis of 50/50 assumption, based on proportions of samples above and below median category; *df* = 1.

* *P* < .01.

** *P* < .001.

politically active, including activity in such organizations as peace groups.¹⁰

Thus far, we have considered only membership or nonmembership in peace groups. It was noted earlier that the peace groups varied in degree of radicalism, with the AAUN group being most conventional, the UWF group next, the CPG being more radical, and the radical pacifists the most radical. If the various scales are predictive of variations in radicalism of behavior, then for any given scale the several samples should have the rank order: (1) nonmember sample, (2) AAUN members, (3) UWF members, (4) CPG members, and (5) radical pacifist sample. Table 5 summarizes the data on the seven relevant scales.

With minor exceptions, my expectations are borne out on all scales except that indexing meaningfulness. The political attitudes scale and the foreign-policy attitudes scale are the most predictive of differences between peace workers and nonmembers, but are not of much value for predicting degrees of radicalness among peace activists. On the political attitudes scale, there is only a 23% difference between the lowest and highest of the four peace-activist categories.¹¹ While AAUN members are clearly separated off by the foreign-policy attitudes scale, there are only very small differences among the other groups.

In the case of the four alienation measures, some variations from the earlier picture emerge. The more radical the form of peace activity, the more alienated are the participants in normlessness and isolation—just as we would have predicted from the earlier gross comparison. However, in normlessness and isolation, only the two most radical categories of peace workers are sharply differentiated from the nonmember sample. In fact, on normlessness, AAUN members are actually less alienated than nonmembers. But the greatest variation from earlier expectation is in the case of meaningfulness. Here, the more alienated the peace activist, the more radical his activity, but the members of all three moderate peace groups are less alienated in the meaningfulness sense than are the nonmembers, while radical pacifists are markedly more alienated in this regard than are either moderate peace-group members or nonmembers. The greater degree of meaningfulness among radical pacifists than among nonmembers may,

¹⁰ In all the samples, political liberalism and a sociological mode of explanation of social events were positively associated. Since many political conservatives are politically active, even though probably holding to a biopsychological form of explanation of social events, it is apparent that other factors also enter into political activism. However, for the total sample, the association between the social explanation scale and the political activity scale was significant at the .001 level ($\chi^2 = 26.39$).

¹¹ The rather surprising lower mean step of the radical pacifists than of the CPG probably reflects the fact that the scale offered only relatively conventional political questions and alternative answers. Some radical pacifists rejected the most liberal alternatives offered because they were too conventional. A scale extending to a really revolutionary pole would almost certainly have found the radical pacifists at an extreme position.

TABLE 5

COMPARISON OF NONMEMBER SAMPLE, MEMBERS OF THREE PEACE GROUPS, AND RADICAL PACIFISTS ON SEVEN SCALES

| SCALE | Nonmember Sample (N = 234) | SAMPLES RANKED BY DEGREE OF RADICALNESS OF ACTIVITY | | | | Ranking Errors |
|---|----------------------------------|--|------------------------------|------------------------------|----------------------------------|-------------------|
| | | AAUN ^a (N = 29) | UWF ^a (N = 21) | CPG ^a (N = 45) | Radical Pacifists (N = 30) | |
| Political attitudes scale: | | | | | | |
| a) % in most liberal third of scale .. | 13.6 | 69.0 | 66.7 | 88.9 | 90.0 | 1 |
| b) Mean step (of 9 steps) | 5.3 | 6.8 | 7.2 | 8.1 | 7.6 | 1 |
| Foreign-policy attitudes scale: | | | | | | |
| a) % in most internationalist third of scale | 4.7 | 34.5 | 81.0 | 82.2 | 93.3 | 0 |
| b) Mean step (of 9 steps) | 4.8 | 6.6 | 7.2 | 7.4 | 8.0 | 0 |
| Powerlessness scale: | | | | | | |
| a) % in most alienated 40% of scale | 67.9 | 37.9 | 19.0 | 11.1 | 3.3 | 0 |
| b) % in least alienated 40% of scale | 18.1 | 31.0 | 57.1 | 80.0 | 90.0 | 0 |
| c) Mean step (of 10 steps) | 6.1 | 4.8 | 3.7 | 3.4 | 3.1 | 0 |
| Meaninglessness scale: | | | | | | |
| a) % in most alienated 40% of scale | 41.8 | 13.8 | 19.0 | 31.1 | 63.3 | 1 |
| b) Mean step (of 10 steps) | 6.1 | 4.1 | 4.5 | 5.6 | 6.9 | 1 |
| Normlessness scale: | | | | | | |
| a) % in most alienated 44% of scale | 17.2 | 6.9 | 9.5 | 62.2 | 83.3 | 1 |
| b) Mean step (of 9 steps) | 3.4 | 2.5 | 4.1 | 5.8 | 6.8 | 1 |
| Isolation scale: | | | | | | |
| a) % in most alienated 30% of scale | 7.3 | 10.3 | 23.8 | 62.2 | 70.0 | 0 |
| b) Mean step (of 10 steps) | 4.1 | 4.5 | 5.9 | 7.5 | 7.7 | 0 |
| Social explanation scale: | | | | | | |
| a) % in sociological half of scale ... | 34.0 | 72.4 | 85.7 | 95.6 | 100.0 | 0 |
| b) % in two highest sociological cate- gories of scale | 12.5 | 31.0 | 61.9 | 73.3 | 86.7 | 0 |
| c) Mean step (of 9 steps) | 3.8 | 5.4 | 6.4 | 7.1 | 7.5 | 0 |

^a In cases where an individual belonged to two peace groups, he is classified in the group in which he was most active. Cases belonging to two groups in which they were equally active are classified in the more radical group.

of course, simply reflect the fact that the nonmember sample was drawn from a high-status community in which there is relatively little alienation. At any rate, among the four peace-activist samples, we now find that normlessness, isolation, and meaninglessness all increase with degree of radical activity, while powerlessness decreases as radicalism increases.

The social explanation scale is highly predictive of degree of radicalism, providing a perfect ranking and sharp differentiation among the five ranks. There is, then, again confirmation that this scale may index an important orientation underlying degree of radical political activity. In summary, table 5 suggests that alienation is of little importance as a predisposing factor for those who enter moderate types of peace action (AAUN and UWF, while political and foreign-policy attitudes and form of social explanation are crucial factors. But for the more radical types of peace action (CPG and radical pacifists), alienation may operate as an important predisposing factor, along with political and foreign-policy attitudes and mode of social explanation. An *absence* of feelings of powerlessness is important for both moderate and radical peace action.

Tables 6 and 7 approach the problem in yet another way: are the seven scales indexing social-psychological orientations predictive of different *degrees* of peace activity? Respondents from the three community peace groups were asked to rate their involvement in their group's activities as "active," "sporadically active," or "member but not involved in activities." No consistent variations were found between the last two categories, and they are combined for this analysis. In table 6, the radical pacifist sample is again treated as the extreme rank. These persons either worked full time in peace activities or had these activities as the core of their identity and spent very large amounts of time in these activities. No one in the three moderate groups was a full-time peace worker. It is clear that for the radical pacifists, peace activities represent not only a more radical but a much more complete involvement than for the members of the three moderate groups.

In addition to the nonmember sample, table 6 includes as its second rank the subsample of nonmembers which, for each scale, had the greatest similarity (that is, the lowest *Q*) to the peace-group member sample. The subsamples included constitute the portion of the total nonmember sample most like the peace-group sample on a range of relevant characteristics but who had not engaged in organized peace activity. Thus, the subsample column represents the zero line for degree of peace activity among respondents whose social and political characteristics presumably make them "eligible" for peace activities.

Table 7 is also concerned with the relation of the seven scales to level of activity in peace work. Here, the active and less active members of the

TABLE 6

COMPARISON OF SEVEN SCALES OF NONMEMBER SAMPLE, NONMEMBER SUBSAMPLE MOST SIMILAR TO PEACE-GROUP SAMPLE, AND MEMBERS OF PEACE-ACTIVIST SAMPLES BY DEGREE OF ACTIVITY

| SCALE | SAMPLES RANKED BY DEGREE OF ACTIVITY IN PEACE WORK | | | | | RANKING ERRORS |
|---|--|--|------------------------------------|-------------------------------------|----------------------------------|-------------------|
| | Nonmember Sample (N = 234) | Nonmember Subsample (N = 45-74) ^a | Less Active Members (N = 46) | Active Group Members (N = 49) | Radical Pacifists (N = 30) | |
| Political attitude scale: | | | | | | |
| a) % in most liberal third of scale .. | 13.6 | 41.9 | 74.4 | 83.7 | 90.0 | 0 |
| b) Mean step (of 9 steps) | 5.3 | 6.8 | 7.4 | 7.6 | 7.6 | 0 |
| Foreign-policy attitudes scale: | | | | | | |
| a) % in most internationalist third of scale | 4.7 | 21.2 | 61.0 | 77.5 | 93.3 | 0 |
| b) Mean step (of 9 steps) | 4.8 | 5.7 | 6.8 | 7.4 | 8.0 | 0 |
| Powerlessness scale: | | | | | | |
| a) % in most alienated 40% of scale | 67.9 | 42.2 | 23.9 | 18.4 | 3.3 | 0 |
| b) % in least alienated 40% of scale | 18.1 | 37.8 | 58.7 | 63.3 | 90.0 | 0 |
| c) Mean step (of 10 steps) | 6.1 | 5.3 | 3.9 | 3.9 | 3.1 | 0 |
| Meaninglessness scale: | | | | | | |
| a) % in most alienated 40% of scale | 41.8 | 31.1 | 28.3 | 18.3 | 63.3 | 1 |
| b) Mean step (of 10 steps) | 6.1 | 5.9 | 5.1 | 4.7 | 6.9 | 1 |
| Normlessness scale: | | | | | | |
| a) % in most alienated 44% of scale | 17.2 | 17.8 | 32.6 | 34.7 | 83.3 | 0 |
| b) Mean step (of 9 steps) | 3.4 | 3.3 | 4.4 | 4.4 | 6.8 | 1 |
| Isolation scale: | | | | | | |
| a) % in most alienated 30% of scale | 7.3 | 22.7 | 43.0 | 32.6 | 70.0 | 1 |
| b) Mean step (of 10 steps) | 4.1 | 4.7 | 6.4 | 6.1 | 7.7 | 1 |
| Social explanation scale: | | | | | | |
| a) % in sociological half of scale | 34.0 | 54.4 | 80.4 | 91.8 | 100.0 | 0 |
| b) % in two highest sociological cate- gories of scale | 12.5 | 33.3 | 50.0 | 65.3 | 86.7 | 0 |
| c) Mean step (of 9 steps) | 3.8 | 4.8 | 6.1 | 6.7 | 7.5 | 0 |

^a Number of cases varies because different subsamples were used in different comparisons, using criterion of taking a subsample which showed the least difference from the total peace-group sample.

TABLE 7
COMPARISON OF ACTIVE AND LESS ACTIVE MEMBERS OF THREE PEACE GROUPS ON SEVEN SCALES, BY MEAN STEP

| SCALE | AAUN | | UWF | | CPG | | ERRORS FROM PREDICTED DIRECTION | MEAN DIFFERENCE IN PREDICTED DIRECTION |
|--------------------------------|------------------------------|-------------------------|-----------------------------|-------------------------|------------------------------|-------------------------|---------------------------------|--|
| | Less Active Members (N = 12) | Active Members (N = 17) | Less Active Members (N = 6) | Active Members (N = 15) | Less Active Members (N = 28) | Active Members (N = 17) | | |
| Political attitudes scale | 6.7 | 6.9 | 6.0 | 7.7 | 8.0 | 8.2 | 0 | 0.7 |
| Foreign-policy attitudes scale | 3.6 | 6.2 | 6.3 | 7.6 | 6.6 | 7.7 | 0 | 1.7 |
| Powerlessness scale | 5.4 | 4.4 | 3.8 | 3.7 | 3.2 | 3.7 | 1 | 0.2 |
| Meaninglessness scale | 4.0 | 4.1 | 5.0 | 4.3 | 5.6 | 5.6 | 1 | 0.2 |
| Normlessness scale | 2.3 | 2.6 | 3.8 | 4.2 | 5.4 | 6.4 | 0 | 0.6 |
| Isolation scale | 4.2 | 4.6 | 4.7 | 6.4 | 7.6 | 7.3 | 1 | 0.6 |
| Social explanation scale | 5.1 | 5.7 | 5.2 | 6.9 | 6.8 | 7.5 | 0 | 1.0 |

three community peace groups are compared in terms of mean step on each scale.

The data in tables 6 and 7 give further confirmation of the hypothesis that variations on the social-psychological scales are systematically associated with behavioral differences. The political attitudes scale and the foreign-policy attitudes scale both differentiate samples perfectly for level of activity, though the political attitudes scale is not very useful for discriminating among peace workers. Among the types of alienation, powerlessness has a negative relation to degree of activity. However, rather surprisingly, a sense of power is a much weaker predictor of level of activity than of radicalism. Except for the radical pacifists, normlessness and isolation are not of value as predictors of level of activity, though they differentiate members and nonmembers more satisfactorily than for radicalism. Tables 6 and 7 offer some clarification of the role of meaninglessness. Among persons with moderate peace views, meaninglessness has no relation to level of activity, while a high degree of meaninglessness is associated with intense involvement in radical peace activities. In table 5, I found that degree of radicalness increases directly with extent of meaninglessness. Thus, it appears that a sense of meaninglessness is associated with a high level of activity only where—doubtless in combination with other factors—it inspires a high degree of radicalness.

In both tables 6 and 7, the social explanation scale differentiates sharply and consistently between levels of activity: the more the sample accepts a sociological mode of explanation, the higher the level of activity.

We may now look at the interrelations of the various measures. The radical pacifists are not markedly different from the much more moderate CPG and UWF members on political attitudes or foreign-policy attitudes. Nor are the radical pacifists greatly different from the CPG members in sense of powerlessness or mode of explaining social events. But radical pacifists are unique in being highly alienated in three senses: meaninglessness, normlessness, and isolation. Since degree of radicalness among moderate peace-group members is also directly related to degree of alienation in all three of these senses, *it seems reasonable to conclude that a high degree of meaninglessness, normlessness, and isolation, in combination with a high sense of being able to influence a social environment perceived in terms of a sociological mode of causation, predisposes persons toward radical social action.*

DISCUSSION

The basic theoretical problem of this paper is the relation of social-psychological orientations to social behavior. I have shown that a number of orientations are systematically associated with complex *patterns* of social

action." In tables 1-7, there are 192 comparisons of samples, and only 10 deviations from the initially predicted directions. Three of these deviations are connected with meaningfulness, which analysis showed is subject to interpretation making these apparent deviations nonerrors. Another of the 10 errors was between segments of the nonmember sample, leaving us with only 3% errors in the prediction of membership, radicalness, and level of activity from seven kinds of social-psychological orientations.

The analysis also shows that it is configurations of social-psychological orientations rather than single orientations which are associated with different kinds of action. However, which configuration is crucial seems to vary with the problem. For example, if our problem is simply predicting peace-group *membership*, a combination of political and foreign-policy attitudes explains the bulk of the variance: less than 5% of the nonmember sample overlaps with the more than 65% of the peace-group members who scored in the three most extreme steps on *both* nine-step scales. But if the question has to do with *radicalness* of peace activity or *level* of activity, then political attitudes or foreign-policy attitudes, or a combination of these, do not account for much of the variance. For these questions, the alienation scales and the mode of social explanation scale (the latter being the most consistent and discriminating predictor) become more crucial.

However, in relating orientations to action, a difficulty arises from the fact that an indefinite number of other orientations besides those considered here could be adduced which also discriminate significantly between kinds and levels of activity. In the present study, religious orientation, political preference, altruism, rationality, and attitudes toward communism all differentiated peace-group members and nonmembers significantly in all comparisons, including controlled subsamples (though for a large number of scales and items, no significant differences were found). While techniques such as factor analysis can cluster orientations, such techniques cannot determine whether the data-gathering procedure has included all relevant orientations.

Another difficulty in interpreting findings about the relation of orientations and action lies in the question of whether the social-psychological orientations cause or result from group involvements. In the absence of before-and-after data, no conclusive answer can be given in the present study. However, considerable relevant data were collected, and they suggest a complex answer. Interviews on recruitment were conducted with 25 active members as well as leaders of the peace groups. Interview and questionnaire data point to two conclusions: (1) recruitment into peace groups is less often the result of self-selection of the group by the recruit than of being recruited through belonging to social networks, some of whose members already belong to the peace group; and (2) the orientations predisposing individuals to peace activities are not, at least in their immediate

forms, internalized in childhood but develop in youth and early adulthood and certainly, for the bulk of peace activists, prior to membership in a peace group.

A high proportion of peace activists had a history of membership in liberal organizations or "causes," though only a small portion had previously been members of peace groups. Most recruits into the CPG and UWF were already associated with persons who belonged to or were organizing the peace group, and were recruited through these interpersonal channels. Even when recruitment was not directly interpersonal, recognition from newspaper stories of persons known in previous groups was a crucial factor in joining peace groups. Three chains of group affiliation were identifiable. One led from a cooperative parents nursery organized some years earlier, through a Unitarian Fellowship subsequently formed by many of the same people, and then into peace activity. A second chain began with an anti-capital-punishment group formed two years earlier, through a race-relations group then organized by most of the same people, and thence to the CPG. The third chain involved several groups associated with activities of the Friends, activities which involved some people who were not Quakers. In addition, many peace-group members had been associated with one another in a local Democratic Club or through a chapter of the American Civil Liberties Union. Informal social networks were also important in recruitment to the AAUN chapter, which was instigated largely by one person. Most of the very deliberate recruitment process was carried on by a small core of women already associated in the informal social network of the founder.

Selective recruitment is also reflected in the fact that the active core of each peace group was derived from distinguishable status groups in the community. The AAUN (whose members were to a considerable degree sought to provide a certain image) recruited largely from the highest-status sectors of the community and, in general, from middle-of-the-road political liberals. Membership included high-level university administrators and tenured faculty and was unique among peace groups in having a significant number of persons from the business sector. The UWF, though the most heterogeneous as the community's oldest peace group, drew most heavily from the more affluent (but subadministrative) segments of the university and from other prestige professions, such as law, medicine, and the ministry, but almost not at all from the business world. The UWF drew mainly from political progressives as contrasted with the more cautious liberals of the AAUN. The CPG recruited overwhelmingly from university-connected people; only one-sixth of its members were not affiliated with the university. However, these people were largely younger, nontenured members and, to a much higher degree than the other two groups, nonfaculty, such as research associates, librarians, or graduate students.

Thus, it is clear that recruitment is not a simple matter of predisposing social-psychological orientations but is much influenced by social relations and status. But it would be equally wrong to conclude that these orientations are of no significance. Most persons in the various status groups mentioned did not join a peace group, even when invited. The AAUN's initial recruitment list had two-thirds of its prospects from outside the academic community, but in fact, the group could get only about one-third of its members from the outside. Similarly, only a minority of such groups as the Unitarian Fellowship and the Democratic Club even became involved in a peace group. In many cases, ideological orientations were crucial in refusing membership, even in social networks of friends.¹²

The history of high participation in liberal activities and "causes" prior to peace-group membership indicates that most members had congenial social-psychological orientations before their recruitment into a peace group. (Only 20% of the moderate peace-group members were under 35 years of age, though two-thirds of the radical pacifists were under 35.) However, the evidence suggests that these orientations were not, for most members, simply internalized in childhood. Very few peace-group members had been raised in traditionally pacifist religions. Even in the radical pacifist sample, 83% had not been raised as pacifists. The evidence points to the hypothesis that the social-psychological configuration crucial to eventual peace activity came in youth and early adulthood. Most suggestive was the extent to which peace activists had changed religion since childhood. Religion was categorized as "none," "Roman Catholic," "Jewish," "fundamentalist Protestant," "middle Protestant" (e.g., Methodist, Presbyterian, Episcopalian), "liberal Protestant" (e.g., Unitarian, Quaker, Congregational), and "other." Taking mother's religion as the index of childhood religion, only 31% of the nonmember sample had changed in religious category, whereas 62% of moderate peace-group members, 67% of the "active" members of these groups, and 77% of the radical pacifists had changed. The direction of change among nonmembers was random (that is, as many in the fundamentalist direction as toward "liberal" or "none"), while virtually all changes by peace-group members and radical pacifists were toward more liberal religious preferences or no religion. All evidence indicates that such changes come most often in youth.

Another piece of evidence suggesting development of congenial orientations in youth and early adulthood is the large proportion of peace activists

¹² This interpretation was offered by core members of the peace groups who were interviewed with regard to recruitment into the groups. In addition, the author, as a member of the university faculty and of a number of community groups, had a rather close acquaintance with a number of refusers and found the ideological interpretation consistent with his own knowledge of these cases. There were enough instances of people espousing other unpopular causes but rejecting peace-group membership to justify discounting facile explanations such as fear of loss of social esteem.

who had majored in the social sciences in college. Among those who had attended college, only 14% of nonmembers but 42% of moderate peace-group members and 56% of radical pacifists had majored in a social science. These data are especially significant in conjunction with the finding of the importance of a sociological mode of explanation of social events. The chains of group affiliation—such as that from a cooperative parents nursery, to the forming of a Unitarian Fellowship (few members of which had previously been Unitarians), to peace activities—are also suggestive of young-adult socialization into the kinds of views leading to peace activity. The fellowship grew out of a desire of these young parents for intellectual and ethical discussion.

If sufficient information were available about the chain of group interactions which preceded joining a peace group, it seems likely we could show how patterns of social relationships in youth and early adulthood had generated the social-psychological orientations congenial to peace activity for persons who were ultimately to join peace groups. But if a preceding sequence of group interactions molded these orientations, we must also suppose that participation in a peace group further shapes the orientations of members. That is, if we reject the notion that adult life is simply an acting out of social-psychological orientations internalized during childhood, it is no more logical to suppose that orientations developed between, say, 18 and 30 unalterably control behavior in middle age. It is probable, at least for individuals intensely involved in a peace group, that personal orientations are sharpened or modified by the experience. The intense activities of radical pacifists are not merely an acting out of alienation but an expression of identification with their particular groups. That these groups (such as CNVA and WRL) are sectarian associations in conflict with society implies that they intensify feelings of alienation in their members. The extreme alienation of the radical pacifists is thus probably partially a reflection of selective recruitment and partially a matter of intensification of alienation by participation in radical groups. Among the three community peace groups, the most intense involvement was unquestionably among active members of the CPG. It is a reasonable hypothesis that the relatively more extreme position of CPG members on most scales reflects, in part, an accentuation of those orientations which helped lead them into peace work in the first place.

Thus, no simple answer can be given to the question of whether social-psychological orientations are cause or effect of action in groups. The individual is not an actor but an interactor. To separate out social-psychological orientations from the individual's group involvements is an artifice of our techniques. By the same token, there is no simple way to answer the question of whether the kinds of orientations I have dealt with are basic personality qualities or simply transitory aberrations. These orientations

surely transcend particular situations, but they seem subject to alteration in the course of group involvement. What is called for is that very difficult kind of longitudinal study in which changes in personal orientations are related to changes in group contexts over a number of years.

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Parents, Peers, and Delinquent Action: A Test of the Differential Association Perspective¹

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This study attempts to go beyond the well-documented relationship between delinquent associations and involvement in delinquency to a consideration of the independent consequences of delinquent peers, parents, and "delinquent" definitions for delinquent action. The data fail to support Sutherland and Cressey's argument that family life is relevant to delinquency only when "delinquent patterns" are available to copy. Using a variety of measures of availability of deviant patterns, paternal supervision and support were found to be negatively related to delinquency to approximately the same degree under almost all conditions. Moreover, delinquent peers and paternal supervision and support were both found to influence delinquency involvement regardless of definitions favorable and unfavorable to the violation of the law. The family, peers, and definitions relevant to law breaking appear to exert independent effects on delinquency which are not adequately encompassed by etiological perspectives that introduce such definitions as intervening between other important variables and delinquency.

INTRODUCTION

Most empirical tests of the differential association perspective on delinquency (Sutherland and Cressey 1966, pp. 77-100) have focused on the most basic relationship implied by the theory: association with delinquent peers is assumed to lead to differential exposure to "definitions favorable to the violation of the law" and is subsequently examined in relation to delinquent behavior.² A strong positive relationship has been well documented (Glueck and Glueck 1950, pp. 163-64; Short 1957, pp. 233-39; Voss 1964, pp. 74-85; Erickson and Empey 1965, pp. 268-82). The present study represents an attempt to go beyond the usual concern with the direct relationship between differential association and delinquency to

¹ The data utilized in this paper were collected as part of the Richmond Youth Study under the direction of Alan B. Wilson (Survey Research Center, University of California, Berkeley). I gratefully acknowledge my debt to Travis Hirschi. This paper elaborates, in part, on some aspects of his earlier analysis of the Richmond data as reported in *Causes of Delinquency* (Berkeley: University of California Press, 1969).

² Two notable exceptions are Hirschi's *Causes of Delinquency* (1969) and Hackler's study of "norms vs. others" as predictors of delinquency (1968, pp. 92-106).

an assessment of the consequences of peers, parents, and definitions for delinquency involvement and an empirical examination of the ability of the theory to encompass a certain set of "known" relationships between patterns of family life and delinquency.

DIFFERENTIAL ASSOCIATION AND THE FAMILY

An adequate theory of delinquency must be able to encompass the persistent finding that the nature of parent-child relationships is a fairly important determinant of involvement in delinquency (Healy and Bronner 1936; Glueck and Glueck 1950; Nye 1958; Gold 1963; Hirschi 1969), and the differential association perspective has been posited as just such a theory.³ Recognizing such persistent relationships, Sutherland and Cressey (1966, pp. 226-27) argue that most family variables are related to delinquency through the following two processes:

A child may be driven from the home by unpleasant experiences and situations or withdraw from it because of the absence of pleasant experiences, and thus cease to be a functioning member of an integrated group. . . . The important element is that isolation from the family is likely to increase the child's associations with delinquency behavior patterns and decrease his associations with antidelinquency behavior patterns.

The home may fail to train the child to deal with community situations in a law-abiding manner. That is, delinquency patterns may not be present in the home, but the home may be neutral with respect to delinquency of the child. . . . Again, whether such a "neutral" child becomes delinquent or not will depend upon his associations with delinquent and antidelinquent patterns outside the home.

The first of these two processes encompasses situations of isolation or alienation from the home as distinct from the failure of the home to present antidelinquent patterns. In the latter situation, the child is not necessarily driven or withdrawn from the family, but the family situation is such that he fails to develop attitudes, inhibitions, or definitions which work against the violation of the law.

In either case, whether the "unattached" child becomes delinquent depends on his associations *outside the home* (Sutherland and Cressey 1966, pp. 227-28).

³ Since the hypotheses to be tested in this analysis treat conditions of family life as independent variables and delinquency as the dependent variable, our analysis and interpretations are in terms of that assumed causal ordering. However, it should be recognized that delinquency may be causally implicated in generating certain conditions of family life as well. Similarly, when we test certain notions derived from the differential association perspective, the interpretation is in terms of the ordering suggested by such theorists. Again, it should be recognized that, even where the associations suggest that certain relationships do exist, the causal direction may be quite different from that assumed by a particular theorist.

These two processes are important because they increase the probability that a child will come into intimate contact with delinquents and will be attracted by delinquent behavior.

A child does not necessarily become delinquent because he is unhappy. Children in unhappy homes may take on delinquency patterns if there are any around for them to acquire.

While the stress is placed on contacts outside the home, Sutherland and Cressey do note that a child may learn to be delinquent in the home as well. They feel, however, that peers of the same sex are more important than parents in shaping the behavior of adolescents, whether in delinquent or antidelinquent directions. Hence, their emphasis is on contact with children of the same age and sex.⁴

In sum, while numerous theorists and researchers have supported a theoretical model locating the development of delinquent behavior in the nature of direct and indirect control by the family over the teenager, Sutherland and Cressey's arguments add a qualifying condition to the model. The lack of control by parents is argued to be associated with delinquent behavior only in situations where there are delinquent patterns around to copy. In short, the known relationships between qualities of family life and delinquency are thought to hold up only within certain contexts.

Robert Stanfield (1966, pp. 411-17) has found several patterns of "interaction" consistent with such an hypothesis. For example, using appearances in a court for one or more juvenile offenses as his measure of delinquency, he found parental discipline to be more strongly related to delinquency among low-status families than high-status families, and he interprets that finding as suggesting that "unpleasant family experiences are more likely to produce delinquency in circumstances where there is an alternative cultural pattern that is favorable to the violation of the law." However, recent research (Hirschi 1969, pp. 212-23) suggests little, if any, correlation between family status and acceptance of alternative cultural patterns favorable to the violation of the law. While such findings cast doubt on the notion that it is such "cultural" patterns which account for the conditioning effect of social status, it may be that status is related to association with delinquents and that such associations increase the probability of delinquency through group pressure rather than through socialization into competing cultural standards alone. At any rate, Stanfield's

⁴ Other authors have argued that a "Fagin" pattern of socialization is rare (Sykes and Matza 1957, p. 665); and, in support of such contentions, Hirschi (1969, pp. 94-97) has found that, irrespective of social class standing, attachments to one's parents are negatively related to delinquency. He notes that "the lower-class parent even if he is himself committing criminal acts, does not publicize this fact to his children He operates to foster obedience to a system of norms to which he himself may not conform."

study is a suggestive indirect test of the "interaction hypothesis," based on a small, homogeneous, and nonrandom sample.

This study attempts a more direct and encompassing test through an examination of the relationship between direct and indirect parental control and delinquency under conditions varying in the availability of delinquent patterns, utilizing data from a large heterogeneous random sample of adolescents.⁵ Moreover, since differential association theory has put so much emphasis on "definitions," we test some rather basic but as yet unexamined claims of the theory involving such normative phenomena. Do delinquent peers influence acceptance of unconventional cultural standards, and, if so, do delinquent peers affect delinquency only through such alternative cultural patterns? One of the major implications of Short and Strodtbeck's (1965) group-process perspective on delinquency is that delinquent peers can influence involvement in delinquency independently of definitions favorable and unfavorable to the violation of the law. While such definitions are likely to be related to delinquent behavior and to be, in part, a product of interaction with delinquent friends, it can be hypothesized that the delinquent peer group may be a source of "situationally induced motives" (Briar and Piliavan 1965, pp. 35-45) and that delinquent peers can thus provide the impetus to deviate before one has come to accept unconventional definitions and quite often in spite of commitments to conventional normative standards.

STUDY DESIGN

These relationships will be examined utilizing questionnaire data gathered as part of the Richmond Youth Study by the Survey Research Center (University of California at Berkeley) in 1965. The population consisted of the 17,500 students entering the 11 junior and senior high schools of Western Contra Costa County in the fall of 1964. While the original sample consisted of both black and nonblack, male and female adolescents in grades 7 through 12, the present analysis was carried out on the 1,588 nonblack males in the sample.⁶

⁵ One of the many findings in Hirschi's (1969, pp. 154-55) earlier multivariate analysis of this body of data was that attachment to parents is negatively related to delinquency, regardless of number of delinquent friends an adolescent has. In the present analysis, we elaborate on that finding through more extensive controls for exposure to criminogenic influences as well as "definitions" favorable to law breaking. Moreover, while Hirschi focused on the independent role of such attachments in the etiology of delinquency, the present analysis focuses more extensively on the differential association theorists' prediction of interaction effects.

⁶ The sample was drawn from the student population of Western Contra Costa County, California, which is located in the San Francisco-Oakland metropolitan area, bordered by Berkeley and the San Francisco and San Pablo Bays. The largest city in the area is Richmond, which is primarily an industrial community, with more than

The dependent variable, delinquent behavior, was measured by the respondent's answers to a series of six questions concerning offenses of varying degrees of seriousness.⁷ Only acts committed within a year previous to the administration of the questionnaire are included in the score. The respondents are categorized as admitting to no delinquent acts, one delinquent act, and two or more delinquent acts.

Supervision and emotional support by the father were measured by means of questionnaire items. Paternal supervision was assessed by asking respondents to answer "usually," "sometimes," or "never" to the following questions: (1) Does your father know where you are when you are away from home? and (2) Does your father know who you are with when you are away from home? Paternal support was measured by answers to the three questions: (1) Does your father seem to understand you? (2) Have you ever felt unwanted by your father? (3) Would your father stick by you if you really got into bad trouble?

Number of close delinquent friends, perceptions of "trouble" in the neighborhood, and official delinquency rates of the schools attended are each used as indicators of variation in the presence of delinquent patterns. Respondents were asked, "Have any of your close friends ever been picked up by the police?" and were categorized for purposes of analysis into those with zero, one or two, and three or more delinquent friends. The adolescents in the sample were also presented the statement "Young people are always getting into trouble" and were asked whether or not it applied to their neighborhood. These two items are similar to measures of differential association used in other studies (Short 1957, pp. 335-36). Utilizing police data gathered from the Richmond and San Pablo police departments and the Contra Costa County Sheriff's Office, the schools sampled were ranked in terms of the percentage of nonblack "official delinquents" and classified as "high," "medium," and "low." There was not sufficient variation in self-reported delinquency from school to school to rank them in terms of self-reported delinquency.

As DeFleur and Quinney (1966, p. 7) specify in their formalization of

60% of the employed males holding manual jobs. For more extensive details on the area, see Wilson (1966). The nonblack segment of the sample consisted primarily of Caucasians (90%), with a small proportion of Orientals and "others." For a more detailed discussion of sampling and data-gathering procedures, see Wilson, Hirschi, and Elder (1965) and Hirschi (1969).

⁷ Self-reported delinquency was measured by responses to the following questions: (1) have you ever taken little things (worth less than \$2.00) that did not belong to you? (2) have you ever taken things of some value (between \$2.00 and \$50.00) that did not belong to you? (3) have you ever taken things of large value (worth over \$50.00) that did not belong to you? (4) have you ever taken a car for a ride without the owner's permission? (5) have you ever banged up on purpose something that did not belong to you? (6) not counting fights you may have had with a brother or sister, have you ever beaten up on anyone on purpose?

the differential association theory, it is criminal motives, attitudes, and techniques which are assumed to effectively result in the overt commission of crimes. Associating with delinquents is thought to be conducive to learning definitions favorable to the violation of the law, and an excess of such definitions over antidelinquent definitions is assumed to lead to delinquency. Sykes and Matza (1957, pp. 664-70) suggest that "techniques of neutralization" are a crucial component of these definitions, in that a definition favorable to the violation of legal codes can be viewed in one sense as a justification of deviant behavior. Two of the items making up the "definitions" score are implicated in a discussion of these techniques: (1) most things that people call "delinquency" don't really hurt anyone, and (2) policemen try to give all kids an even break. The first item is concerned with the "denial of injury," while the second is assumed to be involved in the "condemnation of the condemners." Two other items went into the score: (1) it's alright to get around the law if you can get away with it, and (2) to get ahead, you have to do some things which are not right. On all but the police item, "strongly agree" or "agree" was coded zero; "undecided, 1; and "disagree" or "strongly disagree," 2. The codes were reversed for the police item. These four items were combined to form a score that measures criminal attitudes or definitions favorable to the violation of the law. No measure of the respondent's knowledge of techniques was available, limiting this study to a score measuring only the attitudinal content of the learning that leads to delinquency.

Gamma, a symmetrical measure of association with a "proportional reduction in error" interpretation appropriate to an ordinal level of measurement (Goodman and Kruskal 1954, pp. 732-64; Costner 1965, pp. 341-53) and percentage distributions are used to assess the association between variables. The differential association variables are controlled by subdivision, and the relationships of paternal supervision and support to delinquent behavior are examined within these subcategories.

FINDINGS

The zero-order relationships among paternal supervision and support, number of delinquent friends, perception of neighborhood trouble, and delinquent behavior are summarized in table 1. All of the gamma coefficients are statistically significant at the .01 level. As expected, both paternal supervision and support are negatively related to delinquency. Moreover, the data are consistent with Sutherland and Cressey's argument that the nature of a child's home life can affect the probability that he will come into intimate contact with delinquent peers, in that paternal supervision and support are negatively related to intimate associations with others who have been picked up by the police. They are similarly related

TABLE 1
ZERO-ORDER GAMMA COEFFICIENTS

| Variables | Delinquent Friends | Delinquent Definitions | Neighborhood Trouble | Delinquent Behavior |
|------------------------------|-----------------------|---------------------------|-------------------------|------------------------|
| Paternal supervision | -.37 | -.25 | -.18 | -.28 |
| Paternal support | -.28 | -.29 | -.23 | -.30 |
| Delinquent friends | ... | .43 | .26 | .60 |
| Delinquent definitions | ... | ... | .18 | .38 |
| Neighborhood trouble | ... | ... | ... | .25 |

to "delinquent" attitudes and beliefs and to perceptions of trouble in the neighborhood. However, as the strength of the relationships suggests, many well-supervised and emotionally supported adolescents have delinquent friends, engage in delinquent action, and exhibit tenuous commitments to conventional moral standards.

The three differential association variables are, in turn, related to involvement in delinquent behavior. The more delinquent associates one has, the more likely he is to be delinquent. Similarly, the less the attachment to the conventional normative system and the greater the perception of trouble in the neighborhood, the greater the tendency toward delinquent action. Not only are all three related to the overall measure of delinquency, but each was found to be associated with every act that went into the overall measure as well with all the gamma coefficients in the expected direction and significant at the .01 level. Moreover, number of delinquent friends, neighborhood trouble, and the definitions score are interrelated as well with an especially strong relationship exhibited between intimate associations with delinquents and "delinquent" attitudes and beliefs. Associating with persons assumed to embrace attitudes and beliefs favorable to the violation of the law is, as expected, positively associated with embracement of such attitudes and beliefs.

However, as predicted on the basis of group-process and situational-inducement perspectives on delinquency, association with delinquents and the definitions score are independently related to delinquency involvement (table 2). Together, these two variables differentiate adolescents in terms of delinquency fairly well. While only 22% of the adolescents with no delinquent friends and a preponderance of definitions unfavorable to the violation of the law have committed one or more delinquent acts, fully 80% of the adolescents at the other end of the continuum fall in that category. However, it is clear that the effect of the number of delinquent friends on delinquency is not solely a product of socialization into competing normative standards. The partials are somewhat smaller than the zero-order coefficient of .60 but are still quite strong relative to other rela-

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TABLE 2
SELF-REPORTED DELINQUENCY WITHIN "DEFINITIONS" SUBGROUPS,
BY DELINQUENT FRIENDS

| SELF-REPORTED DELINQUENT ACT | DEFINITIONS FAVORABLE AND UNFAVORABLE TO THE VIOLATION OF THE LAW | | | | | | | | |
|---------------------------------|---|-----|----|----------------|-----|----|------------------|-----|-----|
| | Unfavorable (%) | | | Neutral (%) | | | Favorable (%) | | |
| | 0 | 1-2 | 3+ | 0 | 1-2 | 3+ | 0 | 1-2 | 3+ |
| 0 | 78 | 64 | 38 | 69 | 49 | 34 | 60 | 37 | 20 |
| 1 | 18 | 26 | 24 | 19 | 33 | 33 | 24 | 33 | 29 |
| 2+ | 4 | 10 | 38 | 11 | 18 | 34 | 15 | 30 | 51 |
| N | 251 | 81 | 34 | 202 | 105 | 98 | 91 | 86 | 138 |
| Gamma | .47 | | | .43 | | | .49 | | |

tionships in the data. As suggested by Briar and Piliavan (1965), it may be that adolescents with delinquent friends are more likely to experience short-run, situationally induced pressures to deviate.

To assess Sutherland and Cressey's (1966) hypothesis, it is necessary to go beyond the zero-order relationships to an examination of more complex three-variable relationships. For example, tables 3 and 4 summarize the relationships between paternal supervision and support and delinquency involvement, controlling for number of delinquent friends. It turns out that, while parental supervision is negatively related to the number of close delinquent friends and number of delinquent friends is positively associated with delinquency, the relationship between supervision and delinquent behavior persists within all three categories of delinquent friends. The data fail to support Sutherland and Cressey's (1966) argument, in that the association is equally strong among those with three or

TABLE 3
SELF-REPORTED DELINQUENCY WITHIN "DELINQUENT FRIENDS" SUBGROUPS,
BY PATERNAL SUPERVISION

| SELF-REPORTED DELINQUENT ACTS | NUMBER OF DELINQUENT FRIENDS | | | | | | | | |
|----------------------------------|------------------------------|--------|------|------------|--------|------|----------|--------|------|
| | 3+ (%) | | | 1-2 (%) | | | 0 (%) | | |
| | Low | Medium | High | Low | Medium | High | Low | Medium | High |
| 0 | 26 | 28 | 30 | 39 | 50 | 59 | 60 | 78 | 74 |
| 1 | 21 | 37 | 35 | 36 | 28 | 29 | 28 | 17 | 18 |
| 2+ | 53 | 35 | 36 | 26 | 22 | 13 | 13 | 5 | 8 |
| N | 119 | 57 | 98 | 90 | 60 | 111 | 120 | 96 | 346 |
| Gamma | -.17 | | | -.26 | | | -.17 | | |

TABLE 4

SELF-REPORTED DELINQUENCY WITHIN "DELINQUENT FRIENDS" SUBGROUPS,
BY PATERNAL EMOTIONAL SUPPORT

| SELF-REPORTED DELINQUENT ACTS | NUMBER OF DELINQUENT FRIENDS | | | | | | | | | | | |
|----------------------------------|------------------------------|------|----|------|------------|------|----|------|----------|------|-----|------|
| | 3+ | | | | 1-2 | | | | 0 | | | |
| | 3+ (%) | | | | 1-2 (%) | | | | 0 (%) | | | |
| | Low | 2 | 3 | High | Low | 2 | 3 | High | Low | 2 | 3 | High |
| 0 | 21 | 20 | 33 | 43 | 44 | 52 | 49 | 56 | 61 | 68 | 71 | 80 |
| 1 | 24 | 39 | 33 | 26 | 30 | 30 | 36 | 32 | 21 | 22 | 21 | 17 |
| 2+ | 55 | 41 | 33 | 31 | 26 | 18 | 15 | 12 | 18 | 10 | 8 | 3 |
| N | 104 | 59 | 51 | 49 | 86 | 54 | 47 | 72 | 104 | 100 | 140 | 190 |
| Gamma | | -.27 | | | | -.15 | | | | -.28 | | |

more delinquent friends and among those with no delinquent friends at all. Thus, paternal supervision appears to be related to delinquency, irrespective of the availability of delinquent patterns as measured by the number of close delinquent friends. Similarly, supportive or affectionate father-son relationships are negatively related to delinquent behavior within each of the delinquent-friends subdivisions, with each variable contributing independently to delinquent behavior.

By subdividing on the basis of perceptions of trouble in the neighborhood, it was possible to test the hypothesis that "if the family is in a community in which there is no pattern of theft, the children do not steal, no matter how much neglected or how unhappy they may be at home" (Sutherland and Cressey 1966, p. 227). The data indicate that paternal supervision is negatively related to delinquency even when respondents disagree that the young are always getting in trouble in their neighborhood (see table 5). In fact, supervision is actually slightly less strongly related

TABLE 5

SELF-REPORTED DELINQUENCY WITHIN NEIGHBORHOOD CONTEXTS,
BY PATERNAL SUPERVISION

| SELF-REPORTED DELINQUENT ACTS | YOUNG ALWAYS GETTING IN TROUBLE | | | | | | | | |
|----------------------------------|---------------------------------|--------|------|------------------|--------|------|-----------------|--------|------|
| | Agree | | | Undecided | | | Disagree | | |
| | Agree (%) | | | Undecided (%) | | | Disagree (%) | | |
| | Low | Medium | High | Low | Medium | High | Low | Medium | High |
| 0 | 27 | 70 | 39 | 46 | 52 | 63 | 52 | 57 | 69 |
| 1 | 29 | 9 | 28 | 25 | 27 | 23 | 24 | 30 | 22 |
| 2+ | 44 | 21 | 32 | 29 | 21 | 14 | 24 | 13 | 9 |
| N | 90 | 43 | 71 | 72 | 62 | 124 | 243 | 169 | 468 |
| Gamma | | -.20 | | | -.25 | | | -.28 | |

to delinquency in neighborhoods where the young are perceived as always in trouble than among those where they are perceived as little trouble.

Neither does paternal support appear to interact significantly with perceived trouble (table 6). Emotional support does lead to slightly greater percentage differences among respondents in neighborhoods high in trouble. However, the relations persist within all subdivisions, and the gamma coefficients are only slightly different. As was the case with number of delinquent friends, when perception of trouble in the neighborhood is used to measure the availability of delinquent behavior patterns, the data fail to reveal the predicted pattern of interaction.

Table 7 summarizes the relationships between paternal supervision and support and self-reported delinquency in schools with variable delinquency rates; and, again, there appears to be no tendency for direct and indirect paternal control to make any greater difference in high-rate schools than in low-rate schools. For example, the gamma coefficients combining all low-rate schools (7.6%–10.8% with an official record) were $-.26$ for the paternal support measure and $-.30$ for paternal supervision as compared with $-.32$ and $-.27$ for all high-rate schools combined (26.2%–32.7% officially delinquent). Again, we fail to find support for the interaction hypothesis. Whether in a school with a high rate or a low rate of official delinquency, paternal supervision and support differentiate adolescents in terms of admitted delinquent acts equally well.

We have already seen that the probability of embracing "definitions favorable to the violation of the law" increases with increases in the number of close friends who have been picked up by the police. These definitions are, in turn, associated with delinquency involvement. Moreover, each of these variables, independent of the other, is related to delinquency. When the "definitions" score is held constant by subdivision,

TABLE 6
SELF-REPORTED DELINQUENCY WITHIN NEIGHBORHOOD CONTEXTS,
BY PATERNAL EMOTIONAL SUPPORT

| SELF-REPORTED DELINQUENT ACTS | YOUNG IN NEIGHBORHOOD ALWAYS IN TROUBLE | | | | | | | | | | | |
|-------------------------------------|---|----|-------|------|------------------|----|-------|------|-----------------|-----|-------|------|
| | Agree (%) | | | | Undecided (%) | | | | Disagree (%) | | | |
| | Low | 2 | 3 | High | Low | 2 | 3 | High | Low | 2 | 3 | High |
| 0 | 34 | 40 | 40 | 67 | 47 | 47 | 60 | 72 | 49 | 57 | 68 | 69 |
| 1 | 20 | 20 | 34 | 23 | 22 | 34 | 27 | 21 | 27 | 28 | 21 | 22 |
| 2+ | 46 | 40 | 26 | 10 | 31 | 22 | 12 | 7 | 24 | 15 | 11 | 9 |
| N | 83 | 45 | 35 | 31 | 81 | 65 | 48 | 57 | 212 | 157 | 204 | 278 |
| Gamma | | | — .32 | | | | — .30 | | | | — .25 | |

TABLE 7

GAMMA COEFFICIENTS RELATING SELF-REPORTED DELINQUENCY TO
PATERNAL SUPERVISION AND SUPPORT WITHIN SCHOOLS

| Schools and Percentage Officially Delinquent | Self-Report by Supervision | Self-Report by Support |
|---|-------------------------------|---------------------------|
| High rate: | | |
| 33% | -.30 (<i>N</i> = 92) | -.27 (<i>N</i> = 93) |
| 29% | .03 (<i>N</i> = 31) | -.19 (<i>N</i> = 29) |
| 26% | -.29 (<i>N</i> = 215) | -.30 (<i>N</i> = 212) |
| 26% | -.17 (<i>N</i> = 151) | -.29 (<i>N</i> = 144) |
| Middle rate: | | |
| 21% | -.44 (<i>N</i> = 102) | -.34 (<i>N</i> = 100) |
| 21% | -.41 (<i>N</i> = 112) | -.44 (<i>N</i> = 100) |
| 19% | -.10 (<i>N</i> = 29) | -.11 (<i>N</i> = 26) |
| Low rate: | | |
| 11% | -.32 (<i>N</i> = 285) | -.15 (<i>N</i> = 267) |
| 11% | -.28 (<i>N</i> = 143) | -.39 (<i>N</i> = 140) |
| 8% | -.27 (<i>N</i> = 184) | -.38 (<i>N</i> = 177) |

we find that paternal supervision and support are related to delinquent behavior in all subgroups as well (see tables 8 and 9). The relationships are slightly stronger among those with attitudes and beliefs conducive to law breaking than among those committed to more conventional standards, in that father's supervision and support are slightly more likely to have an effect among those who express attitudes and beliefs conducive to the violation of the law. In short, parental surveillance and support may make for a somewhat greater difference in delinquency involvement among adolescents

TABLE 8

SELF-REPORTED DELINQUENCY WITHIN "DEFINITIONS" SUBGROUPS,
BY PATERNAL SUPERVISION

| SELF-REPORTED DELINQUENT ACTS | DEFINITIONS FAVORABLE TO VIOLATION | | | | | | | | |
|-------------------------------------|------------------------------------|-------------|------|----------------|-------------|------|--------------------|-------------|------|
| | Favorable (%) | | | Neutral (%) | | | Unfavorable (%) | | |
| | Low | Me- dium | High | Low | Me- dium | High | Low | Me- dium | High |
| 0 | 26 | 44 | 45 | 50 | 54 | 64 | 68 | 75 | 78 |
| 1 | 29 | 36 | 28 | 29 | 30 | 24 | 21 | 16 | 17 |
| 2+ | 46 | 20 | 27 | 21 | 17 | 12 | 12 | 8 | 5 |
| <i>N</i> | 145 | 59 | 115 | 127 | 115 | 229 | 96 | 73 | 245 |
| Gamma | -.29 | | | -.20 | | | -.18 | | |

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TABLE 9

SELF-REPORTED DELINQUENCY WITHIN "DEFINITIONS" SUBGROUPS,
BY PATERNAL EMOTIONAL SUPPORT

| SELF-REPORTED DELINQUENT ACTS | DEFINITIONS FAVORABLE TO VIOLATION | | | | | | | | | | | |
|-------------------------------------|------------------------------------|------|----|------|----------------|------|-----|------|--------------------|------|-----|------|
| | Favorable (%) | | | | Neutral (%) | | | | Unfavorable (%) | | | |
| | Low | 2 | 3 | High | Low | 2 | 3 | High | Low | 2 | 3 | High |
| 0 | 26 | 39 | 46 | 50 | 52 | 46 | 63 | 70 | 70 | 69 | 74 | 79 |
| 1 | 30 | 29 | 31 | 30 | 27 | 32 | 26 | 23 | 16 | 22 | 17 | 18 |
| 2+ | 44 | 32 | 23 | 20 | 21 | 21 | 11 | 7 | 14 | 9 | 9 | 4 |
| N | 131 | 75 | 52 | 54 | 132 | 99 | 101 | 121 | 70 | 68 | 104 | 159 |
| Gamma | | -.29 | | | | -.25 | | | | -.16 | | |

who are not committed to conventional normative standards and who hold attitudes favorable to the violation of the law. Among adolescents committed to conventional standards, such external parental control may have lesser impact because of the marked strength of internal controls resulting from a firm acceptance of conventional definitions.⁸ This interaction should not mask the fact that, contrary to Sutherland and Cressey's (1966) arguments, parental supervision and support are related to delinquency even in the categories exhibiting definitions unfavorable to the violation of the law.

SUMMARY AND CONCLUSIONS

This study attempts to go beyond the usual concern with the relationships between delinquent companions and delinquent behavior to an assessment of arguments using differential association theory to encompass certain "known" relationships. Consistent with previous research and as predicated on the basis of the theory, the number of delinquent friends, the perception of "trouble" in the neighborhood, and the variable acceptance of attitudes and beliefs favorable to the violation of legal codes were significantly related to involvement in delinquent action. Moreover, those associating with delinquents are more likely to be delinquent, regardless of the effect of these associations on their attitudes and beliefs. This finding tends to support theories of delinquency which stress the importance of group

⁸ This finding is consistent with the claims of "containment theorists" (e.g., Reckless 1966, pp. 223-30; 1967) that elements of "inner containment" may play a greater role as deterrents to delinquency in situations of weak "outer containment" than in situations where external controls are more likely to be operative. For a critical analysis and reformulation of the containment perspective, see Jensen (1970, pp. 1-14; 1972).

pressure, group processes, and short-term situational motivations in the explanation of delinquency.

Using several alternative measures of the availability of delinquent patterns, the findings consistently tended to cast doubt on Sutherland and Cressey's (1966) arguments concerning the home and family in relation to delinquent behavior. Number of delinquent friends had little effect on the relationship between paternal supervision and delinquency. Nor did it have much of an effect on the association between affectionate father-son relationships and delinquent behavior. The case was similar when neighborhood patterns and school-delinquency rates were held constant. Some interaction was noted when definitions or verbalizations favorable to the violation of the law were controlled: the two family variables were more highly related to delinquent behavior among those whose definitions were most favorable to the violation of the law. However, the relationships persisted within all subdivisions.

In sum, what goes on in the family situation appears to have a significance of its own which is not encompassed by the differential association perspective as presented by Sutherland and Cressey (1966). The neutral or isolated child is more likely to be delinquent than the child who is loved by and attached to his parents even when delinquent patterns "outside the home" are scarce or absent. Moreover, contrary to some studies (Watt and Maher 1958, pp. 321-30), these data suggest that attitudes toward one's parents do affect attitudes toward public law and morality but that direct and indirect parental control have effects on delinquent behavior independent of such beliefs and attitudes. In short, the family has effects on delinquency involvement which cannot be attributed to the "internalization" of values, beliefs, or attitudes towards the law. Such findings are inconsistent with theories that view attitudes and beliefs concerning legal prescriptions as "the" variables accounting for the consequences of family life for delinquency.

As Marvin Olsen (1968, pp. 117-29) points out, actors are led to perpetuate patterns of social organization, not merely because they come to accept certain social norms as their own, but also through processes of social sanctioning, social manipulation, identification, and compliance. Just as actual and potential reactions of delinquent peers can influence behavior without necessarily shaping normative commitments at the same time, so the sensitivity of children to the actual and potential reactions of their parents may shape their behavior even if they do not form commitments to such standards. The theory of differential association stresses "definitions" and "cultural" variables (values, norms, and beliefs) to such an extent that processes shaping human behavior other than internalization of normative standards tend to be slighted.

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Accepting "Significant Others": Six Models¹

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Six alternative models of the ways in which individuals accept and organize information from potential sources are proposed. Parameter estimates are obtained from a set of experiments ($N = 110$), and the models are tested against data from an independent set of experiments ($N = 114$). Results of the tests favor a simple additive model. Two of the models proposed are elaborations of ideas recently proposed by Berger and Fisek (1970), and results of our tests are generally consistent with theirs. Further applications of the models to experimental and to natural settings are drawn, and some consequences of the results of the tests are discussed.

SELF-CONCEPT AND INFORMATION THEORY

Self-concept, according to the tradition growing from the writings of Cooley (1902) and of Mead (1934), is dependent upon the attitudes and opinions of others. If we restrict attention to evaluative aspects of the self, then this tradition would assert that the individual's high or low self-evaluation is a function of the positive and negative evaluations he receives from others. Moreover, the self-evaluation is dependent upon the evaluations received from a particular kind of others, "significant others," in Sullivan's (1947) terminology. In this paper, we propose and test alternative models intended to describe the processes by which the self incorporates others' evaluations.

The proposed models are tested in a series of laboratory experiments designed to evaluate propositions on the development of conceptions of ability of self and others. One experiment tested the proposition that a crucial determinant of becoming a significant other—that is, one whose

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evaluations "mattered" to the individual—is the individual's perception of other's ability at the task. An evaluator perceived to have high ability to perform the task himself was predicted to be accepted as a significant other. If the evaluator were perceived to have low ability, the individual was predicted to be far less likely to "cognize" the evaluations and to incorporate them into his ability conception. These predictions were confirmed (Webster 1969).

However, an interesting result of this first experiment is that the opinions of a low-ability evaluator were not totally ignored. They were relatively unlikely to affect the cognitions and future actions of the individual, but at least in some cases they did produce a measurable effect. This suggests the desirability of attempting to develop a more complete understanding of the ways in which individuals incorporate information from their environments in general and evaluations of ability from others in particular. The perspective we adopt is one shared by a family of theories which are loosely grouped together as "information theory."

From an information theoretic approach, the individual is viewed as an information processing system; he perceives, interprets, and assimilates data, and his cognitions and future actions are assumed to be systematically related to the nature of the information data. But it is clear that not all input sensory data are equally useful as information. The individual must select, evaluate, and combine data to reach a useful conclusion. The mechanisms of this process thus become of central importance.

For studying the information selection and utilization process, we formulate two specific questions. (1) How does the individual utilize conflicting information from various sources? and (2) how does the individual utilize congruent information from two differentiated sources, or from two equivalent sources? As answers, we propose six possible models of information processing. The models are assessed using data from two series of similar experiments, conducted at different times, but with comparable subject pools and nearly identical experimental procedures. The purpose and the design of the experiments have been reported in other works (Sobieszek 1972; Webster 1969); only aspects relevant to the models and the model testing we perform here will be described in this work.

The experiment consists of two parts, or phases. In phase 1, pairs of subjects enter the laboratory and perform a task which requires them to make 20 binary judgments about a series of slides. Each judgment is communicated to a third individual, who has been described as possessing either unusually high ability to make these judgments or unusually low ability. This third individual, the evaluator, supposedly decides whether each subject's judgment is correct and communicates this information to him. Actually, the situation is controlled in a number of ways. First, the slides have no objectively correct answers, and in fact extensive pretesting

has established that in the absence of social influence, the likelihood of choosing either is very close to .50 (see Webster 1969 for details). Thus it is plausible to the subjects that the evaluator's choices might disagree with or agree with their own. Second, communication between individuals is completely controlled. During the experiment, subjects cannot see each other, each other's choices, or the evaluator. (The evaluator is described as being another student from the same school who is seated in another room, but he exists only as a tape-recorded voice.) Thus it is possible for the evaluator to give either positive or negative evaluations to one or both subjects, regardless of their actual choices to a given slide, and the effects of any cues about the evaluator (other than his ability) are minimized and are standardized across conditions.

At the end of the 20 slides in phase 1, the evaluator announces his opinion of the subjects' performances. To each subject he gives either a high proportion of positive evaluations or a high proportion of negative evaluations. At this point, if a subject believes the evaluator, he should think that he is either very good at the task, or that he is very poor. Thus there are two possible values for the evaluator's ability (high or low), and two possible types of evaluations given to a subject (much better than his partner or much worse). Four conditions are produced by varying the evaluator's ability (the H or L conditions) and whether he gives the subject highly positive evaluations (the $[+]$ conditions) or highly negative evaluations (the $[-]$ conditions).

The data from the experiment are gathered in phase 2, when a second series of slides is presented. For each slide, each subject makes an initial choice which is communicated to his partner, restudies the slide, and then makes a private final choice. Communication is again controlled, so that subjects are told that their initial choices are in virtually continual disagreement. The basic operational prediction of the theory is that the higher the subject's conception of his own ability relative to that of his partner, the less likely he is to accept influence when he is told that their initial choices conflict. The proportion of times that subjects in each condition resolve disagreements in favor of self, $P(s)$, is the main statistic used to test these predictions. For example, a subject who refuses to change any of his initial choices would have a $P(s) = 1.00$; a subject who changes 16 out of the 20 disagreeing initial choices would have a $P(s) = .20$.

Combining the notation for the evaluator's ability and for the nature of his evaluations, the theory predicts the following ordering of conditions by the $P(s)$ statistic: $H[+] > L[+] > L[-] > H[-]$.² Results from

² The explicit derivation of this prediction is contained in the earlier report (Webster 1969). Exact theoretical reasons for the predictions are not central to this report and

TABLE 1

P(s) VALUES FOR THE SINGLE-SOURCE
EXPERIMENT

| | Condition | <i>N</i> | <i>P(s)</i> |
|----|------------|----------|-------------|
| 1. | H[+] | 19 | .79 |
| 2. | L[+] | 19 | .64 |
| 3. | L[—] | 20 | .56 |
| 4. | H[—] | 18 | .46 |

80 subjects in this experiment—which we will refer to as the “single-source” experiment—are given in table 1.³

In terms of information processing, three facts are significant. First, the observed ordering of conditions indicates that the individual has accepted and incorporated the evaluation information in phase 1 of the experiment and has used it to determine a feature of his behavior in phase 2. Second, in those conditions of the experiment where the only information available is from an individual described as possessing unusually low ability (the L conditions), subjects do not totally disregard the evaluations: the *P(s)* for the L[+] condition is greater than the *P(s)* for the L[—] condition. Third, not only is the relative evaluation information utilized (the *P(s)* for respective [+] conditions is greater than the *P(s)* for [—] conditions), but also, the source of the information is utilized; the effect of either positive or negative evaluations from the H is greater than the effect of the same evaluations from the L.

In order to use these results to describe precisely the ways in which subjects utilize information from two evaluators—both when that information is consistent and when the two evaluators disagree with each other—we need to estimate the strength of effect of each of these factors. In addition, we need a baseline estimator from which we can assess the additional positive or negative effects of the ability of the evaluator and of the [+] or [—] evaluations.

Such an estimator is the *P(s)* value produced in a second experiment in which the phase 1 evaluations were never communicated to subjects.

so are omitted; however it should be clear from the predicted ordering that *P(s)* is expected to vary directly with two factors: (1) the H or L ability of the evaluator, and (2) the [+] or [—] evaluations given to the subject.

³ Data from about 5%–10% of subjects in these experiments were excluded from analysis on grounds of considerable evidence that subjects did not meet one or more conditions of the experimental design. For example, subjects who clearly did not believe that the disagreements were real could not be said to be accepting or rejecting influence in making their final choices, and so were excluded. Complete criteria for exclusion as well as details on how the inclusion/exclusion decisions are made are available in Webster 1970.

They were told that their performances were being evaluated in phase 1, but that they would not be told the evaluations until after the entire study was completed. All other details of phases 1 and 2 were identical with the experiment previously described. This experiment may be called the "no-source" experiment, or O-S; the $P(s)$ in the O-S was .62 ($N = 30$).

Taking the .62 probability from the O-S as the estimate of the baseline effect of the disagreements without any evaluations, this figure may be subtracted algebraically from each condition of the single-source experiment in order to determine a table of evaluation weights, shown in table 2.

TABLE 2
ESTIMATED EVALUATION WEIGHTS FOR THE
SINGLE-SOURCE EXPERIMENT

| Evaluation | Weight |
|------------|--------|
| H+ | +.17 |
| H- | -.16 |
| L+ | +.02 |
| L- | -.06 |

These weights are an estimate of the combined effect of the evaluator and the evaluation in each of our H[+], H[-], L[+], and L[-] conditions.

By using the baseline estimator and our evaluation weights, we can predict the $P(s)$ values from other experiments in which there are two phase 1 evaluators.⁴ We can predict the $P(s)$ values for situations in

⁴ Because we make direct comparisons of data across these various experiments, a word about comparability of the situations and of the subject pools is in order. The experiments are all variants of a basic experimental situation developed at Stanford by Joseph Berger and his associates. Differences between experiments are slight, being limited to the changes required for tests of the different versions of the theory. The differences probably would not be noticeable to an observer who was not closely acquainted with the particular theory derivations under test. Subjects for the experiments reported were volunteers recruited from English courses at a California junior college. At the time of the experiments, none was under 17 years of age, and none was over 24. The single-source experiments were conducted in spring 1968 with male subjects. The two-source experiments were conducted in spring 1969 with female subjects, except for the O-S and H[+]H[-] conditions, which were conducted in summer 1970 with female subjects. All subjects were recruited from regular classes at the college, not from evening or summer sessions. Though the time difference may have produced slight differences in the subject pools, in terms of any of the frequently used measures—such as academic ability, racial or ethnic background, SES of families, etc.—there was no change during the time these experiments were conducted. The sex difference between the single-source experiments (male subjects) and the later experiments (female subjects) conceivably could make direct comparisons of data dangerous, but in the absence of specific information regarding the effect of sex in these experiments, we have chosen to treat this difference as inconsequential.

TABLE 3

P(s) VALUES FOR THE TWO-SOURCE EXPERIMENT

| Condition | <i>N</i> | <i>P(s)</i> |
|-------------------|----------|-------------|
| 1. H[+]L[+] | 21 | .80 |
| 2. H[+]L[-] | 20 | .75 |
| 3. H[+]H[-] | 27 | .67 |
| 4. H[-]L[+] | 20 | .57 |
| 5. H[-]L[-] | 21 | .42 |

which these two evaluators possess either equal or unequal ability and where they either agree or disagree as to the nature of their evaluations of performance.⁵

The two additional experiments for which the models may make predictions both involve subjects in phase 1 receiving evaluations from two evaluators. In the first, one evaluator in every group is described as possessing unusually high ability and the other as possessing unusually low ability. These two evaluators either agree (conditions 1 and 5 in table 3) or disagree (conditions 2 and 4) on their evaluations of each subject's performances. In the second experiment, shown as condition 3, both evaluators are described as possessing unusually high task ability, and they disagree on their evaluations of each subject's ability. The observed *P(s)* values from these five conditions are shown in table 3.⁶

⁵ There are 20 critical (disagreement) trials in all these experiments. For all tables in this report, the *P(s)* was calculated for the last 15 disagreement trials only, since there is reason to believe that the first few trials of the disagreement phase are used by subjects to adjust to an unfamiliar situation. Both variance across subjects and alternating behavior are greater for the first block of five trials than for any subsequent block of trials. Also, there is no theoretical reason to expect a systematic change in results as a function of the disagreement resolution process, nor is there evidence of such a change in these experiments. Data published by Moore (1969), as well as examination of unpublished data from 12 conditions reported by Camilleri and Berger (1967), show that for most conditions which use this experimental situation, the *P(s)* curve drops (about .02-.09) between the first and second quarter of critical trials and usually is relatively flat thereafter. This is the case for most of our conditions reported in tables 1 and 3 here, and it seems to be a general result produced by the disagreement trials. We know of no theoretical reason to expect the drop, but it seems plausible to us that a subject would not fully consider a partner's disagreeing initial choice until he had time to practice a bit with the equipment and with the decision-making task. Thus we conclude that the final three blocks of trials provide the most representative and stable estimate of *P(s)* for each condition. If *P(s)* data were calculated on the basis of all 20 trials, the figures for most conditions would be the same or higher (though no more than .02 in any case). This would change some individual condition comparisons in tables 4-8 and 10-11, but it would not alter relative assessments of the models.

⁶ Note in table 2 that $|H-| \approx |H+|$ and $|L-| > |L+|$. The self-evaluation theory which guided these experiments does not give any reason to expect that under some circumstances negative evaluations would produce greater effect than positive evalua-

THE MODELS AND THE TESTING

Prediction of results of these more complex experiments depends upon the way in which individuals are believed to utilize information. This process may be represented by a variety of models, derived from a corresponding variety of different substantive theories.⁷ We will consider six models which seem to represent intuitively plausible assumptions and which make distinguishable predictions for behavior in this experimental situation. Then we will assess each model, based both upon the empirical

tions from the same evaluator, nor is any feature of this general experimental design clearly related to the observed effect. Therefore it might seem reasonable to impose the added condition that effects of positive and negative evaluations from the same evaluator have the same degree of effect, or more formally: $|H+| = |H-|$ and $|L+| = |L-|$. This restriction offers the advantage of increasing the amount of information used to estimate the evaluation effect parameters, for two conditions then would be used for each. As the simplest way of using two conditions, the effects of the positive and negative evaluations from the same source could be averaged, to obtain the following values: evaluating $H(+ \text{ or } -)$, weight $\pm .17$; evaluation $L(+ \text{ or } -)$, weight $\pm .04$. However there is no readily apparent justification for adding such a restriction to parameters, and use of these values would not change the results of any comparisons to be made later in this paper. Therefore, we have decided to use the simplest estimates of the evaluation effect parameters, those shown in table 2, and to note that they were estimated on the basis of minimal information (usually, about 20 subjects for 15 trials, or 300 units of information).

⁷ Many investigators, from learning theorists to methodologists, have reported work on issues related to information combining, defined broadly (see Bush and Estes [1959]; Coombs, Dawes, and Tversky [1970]; and Rosenberg [1968a] for recent summaries). However much of this work is not directly applicable to our interests here. Questions of handling data are probably least closely related. Coombs (1964, pp. 284-91) discusses the need to find single measures from incomparable components; for example, a single IQ score from the underlying factors measured by the test items. This is an objective combining procedure—presumably the result of a deliberate decision made after explicitly considering various weighting options. By contrast, our models are designed to reflect subjective processes by which individuals—not necessarily deliberately—reach a single measure such as ability self-evaluation or overall status of an individual from either consistent or inconsistent units of information. How individuals select and combine units of information to reach a single opinion or attitude has been studied most often by cognitive psychologists. The issue whether additive models (similar to our model 3) or averaging models (similar to our model 4) are preferable has been debated (see Feldman 1968, pp. 744-55; and Kepka and Brickman 1971), though apparently without a clear-cut preference emerging. For situations of interest to psychologists—such as forming an overall impression of someone based upon knowledge of several adjectives purported to characterize him—the issue may well be highly relevant, but for more sociological concerns (such as selecting significant others or evaluating ability of others from knowledge of status characteristics) the number of information units is probably smaller, and what seems more relevant is whether some information is completely ignored (our models 1 and 2). In any case, we feel that Rosenberg's criticism (1968b, p. 765) is well taken; namely that this research relies heavily upon giving subjects scant information about a hypothetical other in a necessarily underdefined experimental setting, and then asking him to report his opinions of the other or how he might treat him. We hope our work constitutes a contribution since our results are based upon observing actual, not hypothetical, behavior which is predicted by the models.

consideration of success of prediction and upon theoretical considerations of simplicity and adequacy.

The six models fall into two general categories: additive models and operator models. Additive models assume that the combining process may be adequately represented by a summation of available information—the individual adds units of information, which may or may not be equal in importance, to arrive at the total information which forms the basis for his action. Operator models assume that the individual takes the unit of information and performs some more complicated operation upon it before assimilating the information. In this experimental situation, for example, one simple additive model (3) assumes that the individual adds up the evaluations from two sources to come to a total weight which he assigns to the information. A simple operator model (6) assumes that there is some baseline effect, such as the $P(s)$ of the O-S experiment, which is then proportionally altered by the effect of evaluations and the ability of the evaluator. Therefore, estimates derived from an operator model are a function of the baseline probability, while estimates derived from additive models are independent of the baseline probability. Since additive models assume the more simple combining process, we will examine them first.

A typical term of the additive models is shown by $P_O + E_{H+} + E_{L+} = P_{H+L+}$, where $P_O = P(s)$ for the O-S experiment, E_{H+} = the estimated evaluation weight of a positive evaluation from the high-ability evaluator, E_{L+} = the estimated evaluation weight of a positive evaluation from the low ability evaluator, and $P_{H+L+} = P(s)$ for the $H[+]L[+]$ condition of the two-source experiments.

1. The Single-Source Model

When the individual is aware that more than one other individual is giving him opinions, a first step in utilizing this information is to decide whether to accept all available data, or to perceive it selectively and to ignore some of it. In this experimental situation, the question is whether a subject who is receiving evaluations from two evaluators will pay attention to both of them or whether he will simplify the situation cognitively by choosing only one of them. The first model we examine assumes the latter process.⁸ Thus, it becomes necessary to specify which evaluator

⁸ This model is implicitly adopted by many of those who have worked in the areas of cognitive consistency and distortion. The assumption is frequently made, for example, that potential information which conflicts with (or is dissonant with) the individual's previous beliefs (or with his psychological needs) will not be accepted so readily as information which is consistent (see Heider 1946, 1958; Secord and Backman 1961, for similar statements). Though our results do not provide a definitive test for acceptance or rejection of these orientations, it is important to point out that the theoretical issues being examined have implications beyond the specific models.

will be accepted and which ignored. A basis for making this specification is provided in the original theory which guided these experiments. When both a high-ability evaluator and a low-ability evaluator are available, it seems reasonable to expect that the individual will accept the high-ability evaluator and will ignore the low-ability evaluator. For these experiments, this process would have two consequences: First, the $P(s)$ from the $H[+]$ condition would equal the $P(s)$ from the $H[+]L[+]$ and the $H[+]L[-]$ conditions. Second, the $P(s)$ from the $H[-]$ condition would equal the $P(s)$ from the $H[-]L[+]$ and the $H[-]L[-]$ conditions.

For the case of two disagreeing high ability evaluators (the $H[+]H[-]$ experiment), it is still difficult to predict which of them would be accepted. To obtain a simple estimate of $P(s)$, we will assume that half the subjects in this experiment accept each evaluator. Those who accept only the $H+$ evaluations will display the same $P(s)$ as subjects who received positive evaluations from one high-ability evaluator in the single-source experiments; those who accept only the $H-$ evaluations will display the $P(s)$ of subjects in the comparable condition of the single-source experiments. The respective $P(s)$ figures for these two conditions are .79 and .46, which we average to obtain an estimate of .63 for the $H[+]H[-]$ experiment from this model. The $P(s)$ figures predicted by this model are shown in table 4, with the figure for the $H[+]H[-]$

TABLE 4
SINGLE-SOURCE MODEL: EXPECTED AND OBSERVED $P(s)$ VALUES

| Condition | Expected $P(s)$ | Observed $P(s)$ | Discrepancy (Exp. - Obs.) |
|---------------------|-----------------|-----------------|--------------------------------|
| 1. $H[+]L[+]$ | .79 | .80 | .01 |
| 2. $H[+]L[-]$ | .79 | .75 | .04 |
| 3. $H[+]H[-]$ | [.63] | .67 | [.04] |
| 4. $H[-]L[+]$ | .46 | .57 | .11 |
| 5. $H[-]L[-]$ | .46 | .42 | .04 |

experiments in brackets to indicate that an additional assumption was required to get the predicted figure. The general additive model was used for calculating the predicted figures, with the restriction for the two-evaluator situation that $E_{L+} = E_{L-} = 0$. Therefore, $P_O + E_{H+} = P_{H+L+}$.

Recent experimental work reported by Berger and Fisek (1970) has addressed similar issues. Their results appear to support the conclusion that the individual will accept all available information and will somehow combine it, rather than accepting consistent information and rejecting inconsistent information. Therefore, support of the first model from our data would be inconsistent with Berger and Fisek's conclusion; rejection of this model would constitute an independent confirmation of their conclusion.

Comparison of predicted with observed figures in table 4 indicates that this first model provides a close prediction of the $P(s)$ for condition 1 and moderate success for conditions 2, 3, and 5. The approximation of prediction to observed results is less close for condition 4.

2. The Single-Source Given Disagreement Model

A variant of the first model assumes that the tendency of the individual is to accept more than one source of information, so long as all sources are consistent. When potential sources disagree in their information, then the individual will choose one of them and disregard the other, as in the first model. More precisely, this second model assumes that the individual will follow an additive process for consistent information (conditions 1 and 5 of the two source experiments). When the potential sources disagree (conditions 2, 3, and 4) he will resolve the situation by accepting only one source. In other words, this model asserts that the cognitive distortion implied in the idea of accepting only one of several potential sources will occur only when the sources conflict with each other. Table 5 presents the expected and observed $P(s)$ values for this second model.

TABLE 5

SINGLE-SOURCE GIVEN DISAGREEMENT MODEL: EXPECTED AND OBSERVED $P(s)$ VALUES

| Condition | Expected $P(s)$ | Observed $P(s)$ | Discrepancy (Exp. - Obs.) |
|-------------------|-----------------|-----------------|--------------------------------|
| 1. H[+]L[+] | .81 | .80 | .01 |
| 2. H[+]L[-] | .79 | .75 | .04 |
| 3. H[+]H[-] | [.63] | .67 | [.04] |
| 4. H[-]L[+] | .46 | .57 | .11 |
| 5. H[-]L[-] | .40 | .42 | .02 |

This elaborated model provides a good fit to the observed data for condition 1 and a better fit than the single-source model for condition 5. It makes the same predictions of conditions 2, 3, and 4. The single-source given disagreement model thus provides a better fit than the simple single-source model, the simplest of the balancing models.

3. The Simple Additive Model

The third model assumes that the individual accepts all information available in the situation and simply sums it—both when sources agree and when they disagree—in order to arrive at a useful conclusion. The formula for calculating predicted values for the simple additive model is the

TABLE 6
SIMPLE ADDITIVE MODEL: EXPECTED AND OBSERVED $P(s)$ VALUES

| Condition | Expected $P(s)$ | Observed $P(s)$ | Discrepancy (Exp. - Obs.) |
|-------------------|-----------------|-----------------|--------------------------------|
| 1. H[+]L[+] | .81 | .80 | .01 |
| 2. H[+]L[-] | .73 | .75 | .02 |
| 3. H[+]H[-] | .63 | .67 | .04 |
| 4. H[-]L[+] | .48 | .57 | .09 |
| 5. H[-]L[-] | .40 | .42 | .02 |

same as the general additive model, with no additional restrictions: $P_O + E_{H+} + E_{L+} = P_{H+L+}$. Table 6 reports the predicted and observed $P(s)$ values, using the simple additive model.

The simple additive model gives a better overall fit to the observed data than either the single-source model or the single-source given disagreement model. It retains the good predictions of conditions 1 and 5 in model 2, provides the same fit as models 1 and 2 for condition 3, and has a better fit for conditions 2 and 4 than either previous model. Also for condition 3, the predicted $P(s)$ and observed discrepancy are no longer bracketed, since both the H[+] and the H[-] are weighted in this model. In addition, we note the greater conceptual simplicity of the simple additive model than of the single-source given disagreement model. Both on grounds of theoretical simplicity and on grounds of accuracy of empirical prediction, the simple additive model is to be favored over either of the two balancing models.⁹

4. The Averaging-Effects Model

The fourth model asserts that the total effect of each additional potential piece of information is diminished by the effect of every previous piece

⁹ Both models 1 and 2 would predict two populations of subjects in all two-evaluator experiments: those who have accepted a source and those who have not. This may be used to provide another type of test of these models. For condition 3 we assume that all (or nearly all) subjects will accept one and only one evaluator. If this occurred, it would produce a bimodal distribution of subjects: those who accepted H[+] would have a high $P(s)$ and those who accepted H[-] would have a low $P(s)$. Examination of the data from condition 3 shows no evidence of bimodality, but rather a unimodal, approximately normal distribution about the mean. Also, existence of two distinct populations increases variance across subjects within a condition, but the variance across subjects of the O-S condition (the one most nearly comparable with the H[+]H[-] condition) is more than two times greater than variance within the H[+]H[-] condition. Thus the distribution of subjects in condition 3 is also inconsistent with models 1 and 2.

of information; that is, that information from more than one of several equivalent sources is averaged to produce the final cognition.¹⁰ A typical term of an averaging model is: $P_{H+L+} = P_O + (E_{H+} + E_{L+})/k$, where k is the number of potential sources of information, and the other terms are interpreted as in the previous models.

This averaging model (or the elaboration of it proposed as model 5 below) appears to be an explicit statement of the combining model proposed by Berger and Fisek (1970). Thus, acceptance of a version of this model from our data would probably be consistent with their conclusion, and rejection would probably be inconsistent. However it should be noted that from their description of the combining model it is not possible to be certain that our averaging model adequately represents their ideas.

In any empirical operation, the averaging model seems most plausible for situations where the information available is inconsistent; that is, individuals may average inconsistent information but they will not do this for consistent information. To assert that individuals average information from one high-ability evaluator and one low-ability evaluator when the evaluators agree is to assert that the agreement from the low-ability evaluator in some way decreases the subject's confidence in the information from the high-ability evaluator. Thus, in our experimental situation, this model predicts that the effect of a negative evaluation from a low-ability evaluator would decrease the strength of effect of a positive evaluation from a high-ability evaluator. Though at first it might seem intuitively unlikely, this assertion can plausibly be argued. The low-ability evaluator in these experiments is expected to make some mistakes in his evaluations, and if the high-ability evaluator always agrees with him, then the high-ability evaluator may be making a few mistakes also.

¹⁰ This type of model seems to be of greatest interest to cognitive psychologists, and it possesses considerable intuitive appeal as well. Interestingly, Triandis (1968, pp. 723-30) concluded that for semantic differential measures of opinion and social distance measures of behavior toward hypothetical others, no important cultural differences exist to affect choice between a congruity model (similar to our 1), a summation model (similar to our 3) or a weighted average model (a more complex version of our 4). Triandis's data suggest that the summation model was slightly preferable cross-culturally on social distance but not on opinion items. As we noted above, this result is based upon asking respondents how they would behave rather than by observing actual behavior. For an individual confronted with large amounts of information—say knowledge of another's behavior in 20 different situations—this multiplying process seems intuitively likely; it says that all units of information will be utilized, but that the utility of each unit declines. Anderson (1968, pp. 731-34) measured effects of various descriptive adjectives upon overall liking for a hypothetical individual. His results seem to favor a more complex version of our model 6. However, he also notes that Anderson and Jacobson (1965) concluded that inconsistent items are treated as less important than consistent items, a result which has some elements of our averaging (4) and operator (6) models.

Thus, agreement of a low-ability evaluator could be a factor in reducing the credibility of the high-ability evaluator: "If that stupid person agrees with him, then I don't believe what he says."

We first assume the simpler condition, that the averaging model may be applied to all cases. Table 7 presents a comparison of the observed $P(s)$ values and those which would be expected from a simple averaging model.

TABLE 7
SIMPLE AVERAGING MODEL: EXPECTED AND OBSERVED $P(s)$ VALUES

| Condition | Expected $P(s)$ | Observed $P(s)$ | Discrepancy ([Exp. — Obs.]) |
|-------------------|-----------------|-----------------|--------------------------------|
| 1. H[+]L[+] | .72 | .80 | .08 |
| 2. H[+]L[-] | .68 | .75 | .07 |
| 3. H[+]H[-] | .63 | .67 | .04 |
| 4. H[-]L[+] | .55 | .57 | .02 |
| 5. H[-]L[-] | .51 | .42 | .09 |

Compared to the simple additive model, the simple averaging model gives the same prediction for condition 3 and a better prediction for condition 4. However, the simple additive model gives better predictions for conditions 1, 2, and 5. Thus the simple averaging model is better than the simple additive model for only one case; however, that case is the one for which the simple additive model gives a particularly poor prediction.

5. The Averaging Given Disagreement Model

For model 5, we assume the intuitively more likely condition: that the individual averages information only when he is exposed to inconsistency, when the evaluators disagree. We make the further assumption that the simple additive model should be applied in cases of agreement, since it provided the best fit to those conditions. Table 8 presents the results of predicted and observed $P(s)$, using the model which assumes a simple additive process when information is consistent, and averaging when it is inconsistent.

The averaging given disagreement model retains the good prediction of condition 4 from the simple averaging model, and it incorporates the good predictions of conditions 1 and 5 from the simple additive model. All predictions from this model are reasonably close to the observed data except condition 2. By comparison with predictions from the simple additive model, however, predictions from the averaging given disagreement

TABLE 8

AVERAGING GIVEN DISAGREEMENT MODEL: EXPECTED AND OBSERVED $P(s)$ VALUES

| Condition | Expected $P(s)$ | Observed $P(s)$ | Discrepancy (Exp. — Obs.) |
|-------------------|-----------------|-----------------|--------------------------------|
| 1. H[+]L[+] | .81 | .80 | .01 |
| 2. H[+]L[-] | .68 | .75 | .07 |
| 3. H[+]H[-] | .63 | .67 | .04 |
| 4. H[-]L[+] | .55 | .57 | .02 |
| 5. H[-]L[-] | .40 | .42 | .02 |

model are not significantly better. The simple additive model makes a considerably better prediction for condition 2, and the averaging given disagreement model makes a better prediction of condition 4. Both models make the same prediction for condition 3. Therefore, on the criterion of empirical prediction, choice between these models is not clear-cut.

On grounds of conceptual simplicity, the simple additive model is clearly preferable. The averaging given disagreement model requires that an additional assumption be made, and moreover, it provides no theoretical reason for using the simple additive model in cases where the available information units are consistent. For the tests shown in table 8, the simple additive model was used for the consistent evaluation conditions on the ad hoc grounds that it provided a good fit to observed data, but acceptance of the averaging given disagreement otherwise additive model would require a more complete justification of this. Such a justification, of course, remains to be worked out. Therefore, on grounds of theoretical simplicity, and especially in view of the fact that the simple additive model provides a reasonably good fit of the data, we conclude that it is to be preferred over the averaging given disagreement model.

6. A Simple Operator Model

An operator model assumes that an individual processes information through a multiplicative rather than an additive process. This model takes the O-S probability of rejecting influence (.62) as a baseline, and then multiplies it by an estimate of the multiplicative effect of positive or negative evaluations from a high-ability or a low-ability evaluator to obtain the observed $P(s)$ of the single-source conditions.¹¹ These estimates of multiplicative effect will be represented by α 's.

¹¹ In general, additive models are conceptually simpler than operator models and therefore would be preferred so long as the operator model did not provide a markedly superior fit to the observed data. In view of the fact that the simple additive model has provided a close fit to the data, it does not seem likely at this point that predictions would be improved by an operator model. However at the time this

TABLE 9
 α ESTIMATES FOR OPERATOR MODEL

| Parameter | Estimate |
|---------------|----------|
| α_{H+} | 1.274 |
| α_{H-} | 0.742 |
| α_{L+} | 1.032 |
| α_{L-} | 0.903 |

In order to obtain the estimate of α for a positive evaluation from a high-ability evaluator, for example, we take the O-S $P(s)$ figure as the estimate of baseline effect of no evaluator and then determine the figure by which it must be multiplied in order to obtain the observed $P(s)$ for the $H[+]$ condition. Thus the formula for obtaining α estimates for the $H[+]$ conditions: $\alpha_{H+}(P_0) = P_{H+}$, or $\alpha_{H+} = P_{H+}/P_0$. Using this general formula and solving for the α estimates of the $H[+]$, $H[-]$, $L[+]$, and $L[-]$ conditions, we get the values shown in table 9.¹²

A typical term for the operator model is given by: $\alpha_{L+}[\alpha_{H+}(P_0)] = P_{H+L+}$, where P_0 is the $P(s)$ for the O-S experiment and P_{H+L+} is the predicted value for the $H[+]L[+]$ experiment. Using this formula, the $P(s)$ values shown in table 10 would be predicted.

The operator model does not do a particularly satisfactory job of predicting the observed values for the two-evaluator experiments. Only for conditions 1 and 5 are predictions reasonably close to the observed figures; for conditions 3 and 4 the fit is poor; and for condition 2 it is moderate. By comparison with the additive models considered, the operator model does less well than any of them at predicting the observed figures. In particular, the fit by the operator model is considerably poorer than it was by the simple additive model, the one which generally made

investigation was undertaken this outcome was not apparent, and it is always possible to improve upon a prediction; hence one simple operator model was formulated for testing. Especially in view of the prevalence of models of this type in learning psychology and cognitive psychology (see Rosenberg 1968a), it seems worthwhile to examine the results of applying the operator model to this situation.

¹² These estimates are dependent upon the O-S probability; by comparison, the additive models use estimates which are independent of the O-S probability. That is, one may speak of the effect of a positive evaluation from a high-ability evaluator as having a value of $+.17$ (shown in table 2), and this value could be added to whatever the baseline probability for no evaluator might be in another experimental situation. This is not the case for the operator model, for the α estimates speak of a proportional increase over the given baseline probability for the given situation. Thus, the α estimates are specific to the experimental situation and to the subject pool used. One could not use the α estimates from one experimental situation in a different situation, or use α estimates from one subject pool in another subject pool.

TABLE 10

OPERATOR MODEL: EXPECTED AND OBSERVED $P(s)$ VALUES

| Condition | Expected $P(s)$ | Observed $P(s)$ | Discrepancy (Exp. — Obs.) |
|-------------------|-----------------|-----------------|--------------------------------|
| 1. H[+]L[+] | .82 | .80 | .02 |
| 2. H[+]L[-] | .71 | .75 | .04 |
| 3. H[+]H[-] | .58 | .67 | .09 |
| 4. H[-]L[+] | .47 | .57 | .10 |
| 5. H[-]L[-] | .41 | .42 | .01 |

the best predictions. Based upon these comparisons, we conclude that the additive models as a set, and especially the simple additive model, are preferable to operator models. Though operator models in general may have more intuitive appeal, the additive models are preferable on grounds both of conceptual simplicity and adequacy of empirical predictions.

DISCUSSION, THEORETICAL SIGNIFICANCE, IMPLICATIONS

The results of our model testing support the following conclusions. First, among the additive models, the simple additive model is preferable. It is the simplest model and the one which makes the most satisfactory predictions of observed data. Second, the additive models are preferable to the operator model for the same reasons; namely, the additive models are simpler, and they enable better predictions of the observed data.

In terms of the question of how individuals accept information from a variety of potential sources, the results of our comparisons suggest some more general conclusions. Whether individuals assimilate consistent information more easily than information which is inconsistent, or whether they accept all information without regard to consistency, has been studied repeatedly by different investigators in a variety of empirical settings. The former process is represented by our single-source and single-source given disagreement models; the latter, by the simple additive and the averaging models. The results of the comparisons of predicted and observed effects support the latter models and, thus, the latter process.

The recent experimental work by Berger and Fisek (1970) also has attempted to differentiate between these two processes (which they called, respectively, balancing and combining). Their results were partially consistent with ours; they found support for the combining process. However, Berger and Fisek report results which are restricted to cases in which individuals had conflicting information from two sources who were identical in plausibility, or in likelihood of being accepted: all information

came from an experimenter. In our experiments, the plausibility of the low-ability evaluator is lower than that of the high-ability evaluator. The differential plausibility permits making two sorts of discriminations between models which are not possible when all evaluative information comes from a source of high plausibility.

First, the differential permits distinguishing the results of combining information from the results of ignoring both potential sources, something which Berger and Fisek note is not possible in their experiments reported to date. For example, the $P(s)$ for our $H[+]H[-]$ condition shown in table 3 is .67, which is close to the $P(s)$, figure of .62 for the O-S experiment. The figures from these two conditions would be expected to be nearly the same if subjects were either (1) accepting and combining information from both potential sources; or (2) ignoring both of them. However, the fact that the $P(s)$ for the $H[+]H[-]$ condition differs from both the $P(s)$ for the $H[+]$ condition and from the $H[+]L[-]$ condition is consistent only with the interpretation that subjects accept information from both potential sources and then combine it.

The second way in which our results differ from those of Berger and Fisek is that, whereas they compared only two general types of models (combining and balancing), we tested several specific versions of both types. In general, our results agree with theirs: the evidence for some sort of combining process was stronger than the evidence for a balancing process. However, the results are not strictly equivalent. The combining model which Berger and Fisek proposed and for which they found support seems most closely related to our simple averaging model (4), which asserts that individuals will combine bits of information by weighting them. While this model makes fairly good predictions to our observed data, our simple additive model (3) makes even better predictions. The simple additive model assumes that the individual combines information, not by weighting it, but simply by summing all consistent and contradictory information. For an experiment in which both sources of information are of equal importance, such as the experiment Berger and Fisek reported and our $H[+]H[-]$ condition, it is impossible to distinguish the predictions of the simple additive model from the predictions of the simple averaging model. For this, data are needed from experiments in which the contradictory sources of information are not equally plausible, and our results for these conditions of our experiments were better for model 3. Thus it seems more accurate to consider our results a refinement of the theoretical ideas proposed by Berger and Fisek.

An especially appealing feature of the model which provided the best fit to the observed data, the simple additive model, is that it is the simplest of the six models proposed. The process of information utilization described by the simple additive model is even a simplification of the

process hypothesized by Berger and Fisek, if our interpretation of their ideas is accurate. We are encouraged that the processes of interpreting and assimilating information, which are often considered to be extremely complex, may be represented relatively simply for this situation. Of course, the results of similar experiments in more complex social situations—for example, with three evaluators—could well require a more complex theoretical conceptualization.

It may not be immediately apparent that the simple additive model represents one intuitively very plausible description of the way in which individuals utilize information; namely, that they give a positive or negative evaluation some weight, which is then multiplied by the individual making the observation to arrive at the overall importance of the information. Such a process may seem to be a multiplicative process at first glance; however it may easily be seen that it is not, by the following considerations. Using the intuitive model, we could say that the weight of a single evaluation in affecting the $P(s)$ is .01. Then the individual multiplies this weight by some factor determined by the source and by whether the evaluation was positive or negative, and finally the resulting figure is added to the baseline estimator to predict the $P(s)$ for any given combination of evaluators and positive or negative evaluations. At this point, it should be clear that the process described is no different from that envisioned in the simple additive model, for in fact what was multiplied by .01 then would be the evaluation weights shown in table 2. Though this description of the process of information usage is formally no different than the earlier description of the simple additive model, it may help to increase its intuitive appeal.

Finally, we may consider some implications of the numerical values used in this work for parameter estimates of effect of evaluations for the additive models. First, comparisons may be drawn between the estimates of effect of a positive evaluation versus a negative evaluation from the same evaluator by comparing the estimates of the H+ to H— and the estimates of L+ to L— in table 2. The estimates of effect of evaluations from a high-ability evaluator do not differ appreciably: positive evaluation is predicted to raise the $P(s)$ by .17; and negative evaluation, to lower the $P(s)$ by .16. However, the estimates of effect of evaluations from the low-ability evaluator do differ appreciably: positive evaluation is predicted to raise the $P(s)$ by only .02; and negative evaluation, to lower the $P(s)$ by .06. In absolute terms, the difference between .02 and .06 is small, and it is possible to ascribe it to chance factors operative in this experimental situation. But these estimates enable quite good predictions for the second series of experiments, in which there were evaluations from a low-ability evaluator. Moreover, though the absolute difference between .02 and .06 is small, the proportional difference is

considerable. Therefore, we may ascribe some reliability to these figures and consider an implication of this proportional difference.

The idea is frequently expressed in social psychology that individuals in some sense prefer a positive self-image, and thus that they will act, either behaviorally or cognitively, to maximize the actual or the perceived positive evaluations they receive from others. One way of maximizing positive evaluations from others is to emit behaviors valued by those others; for example, Zetterberg (1957) has postulated that individuals will give compliance in return for positive evaluations. A second way to maximize positive evaluations which has been frequently posited is cognitively to distort evaluations from others; either to change the valence of negative evaluations or to ignore a greater proportion of negative than of positive evaluations (for example, see Heider 1944; especially pp. 368-69). The behavioral means of seeking positive evaluations was not available to subjects in our experiments, but the cognitive means was available.

For the high-ability evaluator cases, it may not be surprising that the effects of the positive evaluations and the negative evaluations were virtually identical. The high-ability evaluator is one whose opinions would be difficult to ignore from any theoretical or intuitive viewpoint. But there is reason to believe that the low-ability evaluator may be overlooked by some subjects. Indeed, this is a central assertion of the theory under test in these experiments: the low-ability evaluator is perceived to be less competent to evaluate performance, and thus he is less likely to become a significant other whose evaluations become part of the self-evaluation of the individual.

In view of these considerations, it is striking that it is the positive evaluations from the low-ability evaluator which have the smaller effect in this situation. Whether his positive evaluations are ignored by a greater proportion of subjects, or whether all subjects ignore a greater proportion of his positive evaluations (an independent question which cannot be answered from the data we present here), it is the positive evaluations, not the negative evaluations, which are more ignored. If confidence may be placed in our estimated values, positive evaluations are ignored by a ratio of 3 to 1 over negative evaluations. This interpretation, of course, is directly counter to an asserted tendency to maximize the self-image, or at least it is counter to any asserted tendency consistent with the Cooley-Mead tradition regarding the sources of self-evaluation.

This conclusion may be seen more clearly by constructing and testing a model which assumes that the individual's tendency is to maximize the self-image through selective perception of the evaluations received from two evaluators.¹³ Several variants of the basic model could be constructed;

¹³ We thank an extremely conscientious and helpful (but anonymous) *AJS* reviewer

we shall examine only the simplest for illustration. In the simplest case, the subject is assumed to overlook negative evaluations, whether they come from the H or from the L evaluator. Thus, $H[-] = L[-] = 0$. So that we can use the estimated evaluation weights from table 2, we will assume that it is only in the more complex social situation of two evaluators that the negative evaluations are overlooked. In this model, $H[+]H[-] = H[+]L[-] = H[+]$, and $H[-]L[+] = L[+]$. For condition 1, where the individual receives positive evaluations from both evaluators, we will assume that the simple additive process applies; and for condition 5, where both evaluators give negative evaluations, we will assume that both of them are ignored so that this condition is equivalent to the O-S condition. Table 11 presents expected and observed $P(s)$ values for this model.

TABLE 11
SELF-MAXIMIZING MODEL: EXPECTED AND OBSERVED $P(s)$ VALUES

| Condition | Expected $P(s)$ | Observed $P(s)$ | Discrepancy (Exp. - Obs.) |
|---------------------|-----------------|-----------------|--------------------------------|
| 1. $H[+]L[+]$ | .81 | .80 | .01 |
| 2. $H[+]L[-]$ | .79 | .75 | .04 |
| 3. $H[+]H[-]$ | .79 | .67 | .12 |
| 4. $H[-]L[+]$ | .64 | .57 | .07 |
| 5. $H[-]L[-]$ | .62 | .42 | .20 |

As can be seen from the data in table 11, the self-maximizing model does a particularly poor job of predicting the observed data. Only for condition 1, where we actually used the prediction of another model, is the predicted value acceptably close to the observed.

Finally, one may compare the relative strengths of effect of the high- and low-ability evaluators.¹⁴ If we consider the estimates of .16 and .17 to be reasonably accurate for the high-ability evaluator, and the accurate estimate for the low-ability evaluator to be somewhere between .02 and

for suggesting this test, as well as for providing ideas incorporated in n. 9 above and elsewhere.

¹⁴ It should be noted before beginning this discussion that we are here going beyond what has been definitely demonstrated in the experiments. Our intent has been to construct models of more general applicability; that is, models which are useful in understanding processes of information utilization in natural settings which are more complex than our experimental setting. Just because natural settings are complex, factors other than those we treat as independent variables (ability of the evaluators and positive or negative evaluations) will operate, and some of these factors could be important enough to obscure or to alter the findings established experimentally.

.06, then it is evident that the high-ability evaluator is somewhere between three and nine times as effective as the low-ability evaluator in this situation. One simple interpretation of this finding is that experts are considerably more influential than nonexperts in determining an individual's estimate of his ability.

A more interesting interpretation of this finding relates to practices common in fields where evaluations of an individual's work must be made by others who may not be competent to evaluate it. Often the response to ambiguous or unreliable evaluative information is to increase the number of others who are asked to become evaluators. For example, when articles are submitted to professional journals for publication, it is common to refer them to one or more readers who have worked in the same areas as the author. Especially when it is difficult to find a single referee who is considered highly competent to evaluate, multiple opinions are sought. Or, in the case of school children, when the homeroom teacher has difficulty in making a clear assessment of a child's ability, opinions frequently are sought from a number of other teachers who may not have had nearly so much opportunity to observe the child or nearly so great a competence to evaluate his performance.

The large difference in our estimates of effect of the high- and the low-ability evaluators makes the routine practice of soliciting multiple evaluations from others of doubtful competence appear unsound. In these experiments, for example, if one adopts the model which did the best job of predicting data from the two-evaluator experiments, the simple additive model, then the effect of a single high-ability evaluator is equal to the effect of from three to nine low-ability evaluators. Especially in the case of the schoolchild, where the desire may well be to raise his self-evaluation, our estimates indicate that it could require uniformly positive evaluations from nine separate low-ability evaluators to equal the effects of a single high-ability evaluator. Of course, the empirical probability that nine individuals will give uniformly positive evaluations of any individual's performance is likely to be rather low.

In summary, we have proposed and tested six alternative models of ways in which individuals combine information from potential sources. The model which did the best job of predicting results of independent experiments was the simple additive model, and this model has the additional advantage of being conceptually the simplest. Two of our models are elaborations of ideas presented in Berger and Fisek (1970), and our results may be considered an independent test and partial confirmation of their conclusions. Some consequences of the model and of the parameter values determined in this research were considered; among them, that the practice of substituting multiple sources of ambiguous evaluation for a single source of reliable evaluation appears to be unsound, and that there is no evidence

in our data of the frequently postulated tendency to maximize the evaluation of one's own abilities.

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Extensions of the Mover-Stayer Model¹

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A major drawback to the traditional Markov formulation of mobility processes is that it assumes population homogeneity with respect to transition behavior. This assumption is clearly violated in most instances of social mobility. In an attempt to relax the homogeneity requirement and still retain the essential character of a Markov process, Blumen, Kogan, and McCarthy (1955) developed the "mover-stayer" model in which heterogeneity is attributed to the presence of two types of persons who differ in their rates of movement. In the present paper, the mover-stayer model is generalized to permit a continuous distribution of persons by rate of mobility. The model is illustrated with simulated data and then applied to an analysis of interregional migration.

INTRODUCTION

Applications of Markov processes to the study of social mobility have commonly concluded with the observation that individuals differ in their transition behavior. Although the Markov model requires population homogeneity, transitions from an origin state rarely conform to this assumption. Some persons simply move more often than, or differently from, others. This has been found with industrial mobility data (Blumen, Kogan, and McCarthy 1955), with intergenerational and intragenerational occupational mobility (Hodge 1966; Lieberman and Fuguitt 1967), and with geographic migration (Rogers 1966; Tarver and Gurley 1965).

The main difficulty derives from the Markov model having been constructed with repeated state changes by a single object in mind. In the analysis of social mobility, however, the movements of an entire population are at issue. If this population is heterogeneous in its transition behavior, then even if each individual were to satisfy the central assumption of a first-order Markov process—namely, that his probabilities of making particular transitions are determined solely by his present state and are independent of past history—the population-level process would not be Markovian.

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Attempts to relax the homogeneity requirement while retaining the essential Markov framework have led to research in two directions. In one approach, interest has focused on the construction of subpopulation matrices and on ways to categorize individuals that would permit the "within-category" variation in transition behavior to be reduced. Operationally, this has usually meant disaggregating the population on attributes which are expected, either from theoretical considerations or empirical investigation, to relate to mobility and to construct a separate transition matrix for each subpopulation. For example, Rogers (1966) and Tarver and Gurley (1965), analyzing geographic migration, disaggregate the population to produce transition arrays by age categories and race. In this spirit, McFarland (1970) has reported an analytic method for combining subpopulation or individual-level transition matrices and projecting from these to the k -step population matrix, and I have (Spilerman 1972) presented a regression procedure for disaggregating the population matrix in order to obtain the individual-level transition arrays.

The above strategy casts the problem of heterogeneity into a framework in which each person is viewed as making a single transition during a unit time interval, but following a matrix relevant to the subpopulation that shares his particular attributes. A conceptually different approach to heterogeneity is embodied in an alternative assumption, namely, that all individuals move according to an identical transition matrix when they move but differ in their *rates* of mobility (see Spilerman [1972] for a discussion on the convergence of these two perspectives). Work in this direction has resulted in the development of the "mover-stayer" model (Blumen, Kogan, and McCarthy 1955). Under the specifications of this process, heterogeneity is handled by postulating two types of individuals—stayers, who remain permanently in their states of origin, and movers, who are homogeneous in their transition behavior and therefore follow a Markov process with a common transition matrix. Several estimation methods for the parameters of the mover-stayer model have been developed by Goodman (1961).

Aside from the novel conceptual perspective provided by this model, which seems appropriate to the analysis of geographic migration or intra-generational occupational mobility where repeated moves can be made by a person, it has the advantage of not requiring individual-level attribute data (although parameter estimation can be improved if such information on the waiting time to transition is available [Goodman 1961]). Since much of our mobility data lacks significant detail at the individual level, the mover-stayer model can be applied where the construction of subpopulation transition matrices is not possible.

Although the mover-stayer model postulates two types of persons, this is done out of necessity for keeping the process mathematically tractable,

not because the authors genuinely believed that instances of population heterogeneity can generally be attributed to two types of persons. In fact, in their concluding chapter, Blumen, Kogan, and McCarthy (BKM) (1955) discuss strategies for extending the mover-stayer model to incorporate a wider range of heterogeneity in the rate of transition, although they do not develop such a generalization. An extension of the mover-stayer model in which the rate of individual mobility is specified by a continuous distribution is constructed in this paper. Following the mathematical presentation, the extension is applied to regional migration data.

THE MOVER-STAYER MODEL AND BKM'S COMMENTS ON GENERALIZATION

The Mover-Stayer Model

In their study of industrial mobility, BKM (1955, p. 62) report that calculations of k -step transition matrices from a Markov chain consistently underpredict the main diagonal elements of the observed k -step matrix. That is, if

$$P(1) = \begin{bmatrix} p_{11} & \cdot & \cdot & \cdot & p_{1m} \\ \cdot & & & & \cdot \\ \cdot & & & & \cdot \\ \cdot & & & & \cdot \\ p_{m1} & \cdot & \cdot & \cdot & p_{mm} \end{bmatrix}$$

is the observed one-step transition matrix, and

$$P(k) = \begin{bmatrix} p_{11}^{(k)} & \cdot & \cdot & \cdot & p_{1m}^{(k)} \\ \cdot & & & & \cdot \\ \cdot & & & & \cdot \\ \cdot & & & & \cdot \\ p_{m1}^{(k)} & \cdot & \cdot & \cdot & p_{mm}^{(k)} \end{bmatrix}$$

is the observed k -step transition matrix, then the k -step matrix predicted from a stationary Markov process, $\hat{P}(k) = P(1)^k$, will have main diagonal elements $(\hat{p}_{11}^{(k)} \dots \hat{p}_{mm}^{(k)})$, which commonly have the property that $\hat{p}_{ii}^{(k)} < p_{ii}^{(k)}$ for $i = 1, \dots, m$.

Although one might suspect that overtime change in the p_{ij} elements of $P(1)$ is responsible for this result, this is generally not the case. For example, Hodge (1966) reports similar findings with occupational mobility data, even though the $P(1)$ matrices he uses are time dependent. More formally, Hodge's analysis shows that if $P_t(1)$ for $t = 1, \dots, k$ are *observed* one-step transition matrices for successive time intervals, and if $\hat{\hat{P}}(k)$ is the predicted k -step array,

$$\hat{P}(k) = \prod_{t=1}^k P_t(1),$$

then the relationship between the main diagonal elements of $\hat{P}(k)$ and $P(k)$ may have the same structure as that described between $\hat{P}(k)$ and $P(k)$. The problem is not one of the p_{ij} elements of $P_t(1)$ changing over time for the population but, rather, that some persons are less apt to move than others in each time interval.

To contend with this situation, BKM suggest decomposing the population into movers and stayers,

$$P(1) = S + (I - S)M, \quad (1)$$

where S is a diagonal matrix containing as entries the proportion of persons in each origin state who remain there permanently, $I - S$ is a diagonal matrix with entries which indicate the proportion in a state who are potentially mobile, and M is the transition matrix for mobile individuals. The assumptions of the mover-stayer model, then, are (a) a proportion of the population in each state that never moves, (b) the population which is mobile is homogeneous in its pattern of movement and follows a Markov process, and (c) the process is stationary. We therefore have for the predicted k -step matrix,

$$\hat{P}(k) = S + (I - S)M^k.$$

Follow-up work on this model (Goodman 1961) has been concerned primarily with deriving consistent estimators for the matrices S and M and testing hypotheses relating to the mover-stayer process. Conceptually, however, there is a need to develop models which incorporate a greater range of heterogeneity. Instead of postulating two types of persons, we should like a process which handles several types and, ideally, a continuous range of individual differences in the rate of movement. Blumen, Kogan, and McCarthy address this problem in their concluding chapter (1955, pp. 138-46). Since the extension developed here proceeds from their suggestion, I first present their remarks.

BKM's Comments on Extending the Mover-Stayer Model

Instead of requiring every person to make a fixed number of transitions in each time interval, we assume that transitions are random occurrences²

² A transition from state i to state i would be considered movement in this terminology. For example, if the states of the system were geographic regions, an $i \rightarrow i$ transition would represent *intraregional* movement. Alternatively, we might speak of the expected number of *exposures* to movement as do BKM (1955, p. 139) but allow an individual to not move (an $i \rightarrow i$ transition) at an exposure.

and that the rate of movement by an individual refers to his *expected* number of transitions, not to the actual number. This is conceptually reasonable, since an individual with rate equal to, say, three moves per unit time interval will not necessarily make this exact number of transitions in every time unit. He may make zero or one moves in some intervals, four or five in others. Over a long time period he will nevertheless average three moves per time unit. A formal way of stating this is to assume that individuals move in accordance with a Poisson process³ with parameter value (expected number of moves per unit of time) $\lambda = 3$.

Consider for the moment only individuals with a common rate of movement equal to λ . The $P(1)$ matrix would then be given by

$$P(1) = \sum_{v=0}^{\infty} r_v(1)M^v, \quad (2)$$

where $r_v(t)$ is a Poisson probability,

$$r_v(t) = \frac{(\lambda t)^v e^{-\lambda t}}{v!}$$

(with $t = 1$ in equation 2), and indicates the proportion of individuals who are expected to make v transitions during the time interval $(0, t)$ from among those having a rate equal to λ , and M is the transition matrix followed at each move. We also assume that the $r_v(t)$ values are the same for all states of the process.

With the above specification, it is easy to show⁴ that $P(1) = e^{-\lambda[I-M]}$ for any matrix M . This result is important because the k -step transition matrix $P(k)$ is now given by

$$P(k) = e^{-\lambda k(I-M)} = [e^{-\lambda(I-M)}]^k = [P(1)]^k.$$

³ A precise specification of the (stationary) Poisson process is given by the following four assumptions: (1) λ is constant over time. (2) In an infinitesimal time interval Δt , at most one event can occur. (3) The probability of an event in Δt equals $\lambda \Delta t$; the probability of no event in Δt equals $1 - \lambda \Delta t$. (4) The occurrence of an event during $(t, t + \Delta t)$ is independent of the past behavior of the process. The derivation of the Poisson distribution from these assumptions is a straightforward procedure (see Feller 1957, p. 400).

⁴

$$\begin{aligned} P(t) &= \sum_{v=0}^{\infty} r_v(t)M^v = \sum_{v=0}^{\infty} \frac{(\lambda t)^v e^{-\lambda t}}{v!} M^v \\ &= e^{-\lambda t} \sum_{v=0}^{\infty} \frac{(\lambda t M)^v}{v!} = e^{-\lambda t} e^{\lambda t M} = e^{-\lambda t[I-M]}. \end{aligned}$$

The convergence of the infinite sum to $e^{\lambda t M}$ will hold for an arbitrary matrix M (Gantmacher 1959, p. 113).

Thus, relaxation of the fixed-number-of-moves assumption does not necessarily prevent the population-level process from being Markovian. In the particular case where transitions are Poisson events⁵ and the population is homogeneous in its transition rate, the Markov requirement will, in fact, be satisfied.

Now assume that we have g types of persons who differ in their rates of mobility. Each individual, however, follows the same M matrix when making a transition. If a proportion q_1 of the population moves with rate λ_1 , a proportion q_2 moves with rate λ_2 , etc., we could write separate equations identical to equation (2) for each subpopulation. Alternatively, let $r_v(t)$ equal the expected proportion of the *total* population which makes v transitions during the interval $(0, t)$, irrespective of the individual mobility rates. Then, combining the coefficients of M^v from the separate processes, we have

$$r_v(t) = \sum_{i=1}^g q_i \frac{(\lambda_i t)^v e^{-\lambda_i t}}{v!}. \quad (3)$$

If we generalize this result from g types of persons to sampling from a continuous distribution $f(\lambda)$, we obtain

$$r_v(t) = \int_0^{\infty} \frac{(\lambda t)^v e^{-\lambda t}}{v!} f(\lambda) d\lambda. \quad (4)$$

Equation (4) says that the expected proportion of individuals who make v transitions in the time interval $(0, t)$ equals the sum of the products of two quantities: (a) the proportion of individuals with rate equal to λ , and (b) the probability that an individual with rate λ will make v transitions. The summation is taken over all possible values of λ , which is assumed to have a continuous density function.

Blumen, Kogan, and McCarthy develop the generalization of the mover-stayer model to this point (as does Bartholomew [1967, pp. 27-37] in a recent review of mobility models). In the next section, we present a solution to the proposed extension, provide an estimation procedure for the parameters of the model, and discuss strategies for testing whether the assumptions of the model are met by data.

AN EXTENSION OF THE BASIC MODEL

Assumptions and the Derivation

In order to extend the mover-stayer model, it is necessary to specify a form for $f(\lambda)$ in equation (4). Since we have little a priori knowledge

⁵ In fact, the assumption that the occurrence of transitions follows a Poisson process leads to a *continuous-time* Markov formulation.

about this distribution, we assume a very general family of curves and use the observed data to estimate parameters for the specific distribution. We do, however, restrict $f(\lambda)$ to be unimodal or decline exponentially. This seems reasonable, since several studies of mobility (Goldstein 1964; Lipset and Bendix 1959, p. 158; Palmer 1954, p. 50; and Taeuber, Chiazze, and Haenszel 1968, p. 46) report distributions of persons by number of moves which have these forms.

Specifically, we assume that $f(\lambda)$ can be approximated by a gamma density,

$$f(\lambda) = \frac{\beta^\alpha}{\Gamma(\alpha)} \lambda^{\alpha-1} e^{-\beta\lambda} \quad \lambda > 0, \alpha > 0, \beta > 0, \quad (5)$$

where

$$\Gamma(\alpha) = \int_0^\infty y^{\alpha-1} e^{-y} dy.$$

The gamma distribution is a very general family of unimodal functions and is often assumed where the shape of the actual curve is unknown.

With this assumption regarding $f(\lambda)$, we obtain from equations (4) and (5) (see Chiang 1968, p. 49, for details on the integration)

$$\begin{aligned} r_v(t) &= \int_0^\infty \frac{(\lambda t)^v e^{-\lambda t}}{v!} \frac{\beta^\alpha}{\Gamma(\alpha)} \lambda^{\alpha-1} e^{-\beta\lambda} d\lambda \\ &= \frac{\Gamma(v+\alpha)}{v! \Gamma(\alpha)} \beta^\alpha t^v (\beta+t)^{-(v+\alpha)}. \end{aligned} \quad (6)$$

Using the relation $\Gamma(\alpha) = (\alpha-1)\Gamma(\alpha-1)$, this result may be written as

$$r_v(t) = \binom{\alpha+v-1}{v} \left(\frac{t}{\beta+t} \right)^v \left(\frac{\beta}{\beta+t} \right)^\alpha, \quad (7)$$

which is a negative binomial distribution. Thus, under the assumption that each individual's transitions follow a Poisson process, with the individual rates of mobility specified by a gamma density, the proportion of the population making v moves in $(0, t)$ will satisfy a negative binomial distribution.

Substituting this result into equation (2) yields for the one-step transition matrix,

$$P(1) = \sum_{v=0}^{\infty} r_v(1) M^v$$

$$= [p(t)]^{\alpha} \sum_{v=0}^{\infty} \binom{v + \alpha - 1}{v} [q(t)M]^v, \quad (8)$$

where $p(t) = \beta/(\beta + t)$, $q(t) = t/(\beta + t)$, and $t = 1$.

Conditions for a Closed Form Solution to $P(1)$

Recall that for X , a scalar, and any real number α , $(1 - X)^{-\alpha}$ has the binomial expansion

$$\begin{aligned} (1 - X)^{-\alpha} &= 1 + \alpha X + \frac{\alpha(\alpha + 1)}{2!} X^2 \\ &\quad + \frac{\alpha(\alpha + 1)(\alpha + 2)}{3!} X^3 + \dots \\ &= \sum_{v=0}^{\infty} \binom{v + \alpha - 1}{v} X^v, \quad (9) \end{aligned}$$

where the condition for convergence of the infinite sum is $|X| < 1$. By extension, we write for the infinite sum of matrices in equation (8),

$$\sum_{v=0}^{\infty} \binom{v + \alpha - 1}{v} [q(t)M]^v = [I - q(t)M]^{-\alpha}, \quad (10)$$

where I is the identity matrix. We now discuss the condition for convergence of equation (10) and the computation of the right side when α is an arbitrary real number, not necessarily integer valued.

Analogous to the condition on X in the scalar case (eq. 9), the requirement for convergence of the infinite matrix sum is that all eigenvalues of $q(t)M$ are less than one in absolute value (Gantmacher 1959, p. 113). Since M is a transition matrix, it is stochastic and its largest eigenvalue equals one. However, $q(t) = t/(\beta + t) < 1$ for finite t (since $\beta > 0$), and this ensures that the eigenvalues of $q(1)M$, in particular, will be strictly less than one.

Substituting the result of equation (10) into equation (8), we obtain for $P(1)$,

$$P(1) = [p(1)]^{\alpha} [I - q(1)M]^{-\alpha}. \quad (11)$$

A second consideration concerns the computation of $[I - q(t)M]^{-\alpha}$ when α is an arbitrary real number.⁶ For an integer k and a nonsingular

⁶ Since $[I - q(t)M]^{-\alpha} = ([I - q(t)M]^{-1})^{\alpha}$ and an inverse is defined only for a nonsingular array, this matrix power is defined only if the term in brackets is nonsingular. This condition, however, will be satisfied for $q(t) < 1$, i.e., for finite t .

matrix A , the matrix power A^k always exists. For an arbitrary real number α , we define A^α to be

$$A^\alpha = e^{\alpha \log A},$$

which will hold for nonsingular A (Gantmacher 1959, p. 240).

If the eigenvectors of A are linearly independent (which will be the case if A has distinct eigenvalues), then we can diagonalize A ,

$$A = HDH^{-1}, \quad (12)$$

where D is a diagonal matrix with the eigenvalues of A as entries, and H is a matrix containing the eigenvectors of A as columns.⁷ In this circumstance, $A^{-\alpha}$ can be written⁸ as

$$A^{-\alpha} = e^{-\alpha \log A} = He^{-\alpha \log D} H^{-1} = HD^{-\alpha} H^{-1}, \quad (13)$$

where $D^{-\alpha}$ is a diagonal matrix with elements of the form $\mu^{-\alpha}$. Consequently, letting $[I - q(1)M] = A$ in equation (11), $P(1)$ can be computed from M by the diagonalization procedure of equation (13).

Obtaining the M Matrix from $P(1)$

Equation (11) shows how we can obtain the population transition matrix if we know M , the individual level transition matrix, and α and β , the parameters of $f(\lambda)$. Unfortunately, our problem is generally the reverse of this situation. Given an observed population transition matrix $P(1)$, and estimates of α and β from the empirical distribution of the number of moves by an individual, we wish to obtain M so that equation (11), rewritten in general form, may be used to project to $P(t)$ for some $t > 1$.

No difficulty is encountered in solving equation (11) for M as long as the main diagonal elements of $P(1)$ are greater than one-half. We obtain

$$M = \frac{1}{q(1)} \{I - p(1)[P(1)]^{-1/\alpha}\}. \quad (14)$$

The requirement on the p_{ii} entries constitutes a sufficient (though not necessary) condition for ensuring the uniqueness of M .⁹ When this re-

⁷ If A does not have linearly independent eigenvectors equal in number to its order (which may be the case if the eigenvalues are not distinct), then A cannot be diagonalized. It can, however, be put in Jordan form (Bellman 1960, p. 191) which creates computational difficulties but, frequently, not theoretical ones. With real data, it is rare that the eigenvalues are not distinct, so only the case where $[I - q(t)M]$ can be diagonalized is considered in this paper.

⁸ The eigenvalues of D may be complex numbers, in which case $\log \mu = \log r + i\theta$, $-\pi < \theta \leq \pi$, where r and θ are the polar form components of the eigenvalue μ . To guarantee uniqueness the principal branch of the logarithm is used.

⁹ An analogous restriction is necessary to obtain the matrix of transition intensities,

quirement is not satisfied multiple solutions may exist to equation (14). In this situation, if individual histories are available, the researcher has an option of either reducing the real-time duration of the unit time interval so that the condition on $P(1)$ will be met, or calculating M directly from the observed transitions.

Because most mobility processes operate "slowly" relative to frequency of sampling, the restriction on the p_{ii} entries is not a particularly severe one, even in the absence of individual-level data. Where it is satisfied, M can be obtained by diagonalizing $P(1)$ in the manner discussed previously:

$$[P(1)]^{-1/\alpha} = K e^{-(1/\alpha) \log Q} K^{-1} = K Q^{-1/\alpha} K^{-1},$$

where Q is the eigenvalue matrix of $P(1)$, and the columns of K are the corresponding eigenvectors. Equation (14) therefore provides a method for estimating M from the population-transition matrix $P(1)$, under the assumption that population heterogeneity in the rate of movement can be specified by a gamma density.

The remaining parameters of the model, α and β of the gamma distribution, can be estimated directly from observed data on the number of moves by an individual. If \bar{v} and S_v^2 are the sample mean and variance of this variable, then estimates of α and β can be obtained in terms of these values from the mean and variance formulas for a negative binomial variate (Chiang 1968, p. 50). This yields

$$\left. \begin{aligned} \hat{\beta} &= \frac{\bar{v}}{S_v^2 - \bar{v}} \\ \hat{\alpha} &= \hat{\beta} \bar{v} \end{aligned} \right\} \quad (15)$$

Projection

Having computed M , we can now project forward in time to find $P(t)$, the t -step transition matrix

$$P(t) = \left(\frac{\beta}{\beta + t} \right)^\alpha \left[I - \left(\frac{t}{\beta + t} \right) M \right]^{-\alpha}. \quad (16)$$

This result will hold for finite t . For very large t , $q(t) \approx 1$ and $[I - q(t)M] \approx [I - M]$, which is singular since M is stochastic. Equation (16) is not defined in this circumstance, but other considerations (see Appendix A) suggest that $\lim P(t) = M(\infty)$, the equilibrium matrix for M , which may be found by the usual Markov methods.¹⁰

corresponding to a continuous-time Markov process, from a probability transition matrix (see Coleman 1964, p. 181). I am indebted to Burton Singer for an enlightening conversation on this subject.

¹⁰ This assumes an absence of $\lambda = 0$ individuals. If stayers are present, then $\lim P(t)$ is given by the mover-stayer formulation (BKM 1955, pp. 111-14), $\lim P(t) = S + (I - S)M(\infty)$.

If we view the process as embedded in continuous time so that noninteger values of t are meaningful, these can also be used with equation (16). We are not restricted to multiples of the initial time period, since it is the distribution of the population with respect to the expected number of transitions that is changing and this change is continuous. By any time t , of course, each person will have made an integer number of transitions. Likewise, no mathematical difficulty is presented with projecting backward in time. Starting with $P(1)$, for example, we can find $P(1/2)$.¹¹ This flexibility is useful because we can often obtain better estimates for α and β after a sizable number of moves have been made. The projections of the model could then be compared with transition matrices for shorter time intervals.

Testing the Model

At the outset, we assumed that the matrix $P(t)$ could be written in the form presented in equation (2), and consequently that $r_v(t)$, the proportion of the population expected to make v transitions by time t , is the same for all system states. Blumen, Kogan, and McCarthy (1955, p. 139) also make this assumption in their comments on extending the basic mover-stayer model. A necessary condition for this requirement to be satisfied is that the individual rates of transition not be a function of the state a person is in. Where data are available this assumption can be tested by computing the distribution of waiting times to a transition separately for individuals in each of the system states, and comparing these distributions.

There are two other assumptions of the model which can be tested—that individuals move in accordance with a Poisson process, and that the gamma density provides a reasonable approximation to the distribution of mobility rates in the population. To test one of these, we must, however, assume the validity of the other. If we assume that transitions are Poisson events, we can test the gamma specification by comparing the fit of the negative binomial estimates from equation (7) with the actual distribution of the number of moves. If the fit is poor, then the gamma assumption for $f(\lambda)$ in equation (5) should be altered and the Poisson compounded with a

¹¹ One referee suggested the following interpretation for this procedure. Consider an individual who is in state i at time 0, state j at time $1/2$, and state k at time 1. In arriving at equation (14), the information that his intermediate state is j was discarded; only the information that he made two moves from time 0 to time 1, beginning in state i and terminating in state k , is used. How, then, can $P(1/2)$ be reconstructed? The argument is that because of the assumption of an identical transition matrix for every person and a continuous distribution of mobility rates, there are other individuals who move just like him, only more slowly, who will be in state i at time 0, state j at time 1 , and the information they contribute to $P(1)$ serves as a proxy for the information he would have contributed to $P(1/2)$.

different curve.¹² Alternatively, repair work can be done on the gamma distribution (see discussion on the spiked gamma in the following section).

There are direct tests for whether the occurrence of events conforms to a Poisson process, such as on the interarrival times, but they assume an absence of heterogeneity. If a large number of interarrival times were available for each person, we could test the Poisson assumption separately for each individual without concern for the distribution of λ in the population. Most social data are not so rich in detail; consequently, it is necessary to assume the correctness of $f(\lambda)$ in order to test the Poisson assumption. Therefore, if we believe $f(\lambda)$ to be gamma, a comparison between the actual distribution of moves and the negative binomial estimates would provide a test of the Poisson specification. In practice, however, the form of $f(\lambda)$ would seem to be more problematic and the more interesting question.

AN EXAMPLE USING SIMULATED DATA

The advantage of illustrating the model with constructed data is that we have full knowledge of the actual mobility characteristics of the hypothetical population. We will assume an individual-level transition matrix and a population distribution by rate of movement as presented in table 1. In practice, this information usually would not be available.

TABLE 1
STRUCTURE OF THE SIMULATED DATA

| A. INDIVIDUAL-LEVEL TRANSITION MATRIX | B. DISTRIBUTION OF THE POPULATION BY RATE OF MOBILITY | |
|--|--|---|
| | λ | Proportion of the Population with This λ Value |
| $M = \begin{bmatrix} .600 & .200 & .100 & .100 \\ .150 & .700 & .100 & .050 \\ .100 & .100 & .750 & .050 \\ .050 & .050 & .100 & .800 \end{bmatrix}$ | 0.1 | 0.25 |
| | 1.0 | 0.35 |
| | 2.0 | 0.20 |
| | 3.0 | 0.10 |
| | 4.0 | 0.06 |
| | 5.0 | 0.04 |
| | | 1.00 |

We further assume that the six types of persons in the population (table 1, part B) move in accordance with a Poisson process which is specified by the indicated λ value for each subpopulation. Consequently, the Poisson

¹² If the fit is highly deficient, or if the assumption in the preceding paragraph is untenable, an individual-level transition matrix M which satisfies equation (8) may not exist. In this case, a nonstochastic M will be obtained from equation (14).

distribution was used to generate an expected proportion of each subpopulation who make $v = 0, 1, 2, \dots$ moves during the time interval (0,1). These values, multiplied by the respective subpopulation proportions in the total population, were aggregated to produce a distribution of the total population by number of moves. This distribution is presented in column 1 of table 2. Each of the $r_v(1)$ values has been multiplied by 1,000

TABLE 2
DISTRIBUTION OF NUMBER OF MOVES FROM OBSERVED
(SIMULATED) DATA AND FROM NEGATIVE
BINOMIAL ESTIMATES

| v | [1000 $r_v(1)$] | [1000 $\hat{r}_v(1)$] |
|---------------------|---|--|
| Number of Moves | Number of Persons with v Moves (Observed Data) (1) | Number of Persons with v Moves (Calculated from Negative Binomial $\alpha = 1.371, \beta = .915$) (2) |
| 0 | 388 | 363 |
| 1 | 226 | 260 |
| 2 | 153 | 161 |
| 3 | 97 | 94 |
| 4 | 59 | 54 |
| 5 | 34 | 30 |
| 6 | 19 | 17 |
| 7 | 11 | 9 |
| 8 | 6 | 5 |
| 9 | 2 | 3 |
| 10 | 1 | 1 |
| 1,000 $\sum r_v(1)$ | 996* | 997* |

NOTE.— $\bar{v} = 1.498$; $S_v^2 = 3.133$.

* Value is less than 1,000 because of rounding error.

so we can refer to the number of persons making a specified number of moves. These "observed" data were then used with the equation

$$P(1) = \sum_{v=0}^{10} r_v(1)M^v$$

to generate an "observed" transition matrix $P(1)$. This process was repeated¹³ for the intervals (0,3) and (0,6), so we have three observed transition matrices: $P(1)$, $P(3)$, and $P(6)$. The matrices produced by this construction are presented in table 3. Normally, these transition arrays and the distribution of the population by number of moves (table 2, col. 1)

¹³ The upper limit on the summation was set to 30 for $t = 3$ and to 60 for $t = 6$.

TABLE 3
OBSERVED TRANSITION MATRICES FOR THE
TIME INTERVALS (0, 1), (0, 3), AND (0, 6)

| | |
|----------|--|
| $P(1) =$ | $\begin{bmatrix} .650 & .156 & .101 & .093 \\ .118 & .719 & .101 & .063 \\ .090 & .102 & .747 & .061 \\ .056 & .064 & .101 & .779 \end{bmatrix}$ |
| $P(3) =$ | $\begin{bmatrix} .448 & .218 & .178 & .155 \\ .165 & .529 & .178 & .128 \\ .143 & .177 & .555 & .125 \\ .110 & .135 & .178 & .577 \end{bmatrix}$ |
| $P(6) =$ | $\begin{bmatrix} .365 & .235 & .215 & .184 \\ .177 & .441 & .215 & .166 \\ .164 & .210 & .462 & .164 \\ .141 & .181 & .215 & .463 \end{bmatrix}$ |

TABLE 4
POPULATION TRANSITION MATRICES PREDICTED FROM A
STATIONARY MARKOV PROCESS

| | |
|---------------------------|--|
| $\hat{P}(1) = P(1) =$ | $\begin{bmatrix} .650 & .155 & .101 & .093 \\ .118 & .719 & .101 & .063 \\ .090 & .102 & .747 & .061 \\ .056 & .064 & .101 & .779 \end{bmatrix}$ |
| $\hat{P}(3) = [P(1)]^3 =$ | $\begin{bmatrix} .346 & .261 & .209 & .183 \\ .199 & .446 & .209 & .147 \\ .170 & .209 & .478 & .143 \\ .126 & .154 & .209 & .511 \end{bmatrix}$ |
| $\hat{P}(6) = [P(1)]^6 =$ | $\begin{bmatrix} .231 & .280 & .264 & .224 \\ .211 & .317 & .264 & .207 \\ .200 & .260 & .337 & .203 \\ .174 & .224 & .264 & .336 \end{bmatrix}$ |

are the kinds of data which can be collected.¹⁴ We now assume that only this information is available.

Projections from a stationary Markov chain require that the observed $P(1)$ matrix be raised to the requisite power. Markov estimates of $P(3)$, $\hat{P}(3) = [P(1)]^3$, and of $P(6)$, $\hat{P}(6) = [P(1)]^6$, are presented in table 4. As a result of the heterogeneity in rate of movement which was built into the data the main diagonal elements from this projection are, as expected,

¹⁴ We also have available the distribution of $r_v(t)$ for $t = 3, 6$, analogous to col. 1 of table 2. This information will not be used in the illustration, but a test of the Poisson assumptions could also be based upon the change over time in the distribution.

consistently smaller than the corresponding observed values reported in table 3. Moreover, the discrepancy increases over time.

Turning to the model proposed in this paper, \bar{v} and S_v^2 (from table 2, col. 1) were first used with equation (15) to estimate α and β : $\hat{\alpha} = 1.371$, $\hat{\beta} = 0.915$. Having estimated these parameters, the negative binomial formula (eq. 7) can be used to generate a predicted distribution of moves. This distribution is presented in column 2 of table 2, alongside the observed values. While there are some sizable deviations between expected and observed figures, this method will usually produce a superior fit than simply dichotomizing the population into stayers and movers,¹⁵ especially when the heterogeneity is considerable.

The estimates of α and β , together with the observed $P(1)$ matrix from table 3, allow the M matrix to be derived using equation (14),

$$\hat{M} = \begin{bmatrix} .613 & .191 & .099 & .098 \\ .144 & .707 & .099 & .050 \\ .097 & .099 & .754 & .050 \\ .050 & .050 & .099 & .801 \end{bmatrix}$$

This array is an estimate of the individual-level transition matrix M which, by the assumptions of the model, is the same for all persons. Equation (16) can now be used with \hat{M} , $\hat{\alpha}$, and $\hat{\beta}$ to project to $P(t)$ for any value of t . Estimates of $P(1)$, $P(3)$, and $P(6)$ are presented in table 5.

A comparison of these predicted arrays with the observed transition matrices (table 3) and with the Markov projections (table 4) reveals the superiority of the present model. The main diagonal entries, in particular, decline less rapidly than in the Markov projections.¹⁶ The deviations from observed values, incidentally, cannot be attributed to any inadequacy with the Poisson assumptions, since these were used to generate the data. Rather, the deviations result from an inability of the gamma density to fit perfectly the constructed distribution of λ values in the population, although the discrepancy is not severe. (Compare the negative binomial estimates with the observed distribution of moves in table 2). In fact, using the computed values of α and β , the gamma density $f(\lambda)$ for this population can be drawn directly from equation (5). This graph is presented in figure 1. Superimposed on the curve are vertical lines which indicate the points of

¹⁵ Nevertheless, the inadequacy of the mover-stayer dichotomization for data analysis can be less severe than would at first appear. As BKM (1955, p. 142) point out, it is not the case that all movers need make a single transition during a time unit, only that they follow a Poisson process with a common λ value. A generalization of the mover-stayer model in which permanent stayers are permitted, as in the original formulation, is discussed in conjunction with the "spiked gamma" (with vodka, please).

¹⁶ Estimates of the main diagonal entries from the present model converge to the equilibrium values predicted by the Markov model. At any finite t , however, estimates from the model herein are greater than the corresponding Markov values.

TABLE 5

POPULATION-TRANSITION MATRICES PREDICTED
FROM THE GENERALIZED MOVER-STAYER MODEL*

$$\hat{P}_{(1)} = \begin{bmatrix} .650 & .156 & .101 & .093 \\ .118 & .719 & .101 & .063 \\ .090 & .102 & .747 & .061 \\ .056 & .064 & .101 & .779 \end{bmatrix}$$

$$\hat{P}_{(3)} = \begin{bmatrix} .423 & .232 & .184 & .162 \\ .175 & .510 & .184 & .131 \\ .150 & .184 & .539 & .127 \\ .113 & .138 & .184 & .565 \end{bmatrix}$$

$$\hat{P}_{(6)} = \begin{bmatrix} .319 & .256 & .229 & .197 \\ .193 & .404 & .229 & .174 \\ .176 & .224 & .428 & .171 \\ .148 & .187 & .229 & .434 \end{bmatrix}$$

* Estimates are from equation (16).

concentration of the simulated data (from table 1, part B). It is apparent that the heterogeneity in the population is reasonably well represented by this gamma density, although many real social processes will actually permit a better fit than the arbitrary distribution constructed here (e.g. Spilerman 1970, table 4).

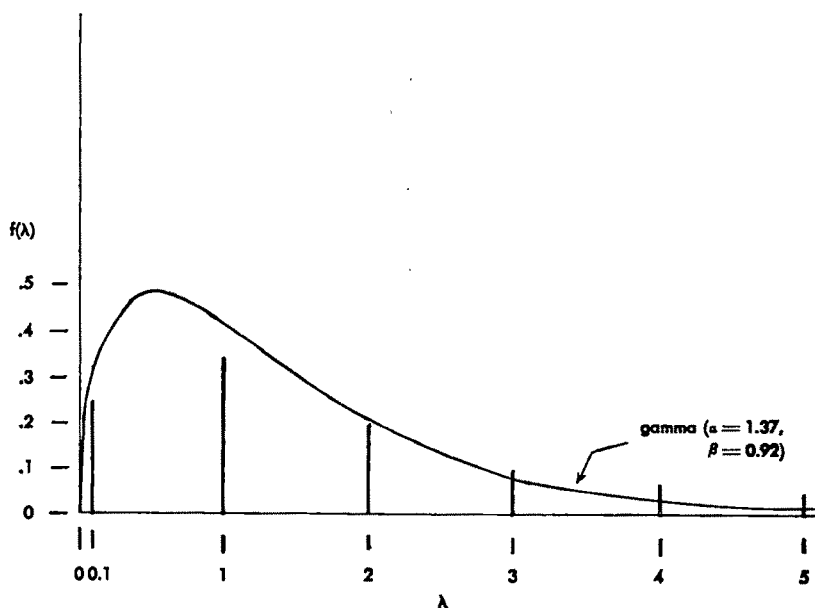


FIG. 1.—Distribution of the population by expected rate of movement, from simulated data.

Spiked Gamma

If the comparison between the negative binomial estimates and the actual distribution of the population by number of moves is less than satisfactory (as judged, for example, by a χ^2 test), the best recourse would be to compound the Poisson with a different family of curves which might allow a more adequate fit to $f(\lambda)$. However, if the gamma assumption fails in the way it is most likely to, by a very heavy concentration of nonmovers, this defect can be repaired by use of the "spiked gamma."

The spiked gamma is a direct generalization of the mover-stayer model, since, unlike the previous extension, the presence of permanent stayers ($\lambda = 0$ persons) is permitted. It is an extension of the mover-stayer model in that heterogeneity is allowed among the movers, who are assumed to be distributed by rate of movement according to a gamma density.¹⁷ One procedure for estimating the parameters of this process would be to first apply a mover-stayer method to separate out stayers from the remainder of the population, then treat movers according to the present model. In fact, having removed stayers, a quick test of the need to even use the model of this paper, rather than the simpler Markov methods, can be obtained by comparing the mean and variance of the distribution of moves. For a Poisson distribution, $\text{var}(v) = E(v)$, while, if heterogeneity is present in the distribution of λ , $\text{var}(v) > E(v)$. Thus, by comparing \bar{v} and S_v^2 , the likely adequacy of a Poisson (mover-stayer) assumption for the movers can be ascertained. (See Spilerman [1970, p. 633] for a lengthier discussion and application of this point.)

An alternate procedure for estimating the size of the spike would be to assume that a gamma density provides the correct distribution of movers and choose that division of the population failing to move which minimizes the deviations of the observed values from the expected distribution of moves. Thus, we would use the gamma to estimate the number of *movers* who happen to make zero transitions during the interval (0,1). The advantage of this approach is that it will allow a "best fit" of the gamma to the distribution of movers who make $v \geq 1$ moves to be obtained, since the term for $v = 0$ does not influence the parameter estimates.

The procedure here is to fit a negative binomial to the observed distribution of moves (table 2, col. 1) except that information about the $v = 0$ term is not used. The "truncated" negative binomial¹⁸ must be employed to

¹⁷ In the BKM mover-stayer model, this gamma specification for movers is replaced by the more restrictive assumption that they are concentrated at a single λ point.

¹⁸ An analogous procedure, using the truncated Poisson distribution (Coleman 1964, p. 366), can be used to estimate the parameters of the BKM mover-stayer model.

estimate α and β when the zero term of the observed distribution is missing. This probability distribution is defined by the equation¹⁹

$$R_v = \frac{1}{1 - p^a} \left(\alpha + \frac{v-1}{v} \right) q^v p^a \quad (17)$$

for $v \geq 1$, where the two independent parameters α and q are estimated by (see Appendix B)

$$\hat{q} = 1 - \frac{\bar{v}(1 - R_1)}{S_v^2},$$

$$\hat{\alpha} = \frac{1}{\hat{q}} [\bar{v}(1 - \hat{q}) - R_1].$$

In these formulas, \bar{v} and S_v^2 are the sample mean and variance (with the $v = 0$ observation deleted from the computations), and R_1 is the proportion of the observed population with $v \geq 1$ moves who make a single transition. The remaining parameter of the gamma distribution, β , may be calculated from \hat{q} : $\hat{\beta} = (1/\hat{q}) - 1$.

These estimates of α and β are now used with the regular negative binomial formula (eq. 7) to estimate the number of *movers* who failed to move during the interval (0,1). The difference between these observed and calculated numbers, appropriately standardized,²⁰ provides an estimate for the size of the spike at $\lambda = 0$. Calculations using the alternate method were carried out with the data in column 1 of table 2. The results are presented in table 6, column 2, alongside the observed distribution. Because of the added degree of freedom in estimating the spiked distribution, the estimates are clearly superior to those obtained from the regular negative binomial.

With the size of the spike estimated by either method, we have a division of the population into stayers and movers and may project to $P(t)$:

$$P(t) = S + (I - S) \left(\frac{\beta}{\beta + t} \right)^a \left[I - \left(\frac{t}{\beta + t} \right) M \right]^{-a}. \quad (18)$$

In this equation, S is a diagonal matrix containing as entries the proportion

¹⁹ Equation (17) is not written as a function of time since only the values for $t = 1$ are considered here.

²⁰ The negative binomial estimates are standardized by forcing

$$\sum_1^{\infty} \hat{n}_v = \sum_1^{\infty} n_v,$$

where n_v is the observed number of persons making v transitions during the time interval (0, 1).

Extensions of the Mover-Stayer Model

TABLE 6

DISTRIBUTION OF NUMBER OF MOVES FROM OBSERVED DATA AND FROM NEGATIVE BINOMIAL, WITH PARAMETERS ESTIMATED FROM TRUNCATED NEGATIVE BINOMIAL

| v | $[1000 r_v(1)]$ | $[1000 \hat{r}_v(1)]$ |
|-----------------------|--|---|
| Number of Moves | Number of Persons with v Moves (Observed Data) (1) | Number of Persons with v Moves ($\alpha=2.231, \beta=1.251$ Calculated from Truncated Negative Binomial) (2) |
| 0 | 388 | 388* |
| 1 | 226 | 224 |
| 2 | 153 | 160 |
| 3 | 97 | 101 |
| 4 | 59 | 58 |
| 5 | 34 | 32 |
| 6 | 19 | 17 |
| 7 | 11 | 9 |
| 8 | 6 | 5 |
| 9 | 2 | 2 |
| 10 | 1 | 1 |
| 1,000 $\Sigma r_v(1)$ | 996† | 997† |

NOTE.— $\bar{v} = 2.448$; $S_v^2 = 2.776$; the $v = 0$ observation is excluded from these calculations.

* This value includes 162 persons in the spike.

† Value is less than 1,000 because of rounding error.

of the initial population in a state who are stayers;²¹ $I - S$ is a corresponding matrix for movers; and the remaining parameters are estimated as before, but now using only information on movers. The improved predictions for $\hat{P}(t)$, using equation (18), are presented in table 7.

GEOGRAPHIC MIGRATION

Using data made available by Karl Taeuber from his analysis of residential mobility in the United States (Taeuber, Chiazze, and Haenszel 1968), the model of this paper was applied to interregional transitions by males.²² The Taeuber data were collected in 1958 from retrospective reports about prior residences and are described in detail elsewhere (Taeuber, Chiazze, and Haenszel 1968). For the purpose of this study, four geographic regions were defined as states of the process: (1) Northeast, (2) North Central,

²¹ Estimates of the proportion of stayers in each state are obtained directly when the mover-stayer estimation procedures are used. With the alternate method, one assumes that stayers constitute an identical proportion of the nonmovers in each state.

²² A computer program which performs the calculations associated with this model has been written by David Dickens. Copies of the program may be obtained from University Microfilms, Ann Arbor, Michigan (request item S-404).

TABLE 7
TRANSITION MATRICES PREDICTED FROM
THE SPIKED GAMMA MODEL*

$$\hat{P}_{(1)} = \begin{bmatrix} .650 & .156 & .102 & .093 \\ .118 & .718 & .102 & .063 \\ .090 & .103 & .746 & .061 \\ .056 & .064 & .102 & .778 \end{bmatrix}$$

$$\hat{P}_{(3)} = \begin{bmatrix} .446 & .219 & .179 & .156 \\ .166 & .529 & .178 & .128 \\ .144 & .177 & .555 & .124 \\ .111 & .135 & .177 & .577 \end{bmatrix}$$

$$\hat{P}_{(6)} = \begin{bmatrix} .371 & .232 & .215 & .183 \\ .175 & .448 & .213 & .165 \\ .163 & .207 & .468 & .162 \\ .142 & .180 & .211 & .468 \end{bmatrix}$$

* Estimates are from equation (18).

(3) South, and (4) West. The time points that were used are $t_0 = 1937$, $t_1 = 1944$, $t_2 = 1951$, and $t_3 = 1958$. These were selected to provide residence histories for the adult years of this cohort.

The data proved to be less than ideal for illustrating the versatility of this model to incorporate a wide range of heterogeneity. One difficulty is that the histories were collected only for the four most recent residences of an individual and for his residence at birth. Persons who have had more than five addresses, therefore, have gaps in their residence histories and had to be excluded from the analysis. Unfortunately, this meant that persons with high rates of mobility were deleted and consequently the heterogeneity in proneness to move was being artificially reduced.

A second difficulty with these data stems from the little interregional migration which appears to take place (see table 8, part B). In part, this derives from the truncation of the distribution at $v = 4$ moves, since individuals with many residence changes are most likely to have had some regional migration experience. However, it is also a consequence of the phenomenon we are examining. Apparently, persons do not change geographic region very frequently during a seven-year interval; indeed, only one out of five residence changes resulted in a move to a different region, using this four-category definition of region.

Table 8 presents the observed transition matrix for the population during 1937-44 (part A) and the observed and predicted distributions of the population by number of moves for this period (part B). In light of the above comments, these data pertain only to persons who made four or fewer residence changes during 1937-58. Comparing the observed distribution of moves (part B, col. 1) with the distribution predicted from the negative

Extensions of the Mover-Stayer Model

TABLE 8

OBSERVED POPULATION TRANSITION MATRIX AND DISTRIBUTION OF MOVES FROM GEOGRAPHIC MIGRATION DATA, TOGETHER WITH DISTRIBUTION PREDICTED FROM NEGATIVE BINOMIAL

A. OBSERVED POPULATION TRANSITION MATRIX (1937-44)

| | | | | | n |
|----------|------|------|------|------|------|
| $P(1) =$ | .970 | .010 | .012 | .008 | 3437 |
| | .007 | .947 | .015 | .030 | 4160 |
| | .011 | .028 | .938 | .023 | 4110 |
| | .003 | .015 | .017 | .966 | 1341 |

B. DISTRIBUTION OF THE POPULATION BY NUMBER OF MOVES DURING 1937-44

| (v) | (n_v) | (\hat{n}_v) |
|-----------------|---------------------------------------|--|
| Number of Moves | Number of Persons with v Moves (1) | Number of Persons with v Moves (Calculated from Negative Binomial, $\alpha=1.771$, $\beta=6.382$) (2) |
| 0 | 10,120 | 10,082 |
| 1 | 2,328 | 2,419 |
| 2 | 507 | 454 |
| 3 | 93 | 77 |
| 4 | ... | 12 |
| 5 | * | 1 |
| Σn_v | 13,048 | 13,045 |

NOTE.— $\bar{v} = 0.278$; $S_v^2 = 0.321$.

* Number not available.

binomial (col. 2), it is evident that the fit is reasonably good, except at the tail end of the distribution. The difficulty at the tail probably results from a tendency to underreport moves when many were made. Remember that we are dealing here with recollections in 1958 of residences during 1937-44.

Using the $P(1)$ matrix from 1937-44 together with $\hat{\alpha}$ and $\hat{\beta}$ from column 2 of table 8, \hat{M} , the estimate of the individual-level transition matrix, was constructed using equation (14):

$$\hat{M} = \begin{bmatrix} .891 & .035 & .046 & .028 \\ .026 & .799 & .059 & .116 \\ .042 & .109 & .763 & .085 \\ .009 & .057 & .065 & .869 \end{bmatrix}$$

Matrix \hat{M} , therefore, indicates how individuals transfer each time they move. It shows, in particular, that the Northeast is most successful in retaining its residents *when they move*; the South, least successful. Using

the estimates of α , β , and M , we can now project to $P(t)$ for any time t . Ordinarily, equation (16) would be used for this purpose. However, to compensate for the effect of truncating the distribution, projection was accomplished using equation (8) with the upper limit of the sum set to four, the maximum number of moves by an individual retained for the analysis. As a result, negative binomial predictions of five or more moves have been deleted from the estimation of $P(t)$. The observed data, and projections obtained by using a Markov chain, as well as those by the present method, are presented in table 9 for the periods 1937-51 and 1937-58.

TABLE 9
OBSERVED AND PREDICTED TRANSITION MATRICES FOR 1937-51 AND 1937-58,
FROM GEOGRAPHIC MIGRATION DATA

| | $P(2)$ —(1937-51) | $P(3)$ —(1937-58) |
|--|--|--|
| Observed matrices | $\begin{bmatrix} .947 & .015 & .019 & .019 \\ .008 & .923 & .018 & .050 \\ .018 & .045 & .907 & .030 \\ .004 & .023 & .019 & .954 \end{bmatrix}$ | $\begin{bmatrix} .934 & .017 & .025 & .022 \\ .009 & .908 & .020 & .063 \\ .020 & .052 & .890 & .038 \\ .004 & .024 & .018 & .954 \end{bmatrix}$ |
| Projection from Markov process [$\hat{P}(t) = P(1)^t$] | $\begin{bmatrix} .942 & .019 & .023 & .016 \\ .014 & .898 & .030 & .059 \\ .021 & .053 & .881 & .044 \\ .005 & .029 & .032 & .933 \end{bmatrix}$ | $\begin{bmatrix} .915 & .028 & .034 & .024 \\ .020 & .853 & .043 & .085 \\ .031 & .076 & .829 & .064 \\ .008 & .043 & .047 & .904 \end{bmatrix}$ |
| Projection from present model* ($\alpha = 1.771$, $\beta = 6.382$) | $\begin{bmatrix} .946 & .017 & .021 & .016 \\ .009 & .926 & .023 & .042 \\ .018 & .033 & .912 & .037 \\ .008 & .036 & .023 & .932 \end{bmatrix}$ | $\begin{bmatrix} .924 & .024 & .028 & .023 \\ .012 & .897 & .032 & .059 \\ .024 & .046 & .878 & .052 \\ .001 & .050 & .032 & .906 \end{bmatrix}$ |

$$* \hat{P}(t) = \sum_{v=0}^4 \hat{p}(t) M^v.$$

By comparing the main diagonal entries, especially for the 1937-58 matrices, it is evident that the model of this paper produces a superior fit to the data than is obtained from the Markov projection, although the latter estimates are themselves not poor. One reason, incidentally, why the predictions from both models are not better is because the stationarity requirement is violated. We are dealing here with a cohort through a 21-year period and extrapolating to a terminal year in which the population is 14 years older than at termination of the period used for parameter estimation. Thus, if age has an effect on migration behavior, as it surely does (e.g., Morrison 1967, pp. 558-59), we have a transition matrix which is changing over time. By comparison, BKM did not have this concern, since their data covered only a three-year period.

While the migration data preclude our demonstrating the suitability of

this model in situations of considerable population heterogeneity, we can investigate the reason why the Markov estimates are reasonably good. We are assuming here that transitions are Poisson events in which λ , the parameter of the distribution, varies over individuals. In this circumstance, an estimate of the variance of λ is given by $\hat{\sigma}_\lambda^2 = S_v^2 - \bar{v}$. Substituting the values of S_v^2 and \bar{v} from table 8, we obtain $\hat{\sigma}_\lambda^2 = .043$, a value which is not very large. (By comparison, $\hat{\sigma}_\lambda^2 = 1.635$ for the simulated data in table 2.) Thus, primarily because of the deletion of individuals with more than four moves, little heterogeneity remains in the population, and the Markov chain model which formally requires all persons to have an identical parameter value now provides reasonably good projections. In fact, having estimated α and β , we can graph $f(\lambda)$ to ascertain the appearance of the heterogeneity in λ . This graph is presented as figure 2. Note that the scale

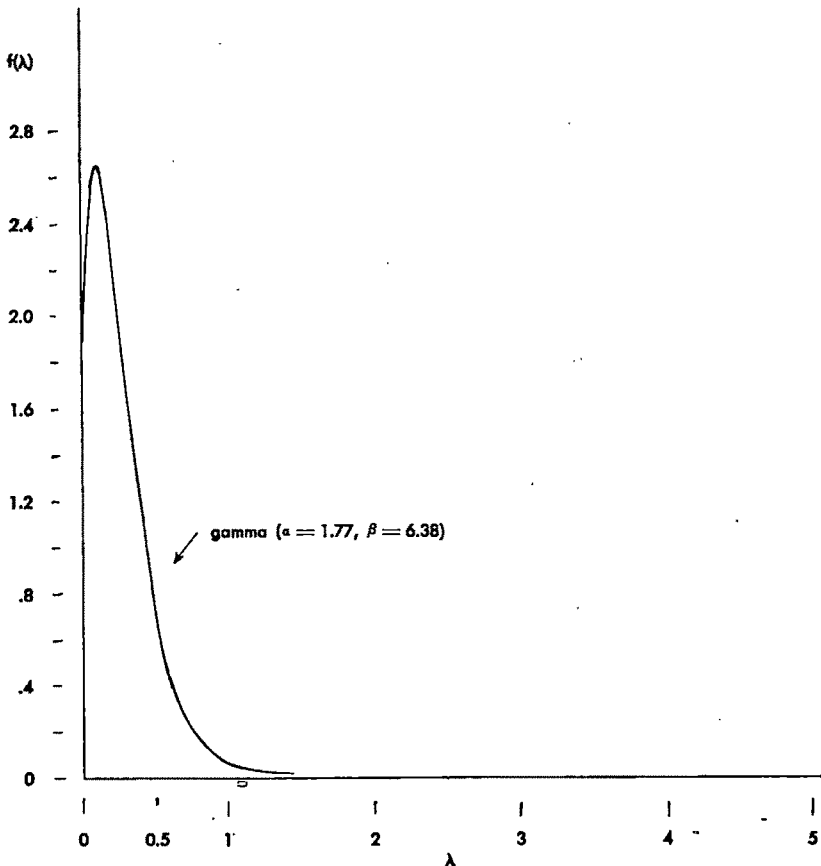


FIG. 2.—Distribution of the population by expected rate of movement, from geographic migration data.

unit on the Y -axis is one-half the size of the corresponding unit in figure 1. It is evident that the population is indeed highly concentrated over a narrow range of λ values.

CONCLUSIONS

The extensions developed in this paper, like the original mover-stayer model, cast the burden of explaining heterogeneity onto variations in the rate of mobility, since, by assumption, all persons follow an identical transition matrix at each move. If individual-level data on social characteristics are available, they can be used to determine the components of heterogeneity. Elsewhere (Spilerman 1970, pp. 646-48), I have argued that a regression methodology, in which the number of moves made by an individual is the dependent variable, is both consistent with this formulation and provides an approximation to analyzing the λ values themselves in terms of the independent variables.

While the emphasis here has been on the analysis of a stationary process, these methods will also shed light on the structure of time-varying processes. Data at two consecutive time points are required for parameter estimation in this model. Therefore, if the parameters are recalculated for adjacent time intervals of a time-varying process, we can ascertain whether the nonstationarity is primarily attributable to changes in the M matrix, which would suggest an alteration in the manner of selecting destination states at a transition, or to change in the gamma distribution, which would be indicative of a shift in the rate at which individuals are making transitions.

It is well known that the negative binomial distribution can be derived from an assumption of positive reinforcement (Coleman 1964, p. 300) as well as from this heterogeneity model. In the context of geographic migration, reinforcement would mean that, with each move, an individual's probability of making a subsequent transition is increased. Although this conceptualization seems forced, it becomes more plausible in an alternate formulation. Making a statement about the process by which moves occur is equivalent to making an assumption about the distribution of durations between the moves. Viewed from the latter perspective, reinforcement would suggest that the longer an individual resides at a particular location, the higher is his probability of remaining. Thus, the reinforcement hypothesis is recognizable as the "Axiom of Cumulative Inertia" in the Cornell Mobility Model (McGinnis 1968).

How do we distinguish between heterogeneity and reinforcement? Conceptually, one or the other is likely to be more appropriate to a particular phenomenon. For example, as McFarland (1970) has pointed out, the assumption that attachments grow over time seems more reasonable for geographic mobility than for occupational mobility. Analytically, it may

be possible to distinguish between these alternative processes by examining the change over time in the distribution of moves in successive time units. The reinforcement model suggests that the variance of this distribution should increase for a cohort as some individuals become increasingly prone to move. By contrast, the heterogeneity explanation suggests that the variance should remain constant. Nevertheless, in many social processes both phenomena probably occur, and individual-level attribute data would seem necessary in order to disentangle their separate effects.

APPENDIX A

$\lim_{t \rightarrow \infty} P(t) = M(\infty)$, The Equilibrium Matrix for M

PROOF

We assume M is ergodic and therefore

$$\lim_{v \rightarrow \infty} M^v = M(\infty)$$

exists (Feller 1957, p. 356). By definition,

$$P(t) = \sum_{v=0}^{\infty} r_v(t) M^v,$$

where $r_v(t)$ is specified by equation (7). For each v ,

$$\lim_{t \rightarrow \infty} r_v(t) = \lim_{t \rightarrow \infty} \left(\alpha + \frac{v-1}{v} \right) \left(\frac{t}{\beta + t} \right)^v \left(\frac{\beta}{\beta + t} \right)^a = 0$$

since $\frac{\beta}{\beta + t} \rightarrow 0$. This implies that for $\epsilon > 0$ and L , an arbitrary integer, there exists a T such that for $t > T$,

$$\sum_{v=0}^{L-1} r_v(t) < \epsilon/2. \quad (\text{A1})$$

Since $\lim_{v \rightarrow \infty} M^v = M(\infty)$, we choose L such that for $v > L$

$$\max_{i,j} \left| m_{ij}^{(v)} - m_{ij}(\infty) \right| < \epsilon/2, \quad (\text{A2})$$

where $m_{ij}^{(v)}$ is the (i,j) entry of M^v . Then, for $t > T$,

$$\max_{i,j} \left| p_{ij}(t) - m_{ij}(\infty) \right| = \max_{i,j} \left| \sum_{v=0}^{\infty} r_v(t) m_{ij}^{(v)} - \sum_{v=0}^{\infty} r_v(t) m_{ij}(\infty) \right|,$$

where $p_{ij}(t)$ is the (i,j) entry of $P(t)$, and the last sum follows from the relation,

$$\sum_{v=0}^{\infty} r_v(t) = 1.$$

Regrouping terms,

$$\begin{aligned} & \max_{i,j} \left| \sum_0^{L-1} r_v(t) [m_{ij}^{(v)} - m_{ij}(\infty)] + \sum_L^{\infty} r_v(t) [m_{ij}^{(v)} - m_{ij}(\infty)] \right| \\ & \leq \sum_0^{L-1} r_v(t) \max_{i,j} |m_{ij}^{(v)} - m_{ij}(\infty)| + \sum_L^{\infty} r_v(t) \max_{i,j} |m_{ij}^{(v)} - m_{ij}(\infty)| \\ & \leq \sum_0^{L-1} r_v(t) \cdot 1 + \frac{\epsilon}{2} \sum_L^{\infty} r_v(t), \end{aligned}$$

since, in the middle line, the maximum in the first summation is less than or equal to one because the matrices are stochastic, and the second maximum has been reduced using (A2). Using (A1) with the first sum in the final inequality and noting that

$$\sum_L^{\infty} r_v(t) \leq 1$$

in the second sum, we conclude that

$$\max_{i,j} |p_{ij}(t) - m_{ij}(\infty)| < \epsilon,$$

and therefore

$$\lim_{t \rightarrow \infty} P(t) = M(\infty).$$

APPENDIX B

The Truncated Negative Binomial Distribution

Let P_v be the negative binomial probability for v events:

$$P_v = \binom{\alpha + v - 1}{v} q^v p^\alpha, \quad v = 0, 1, 2, \dots \quad (\text{B1})$$

For any positive integer-valued distribution, we have

$$1 = \sum_0^{\infty} P_v = P_0 + \sum_1^{\infty} P_v,$$

and therefore,

$$\frac{1}{1 - P_0} \sum_{v=1}^{\infty} P_v = 1.$$

This is a truncated distribution in that the P_0 term is lacking. For the negative binomial distribution, we obtain

$$R_v = \frac{1}{1 - P_0} P_v = \frac{1}{1 - p^a} \binom{\alpha + v - 1}{v} q^v p^a, \quad v \geq 1, \quad (\text{B2})$$

which is the truncated negative binomial distribution. Note for reference that

$$R_v = \frac{\alpha + v - 1}{v} q R_{v-1}. \quad (\text{B3})$$

TO OBTAIN α

$$\begin{aligned} \mu &= \sum_{v=1}^{\infty} v R_v = R_1 + \sum_{v=2}^{\infty} v R_v \\ &= R_1 + \sum_{v=2}^{\infty} (\alpha + v - 1) q R_{v-1} \quad (\text{by [B3]}) \\ &= R_1 + q \sum_{v=1}^{\infty} (\alpha + v) R_v \\ &= R_1 + q\alpha + q\mu, \end{aligned} \quad (\text{B4})$$

and $\alpha = (1/q)(\mu p - R_1)$.

TO OBTAIN q, β

$$\begin{aligned} \text{var}(v) &= \sum_{v=1}^{\infty} (v - \mu)^2 R_v \\ &= R_1 - \mu^2 + \sum_{v=2}^{\infty} v^2 R_v. \end{aligned}$$

Using (B3), we obtain for the last term,

$$\sum_{v=2}^{\infty} v^2 R_v = \frac{q}{1 - q} [\alpha\mu + \alpha + \mu + R_1].$$

Substituting this value into the expression for $\text{var}(v)$, and using (B4) to simplify, yields

$$(1 - q) \text{var}(v) = \mu(1 - R_1),$$

and

$$q = 1 - \frac{\mu(1 - R_1)}{\text{var}(v)}.$$

Finally, since $q = 1/(\beta + 1)$ (from the definition of $q(1)$, eq. [8] in text), we obtain for β , $\beta = (1/q) - 1$.

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Some Problems in the Measurement of Class Voting¹

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The relationship between class and voting has been described using concepts such as class voting, political distinctiveness of classes, class distinctiveness of parties, and class solidarity. Measures of these concepts are shown to rely on the same independent information. The Alford index of class voting can be interpreted as a regression coefficient. A model is developed to analyze under what conditions the index of class voting is affected by different ways of classifying occupational groups. The Gini index of segregation is considered as an alternative measure of the relationship between class and voting. Empirical examples are discussed.

The concept of class voting focuses on the relationship between class position and voting behavior, attempting to describe the nature of the relationship between the position of voters in the stratification system and their party preferences. This concept is thus of central importance to political sociology, where parties often are viewed as representing social classes, and elections sometimes are considered to be the democratic translation of the class struggle. In the literature of political sociology, we find other concepts besides class voting which attempt to capture and describe different aspects of the relationship between class and voting. These are concepts such as the political distinctiveness of classes, class distinctiveness of parties, class solidarity, and cross-class voting. This paper attempts to clarify the relationship between some of the concepts introduced in the literature and to analyze certain problems in the measurement of class voting.

CLASS VOTING AND RELATED CONCEPTS

Voting and class are often measured by indicators which approach the ordinal level of measurement. Political parties can be ordered on a Left-Right continuum. Sometimes this is difficult, for example, with parties based on religion, culture, or region. Occupations can be rank ordered, for example, in terms of prestige, although this also causes some difficulties.

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Both indexes of class voting which primarily have been used so far are based on dichotomies: the index of class voting suggested by Alford (1963, pp. 79–80) and the index of status polarization used by Campbell and his co-workers (Campbell et al. 1960, pp. 344–47). However, the index of status polarization is also applied when there are several categories within the independent variable.

Alford's index of class voting is computed as follows: "Subtract the percentage of persons in the non-manual occupations voting for Left parties from the percentage of persons in manual occupations voting for Left parties" (Alford 1963, pp. 79–80). This is thus the most common of measures: the percentage difference between the manual and nonmanual classes in votes for the left party.

The concept of status polarization is defined so that "variations in the status polarization of a society reflects variation in the intensity and the extent of class identification among its members. When polarization is high, most of the citizenry must have perceived a conflict of interests between strata and have taken on class identifications with fair intensity. When polarization is low, either few people are identifying, or extant identifications are weak, or both" (Campbell et al. 1960, p. 339). Although defined on the subjective level, Campbell et al. (1960, pp. 344–46) also use occupational categories as indicators of status polarization. As Alford (1963, p. 87) points out, this constitutes an ambiguity in their conceptualization. The difference in voting patterns between occupational groups—that is, class voting—should not be confounded with the relationship between subjective class identification and voting.

Campbell et al. (1960) give no single definition of the computation of their measure of status polarization. Instead, they illustrate the idea of status polarization with a figure showing three levels of polarization, from complete polarization to an intermediate stage to complete depolarization (Campbell et al. 1960, p. 345). Here, the measure of status polarization apparently is computed in the same way as Alford's index of class voting. In another context with more than two categories on the stratification variable, they use a coefficient of correlation (τ_b) as a measure of status polarization (Campbell et al. 1960, p. 347).

The concept of class voting refers to the difference between classes in their vote for the left party. Other related concepts are the political distinctiveness of a class—for example, the proportion of workers voting for the left party—and the class distinctiveness of a party—for example, the proportion of the left vote coming from workers (Alford 1963, p. 84). In countries such as Norway and Sweden, where the left party draws the votes of most workers as well as a considerable proportion of votes from higher strata, the political distinctiveness of workers is high but the class distinctiveness of the left party is relatively low. The British Labour

Party, on the other hand, receives votes from workers to a very large extent, but workers also vote for the Conservative Party in significant proportions. There, the class distinctiveness of the left party is high but the political distinctiveness of the working class is relatively low.

The concept of class solidarity is defined as "the degree to which lower or upper strata deviate from 50 percent in their two-party vote proportion" (Campbell et al. 1960, p. 348). Alford (1963, p. 83) defines a similar concept, relative class solidarity, as "the percentage point difference between the degree to which manual workers deviate from a 100 percent vote for the Left party and the degree to which non-manual persons deviate from a 100 percent vote for the Right party." This difference also gives the relative degree of "cross-class voting."

To avoid ambiguities, I will define the concepts referred to above in terms of the frequencies in table 1.

TABLE 1
ELEMENTS USED IN DEFINING RELATIONSHIP OF CLASS TO PARTY

| CLASS | PARTY | | TOTAL |
|---------------|---------|---------|---------|
| | Left | Right | |
| Working | a | b | $a + b$ |
| Upper | c | d | $c + d$ |
| Total | $a + c$ | $b + d$ | (N) |

For simplicity, I will also use proportions rather than percentages in the following. The Alford index of class voting (CV) is then defined as $CV = a/(a + b) - c/(c + d)$. The political distinctiveness of the working class (PDW) is $PDW = a/(a + b)$, and that of the upper class (PDU) is $PDU = d/(c + d)$. The class distinctiveness of the left party (CDL) is defined as $CDL = a/(a + c)$. The class solidarity of the working class (CSW) as defined by Campbell is $CSW = a/(a + b) - 0.50$, while Alford's index of relative class solidarity (RCS) can be written as $RCS = b/(a + b) - c/(c + d) = d/(c + d) - a/(a + b)$. For the following discussion, it is helpful to define the political nondistinctiveness of a class—for example, the upper class ($PNDU$) as $PNDU = 1 - PDU = c/(c + d)$.

RELATIONSHIPS BETWEEN CONCEPTS

Measures of the concepts discussed above are related to each other since in table 1 we have only three pieces of independent information: the two marginal distributions and the frequency in one of the cells.

Before turning to the relationships between the concepts, let us first define two additional indices based on the marginal distributions in table 1 relating class to party. The proportion of left votes (PL) is $PL = (a + c)/N$ and the proportion of workers among the voters (PW) is $PW = (a + b)/N$. Together with PDW , which utilizes the frequency in one of the cells of table 1, these indices will be used to express the other concepts discussed above.

First, the political distinctiveness of the upper class is found to be:

$$PDU = \frac{PW(PDW - 1) + 1 - PL}{1 - PW}. \quad (1)$$

The level of class voting is equal to the difference between the political distinctiveness of the working class and the political nondistinctiveness of the upper class,

$$CV = PDW - (1 - PDU) = PDW - PNDU. \quad (2)$$

The class distinctiveness of the left party depends on the political distinctiveness of the working class, the proportion of left votes, and the proportion of workers in the following way:

$$CDL = PDW \frac{PW}{PL}. \quad (3)$$

The degree of class solidarity of the working class as defined by Campbell and his co-workers is

$$CSW = PDW - .50. \quad (4)$$

Alford's index of relative class solidarity is found to be

$$RCS = PNDW - PNDU = PDU - PDW. \quad (5)$$

Alford (1963, p. 83) states that the "level of relative class solidarity is determined by the absolute level of Left voting, not by the level of class voting." Equations (2) and (5) above indicate, however, that both relative class solidarity and class voting are dependent on the political distinctiveness of the classes and that relative class solidarity is related to class voting in the following way:

$$RCS = 1 + CV - 2 PDW. \quad (6)$$

Since we have only three pieces of independent information, the formulas above indicate some redundancy in the concepts. Once the proportion of left votes is given, relative class solidarity is dependent on the level of class voting except in the special case when the two classes are of equal size. Campbell's measure of class solidarity is neither informative nor useful since it only relates the political distinctiveness of a class to an

arbitrary baseline. On the other hand, the concept of class distinctiveness of parties carries independent information, since it is based not only on the political distinctiveness of a class but also on the two marginal distributions in table 1. This concept can therefore be used to compare parties.

FACTORS AFFECTING THE INDEX OF CLASS VOTING

The Alford index of class voting can be interpreted as a regression coefficient. If the distance between the two occupational categories is equal to unity, the difference between proportions of left votes from the manual and nonmanual classes gives the slope of the regression line of voting on class. The issue of class voting centers around the nature of the relationship between class and voting rather than on how much of the variation in voting can be explained by the stratification variable. A measure of regression is therefore more appropriate here than a measure of association (see Blalock 1964, pp. 50–52), such as the τ_b coefficient used by Campbell et al. (1960, p. 347).

It has been noted that changes in the way of dichotomizing the population into classes may affect the value of the index of class voting (Kahan, Butler, and Stokes 1966). These effects may, however, sometimes be negligible (Alford 1967, p. 86). Let us examine under what circumstances the index of class voting is affected by splitting up the population into two classes.

For this purpose, a model with a continuous stratification hierarchy is assumed which is split at some point into a manual-nonmanual, or working class-upper class, dichotomy. Further, each political party is assumed to represent the interests of people located at a specific point in the hierarchy. Also, an individual's position in the stratification hierarchy is assumed to determine what Dahrendorf (1959, p. 178) has termed his latent interests. These latent interests are converted to manifest interests or political ideologies through processes that turn quasi groups into interest groups (see Dahrendorf 1959, chap. 5). There will usually be some (but not complete) overlap between latent and manifest interests, that is, between position in the stratification hierarchy and political ideology. The left parties represent the interests of persons lower in the stratification hierarchy more than do the right parties.² The location of the parties with respect to the stratification hierarchy may vary, however.

The situation above can be conceptualized in terms of the model underlying Coomb's unfolding technique (see, e.g., Torgerson 1958, chap. 14). This model is completely deterministic. The party choice of an individual is determined by his position on the stratification hierarchy and its loca-

² As far as the methodological argument in the following is concerned, this assumption can be reversed.

tion with respect to the midpoint of the distance between the left and the right parties. Individuals located to the left of this midpoint vote for the Left, whereas those to the right vote for the Right. The trace line showing the probability for a left vote is a monotonic function of position on the stratification hierarchy. The probability for a left vote is unity up to the midpoint between the parties, where it falls to zero and remains so.

If one party wants to win a majority of the electors, it will try to move the crucial midpoint toward the other party. This is one way of arriving at the well-known prediction that the parties in a two-party system will adjust their policies toward each other (see, e.g., Downs 1957, chap. 8).

For simplicity, a rectangular distribution of the population along the stratification hierarchy is assumed. Let us also make the model probabilistic and assume trace lines of the type used in Lazarsfeld's well-known "latent distance" model (see, e.g., Torgerson 1958, pp. 374-85).

In the above model, the index of class voting will reach its *maximum value only when the midpoint between the parties coincides with the dividing line between the manual and the nonmanual class*. As soon as the population is dichotomized at some other point, the index will show a lower value. The amount of change in the value of the index will depend on the shape of the trace line and on the proportion of the population that falls between the point of the class dichotomy and the midpoint between the parties. Let us look at a few examples. Figure 1 and table 2 illustrate the effects on the index of class voting of different ways of dichotomizing the population when the shape of the trace line varies. The trace line depicted in Type A in figure 1 shows a very high degree of class voting with an index value of .80 when the manual-nonmanual dichotomy coincides with the midpoint between the strata (case 2). When increasing proportions of the population fall between the point of the manual-nonmanual dichotomy and the midpoint between the parties, and thus are misclassified in this sense, the value of the index decreases (cases 1, 3, and 4).

When the real level of class voting is lower, changes in the way of dichotomizing the population have similar but less pronounced effects. In Type B in figure 1, the maximum level of class voting is .40 (case 2). With the same changes in the way of dichotomizing the population as above, the value of the index decreases but does not drop off as rapidly as in the first situation (cases 1, 3, and 4).

If the midpoint between the parties divides the population in some other proportion than the 50-50 split assumed above, the effects of misclassification on the index of class voting will also depend on its direction. With 30% of the population below the midpoint between parties and the maximum level of class voting at .80, 20% of the population classified into the working class gives an index of .70, while the same degree of error in classification in the opposite direction gives 40% of the population in the

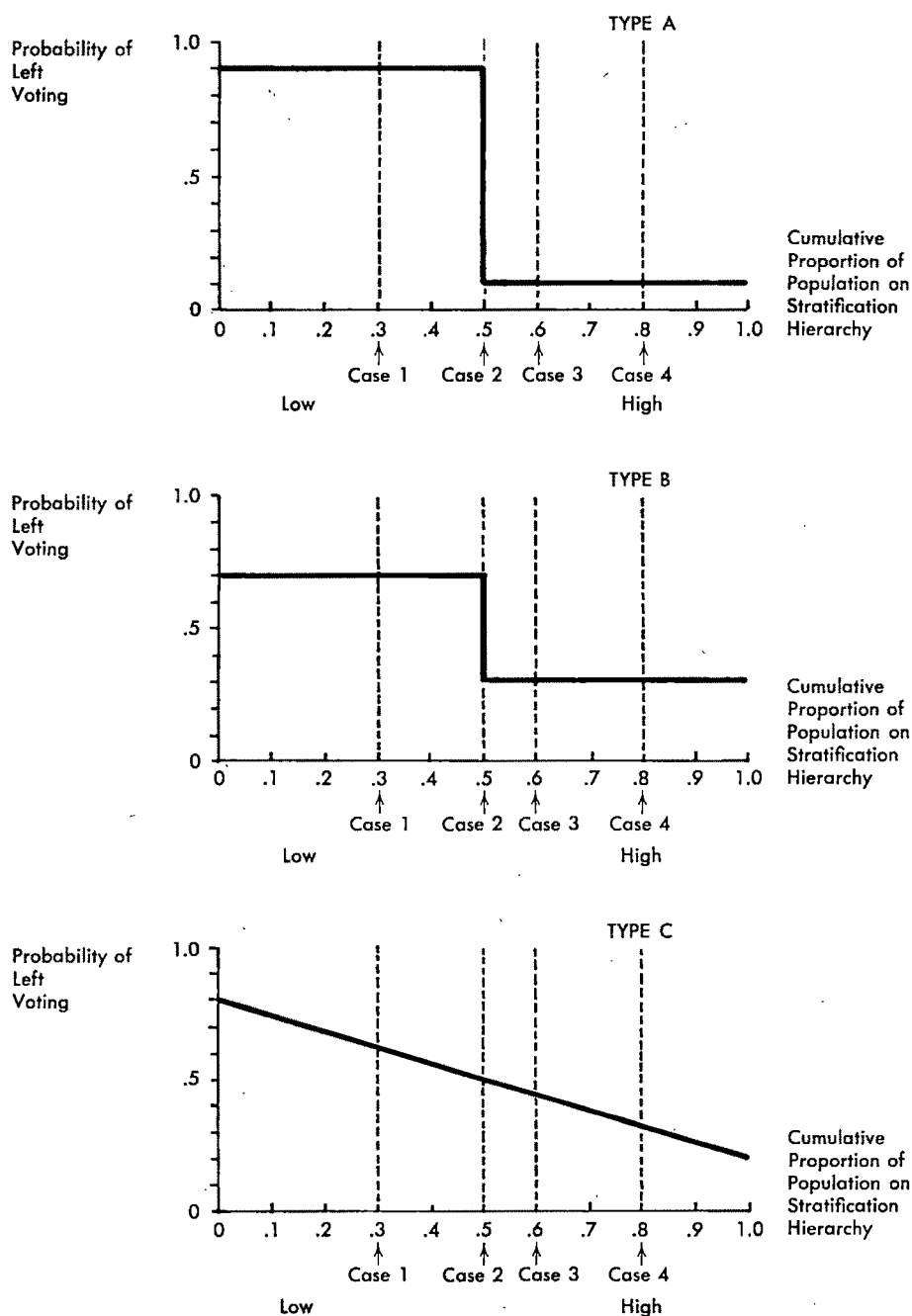


FIG. 1.—Three different types of trace lines, showing the relationship between probability for left voting and position on the stratification hierarchy with the rectangular population dichotomized at four different points (cases 1–4).

TABLE 2

INDEX OF CLASS VOTING AND CLASS DISTINCTIVENESS OF LEFT PARTY AS A FUNCTION OF THE FORM OF THE TRACE LINE AND THE PROPORTION OF THE POPULATION MISCLASSIFIED (CF. FIGURE 1)

| | Proportion of Popula- tion in Working Class | Proportion of Popula- tion Mis- classified | Proportion of Left Votes from Working Class (PDW) | Proportion of Left Votes from Upper Class (PNDU) | Index of Class Voting (CV) | Class Distinctive- ness of Left Party (CDL) |
|--------------|---|---|--|--|----------------------------------|---|
| Type A: | | | | | | |
| Case 1 | .30 | .20 | .90 | .33 | .54 | .54 |
| Case 2 | .50 | .00 | .90 | .10 | .80 | .90 |
| Case 3 | .60 | .10 | .77 | .10 | .67 | .92 |
| Case 4 | .80 | .30 | .60 | .10 | .50 | .96 |
| Type B: | | | | | | |
| Case 1 | .30 | .20 | .70 | .41 | .29 | .42 |
| Case 2 | .50 | .00 | .70 | .30 | .40 | .70 |
| Case 3 | .60 | .10 | .63 | .30 | .33 | .76 |
| Case 4 | .80 | .30 | .55 | .30 | .25 | .88 |
| Type C: | | | | | | |
| Case 1 | .30 | ... | .70 | .40 | .30 | .42 |
| Case 2 | .50 | ... | .65 | .35 | .30 | .65 |
| Case 3 | .60 | ... | .61 | .31 | .30 | .72 |
| Case 4 | .80 | ... | .55 | .25 | .30 | .88 |

working class and an index of .60. With the maximum level of class voting at .40, the corresponding values of the index of class voting will be .35 and .30.

As indicated above, the effects on the index of class voting of changes in the way of dichotomizing the population depend on the shape of the trace line. The more abrupt changes in the slope of the trace line, the more pronounced effects we can expect as a result from changes in dichotomization. When the trace line is linear, these effects on the index of class voting will disappear if the population has the rectangular distribution assumed here. This is illustrated in Type C. Here, the index of class voting has the same value for the different ways of dichotomization.

In the above three types, the proportion of votes for the left party has been set at .50. The class distinctiveness of the left party is then dependent only on the political distinctiveness of the working class and on the proportion of workers in the population. One may note in table 1 that the class distinctiveness of the left party (as eq. [2] indicates) increases with increases in the proportion of the population classified into the working class; the class distinctiveness of the right party is affected in a parallel way. This index is affected by changes in the dichotomization of the population even when the class voting index remains unchanged (Type C).

The shape of the trace line thus determines what effects changes in the

way of dichotomizing the population will have on the index of class voting. The concept of class voting implies that occupational categories may form coalitions, or turn into interest groups. To the extent that this has occurred, we cannot expect linear trace lines but trace lines that have a steeper slope around the dividing line between the classes.

Also, the assumption of a population with a rectangular distribution is unrealistic. In most countries, population distributions are skewed toward the lower end of the stratification hierarchy and with different degrees of variation in different countries. Even if the regression of voting on class is linear, the slope of the regression line (therefore, also the index of class voting) will be affected by changes in the way of dichotomizing the population (see Blalock 1964, pp. 119–24, for a discussion of this problem).

In nations without a genuine two-party system, the problem arises how to dichotomize the parties into two categories: Left and Right. This problem is particularly difficult in countries with agrarian parties. The way in which this dichotomization is done may have effects on the index of class voting. Let us look at a simple case with three parties—Left, Center, and Right—that can be ordered on a left-right dimension. The trace line giving the probability of voting for a party as a function of the stratification hierarchy is assumed to be monotonically decreasing for the left party, monotonically increasing for the right party, and nonmonotonic with a maximum around the middle of the stratification hierarchy for the center party.

Dichotomization will have the effect of changing the slope of the trace line for the party with which the center party is combined. A combination of the center party with the left party will cause the trace line of the new left coalition to decrease more slowly both toward the bottom and toward the top of the stratification hierarchy. Also, the location on the stratification hierarchy of the point where the trace line has its steepest slope may be changed. Such changes might be important for the index of class voting.

ON THE SIGNIFICANCE OF CHANGES IN CLASS DICHOTOMIZATION

In dividing the population into a manual-nonmanual or working class-upper class dichotomy, difficulties arise with two types of occupational categories. One type of problem concerns occupations which can be ranked into a stratification hierarchy, but whether the group is placed into the higher or the lower class is largely arbitrary. The second type of difficulty has to do with occupational groups difficult to fit into a unidimensional rank order.

Among occupational groups that fit reasonably well into a stratification hierarchy but form a marginal sector between the higher and the lower strata, are sales workers, nonsupervisory clerks, policemen, firemen, foremen, railway conductors, noncommissioned officers, factory guards, and

the like. In many Western European countries, these groups constitute roughly 10%–25% of the economically active population. They form an intermediary sector between manual workers and the higher strata in terms of class identifications and voting patterns (see, e.g., Kahan et al. 1966; Dahlström 1954, chap. 2).

How is the index of class voting affected by the way in which we place reasonable proportions of the lower white-collar groups into the different classes? Let us look at a few examples, using data from Great Britain (Kahan et al. 1966), Western Germany (Linz 1967, p. 287), New Zealand (Robinson 1967, p. 97), and Sweden (Official Statistics of Sweden 1965, p. 95; *Indikator* 1969).³ As shown in table 3 the largest increase in the index of class voting is .05 while the largest decrease is —.09.

TABLE 3
CHANGES IN THE INDEX OF CLASS VOTING WHEN PARTS OF THE LOWER
NONMANUAL OCCUPATIONS ARE PLACED IN THE HIGHER CLASS
AND IN THE LOWER CLASS, RESPECTIVELY

| Country | Lower Nonmanuals in Upper Class | Lower Nonmanuals in Lower Class | Difference | Proportion Lower Nonmanuals in Sample |
|-------------------------------------|--|--|------------|--|
| West Germany | .28 | .23 | .05 | .02 |
| Sweden (1964) | .38 | .47 | — .09 | .31 |
| Sweden (1967–69, men only) | .46 | .48 | — .02 | .19 |
| New Zealand | .50 | .46 | .04 | .18 |
| Great Britain | .50 | .48 | .02 | .10 |

Among occupational categories that are difficult to rank into a unidimensional stratification hierarchy, independent farmers probably are the most important group (Carlsson 1958, pp. 145–46). The primary sector engages about 5%–40% of the economically active population in Western European countries. Alford (1963, p. 72) excludes farmers from his study of class voting in the Anglo-American democracies. In many countries, however,

³ The reclassified occupational categories are classified as Grade IV in the British data (lower nonmanual, including shop salesmen and assistants, policemen, caretakers, lodginghouse keepers, waiters, etc. Kahan et al. 1966, p. 131). The reclassified groups are referred to as “artisans” and “lower white collar” in the data from West Germany (Linz 1967, p. 287) and as “white collar” and “uniform” workers in the data from New Zealand (Robinson 1967, p. 97). In the Swedish election statistics from 1964, this group consists of “lower salaried employees, sales workers, etc.,” while in the data from 1967–69 those reclassified are “lower middle class occupations”—for example, policemen, waiters, second line foremen, sales and office workers, and telephone operators (*Indikator* 1969, pp. 3–4). Note that the data from New Zealand are a sample from only a part of the country (Robinson 1967, p. 96).

farmers are such a sizable and relatively homogenous interest group that they can hardly be omitted from a study of voting patterns in the society.

How is the index of class voting affected if farmers are included in the class dichotomies? Since farmers generally do not vote for parties on the left, they should be placed in the nonmanual category. Data from Western Germany (Linz 1967, p. 287), from the 1948 and the 1952 presidential elections in the United States (Campbell et al. 1954, p. 72) and from Sweden (Official Statistics of Sweden, p. 97; *Indikator* 1969, p. 3-4) are used in table 4 to compute indexes of class voting when farmers are excluded and when farmers are placed in the nonmanual category.

TABLE 4
CHANGES IN THE INDEX OF CLASS VOTING WHEN FARMERS ARE INCLUDED IN
THE NONMANUAL CLASS AND WHEN FARMERS ARE EXCLUDED

| Country | Farmers in Nonmanual Class | Farmers Excluded | Difference | Proportion Farmers in Sample |
|--------------------------------|----------------------------------|---------------------|------------|------------------------------------|
| United States (1952) | .23 | .22 | .01 | .13 |
| United States (1948) | .40 | .24 | .16 | .17 |
| West Germany | .31 | .28 | .03 | .12 |
| Sweden (1964) | .45 | .38 | .07 | .11 |
| Sweden (1967-69, men only) ... | .49 | .46 | .03 | .08 |

The change in the index of class voting when farmers are included is largest for the 1948 elections in the United States, when the farmers to a large extent voted for the Democrats. In the other cases, the index of class voting changes from .01 to .07 units.

In order to evaluate the significance of the changes in the above examples, we can compare them with the variation in the index values reported by Alford (1963, p. 102; 1967, pp. 81-82) in his study of the Anglo-American democracies. Alford established the following rank order between the countries based on the mean values of the index of class voting from several different surveys within each country: Great Britain, .40; Australia, .33; United States, .16; and Canada, .08. The difference in the mean index value between Great Britain and Australia is thus .07; between United States and Canada it is .08. Therefore, it appears difficult to argue that changes in the index of class voting caused by different ways of handling farmers and the lower nonmanual occupations are negligible.

SEGREGATION INDEXES AS MEASURES OF CLASS VOTING

It has often been the case in the social sciences that attempts to solve methodological problems in one specific area have not taken advantage of

advances made in solving similar problems in other areas. The problem of measuring class voting is related to problems in measuring inequality or concentration of distribution that have been encountered in economics, geography (see, e.g., Duncan, Cuzzort, and Duncan 1961), and political science (see, e.g., Alker and Russett 1966). Within the field of sociology, similar problems have been attacked in the measurement of racial segregation.

If both class and voting are measured at the nominal level, the problem of indexes for class voting becomes identical to that of measuring racial segregation in ecological studies. Several measures of segregation have been used. Their interrelationships and methodological properties have been analyzed by Duncan and Duncan (1955). They find that the information contained in the various segregation indexes is based upon the proportion nonwhite and the segregation curve. The segregation curve is obtained by arranging the ecological units in the order of increasing proportions of nonwhite and then computing unit by unit the cumulative proportions of nonwhites and whites. Then the cumulative proportion of nonwhites is plotted on the x -axis and the cumulative proportion of whites on the y -axis. The result is the segregation curve. The amount of segregation is depicted by the area between the diagonal, which shows the situation of equal proportions of whites and nonwhites in all units, and the segregation curve. This area can be expressed as a proportion of the total area under the diagonal, giving the Gini index (Kendall and Stuart 1958, pp. 48 ff.).

The segregation curve and the Gini index of segregation can obviously also be used in the study of political cleavage on occupational, religious, regional, or other bases.⁴ The spatial units of ecological studies are then supplanted by the occupational groups (or whatever groupings of voters we may choose), while the proportion left (or right) votes takes the place of the proportion nonwhite.⁵

The values of the Gini index and the index of class voting for eight

⁴ A problem with the Gini index is that it defines the absence of segregation as the situation when all units have *equal* proportions nonwhite rather than the situation when nonwhites are *randomly* distributed between units.

An alternative measure can be developed by examining the properties of pairs of individuals. A measure of "cross-cutting" based on the proportion of all pairs of individuals whose two members are similar in one property but different on the other property has been proposed (Rae and Taylor 1970, chap. 4).

⁵ To compute the Gini index, occupational groups are rank ordered according to the cumulative proportions of left votes (x_i) and right votes (y_i) as in the segregation curve. Let $x_0 = y_0 = 0$. With k groups, the Gini index may be calculated from the following formula:

$$\text{Gini index} = 1 - \sum_{i=1}^k (x_i - x_{i-1})(y_i - y_{i-1}) - 2 \sum_{i=2}^k (x_i - x_{i-1}) y_{i-1}.$$

TABLE 5

A COMPARISON BETWEEN THE INDEX OF CLASS VOTING AND THE GINI INDEX
WHEN FARMERS ARE EXCLUDED AND WHEN FARMERS ARE
INCLUDED IN THE NONMANUAL CLASS

| COUNTRY | FARMERS EXCLUDED | | FARMERS INCLUDED | | NUMBER OF OCCUPATIONAL CATEGORIES |
|-------------------------------------|-----------------------------|---------------|-----------------------------|---------------|---|
| | Index of Class Voting | Gini Index | Index of Class Voting | Gini Index | |
| United States (1952) | .23 | .27 | .22 | .28 | 5 |
| West Germany | .28 | .35 | .31 | .45 | 11 |
| Sweden (1964) | .38 | .48 | .45 | .58 | 6 |
| United States (1948) | .40 | .46 | .24 | .47 | 5 |
| Sweden (1967-69, men only) | .46 | .57 | .49 | .61 | 9 |
| New Zealand | .50 | .59 | ... | ... | 5 |
| Sweden (1946) | ... | ... | .49 | .53 | 3 |
| Great Britain | ... | ... | .50 | .52 | 6 |

different cases are compared in table 5 both when farmers are included and when they are excluded.⁶ On the whole, the two indexes rank order and group the cases in the same way when farmers are excluded. If we include farmers, the greatest discrepancy is again found for the data from the 1948 elections in the United States.⁷ When farmers are included, the relative degree of class voting in Sweden in the 1964 elections and in West Germany depends to a considerable degree on what index we use.

One advantage of the Gini index of segregation is that it can be based on several occupational categories. Thereby more of the available information is utilized than is the case with the index of class voting. Also, it is possible to include occupational groups such as farmers and the lower nonmanual sections that are difficult to place unambiguously into a class dichotomy. The Gini index is theoretically relevant since it measures inequality in the distribution of votes between occupational groups that form the basis of the stratification system. To the extent that there is a uni-dimensional rank order between occupational groups on the stratification variable, information is lost, however. On the other hand, this information is not utilized very effectively in a dichotomy either.

Problems with the definition of occupational groups remain. The Gini index is sensitive to the number of occupational categories used. In general, the higher the number of occupational subdivisions used, the higher index values we can expect to get. If new occupational groups are created through

⁶ The data are from the same sources as those in table 2 and 3.

⁷ This comparison is in a sense "unfair" to the index of class voting, since the Gini index does not reflect changes in the direction of the farm vote between 1948 and 1952, while the index of class voting is affected since farmers are placed in the higher stratum in both elections.

subdivisions of larger occupational categories, the value of the index cannot decrease but will increase if the new subdivisions show voting patterns different from the larger category. The effects on the index depend on the relative size and homogeneity of the new groups created. This is illustrated in the following example.

In the Swedish data from 1967-69, there are nine occupational categories with the following proportions of left votes: unskilled workers (.79), semi-skilled workers (.78), skilled workers (.78), workers in agriculture and forestry (.63), lower white collar (.53), self-employed entrepreneurs (.30), middle white collar (.20), higher white collar (.14), and farmers (.06). If only five occupational categories (manual workers, lower white collar, other white collar, self-employed entrepreneurs and farmers) are used, the Gini index decreases from .61 to .60. If however, the sizable lower white-collar category is grouped together with the other white-collar occupations, the Gini index drops to .50.

SUMMARY AND DISCUSSION

The concepts proposed to cover different aspects of the relationship between class and voting have often been defined only at a verbal level and have not been tied clearly enough to the three pieces of independent information available in table 1: the two marginal distributions and the proportion in one of the cells. Some of the concepts proposed are therefore redundant. With the two marginal distributions given, the index of class voting taps the remaining information. Therefore, concepts like class solidarity or cross-class voting do not give much additional information. The class distinctiveness of a party, however, gives information not covered by the class voting measure.

The regression curve of left voting on position in the stratification hierarchy is the most exhaustive and relevant indicator of the relationship between class position and voting. Where the stratification variable can be measured on the cardinal level (e.g., income or years of schooling) the regression curve can be used as an index of class voting.

The Alford index of class voting can be interpreted as a regression coefficient. Its value is affected by the way in which the population is dichotomized into classes. The seriousness of these effects depends on the shape of the regression curve of voting on class and on the proportion of the population that is misclassified. In most cases such effects will occur even if the regression of voting on class is linear. It appears difficult to argue that these effects are negligible.

The Gini index of segregation is relevant for the theoretical content of the class voting concept and has some advantages when compared with the Alford index. Since the Gini index uses several categories on the occupa-

tional variable, we can in general expect to have more reliable measurements with the Gini index. Occupational categories that are difficult to place unambiguously into a class dichotomy can be handled with the Gini index. Furthermore, it makes better use of available data. If positively skewed population distributions are dichotomized close to the median, the lower class will be much more homogeneous than the upper class. The high political distinctiveness of some of the nonmanual groups will then pass unnoticed.⁸ However, the Gini index is not independent from the particular occupational groupings used and requires that the same definitions of occupational categories be applied in the countries compared.

Simplicity is the appeal and advantage but also the weakness of the Alford index of class voting. If it is to be used for comparative purposes, we should ideally have countries with two-party systems and with unidimensional systems of stratification. The four Anglo-American democracies studied by Alford probably come closest to this ideal. In many other countries, however, the relationship between the system of stratification and the party system is so intricate that the model becomes an undue oversimplification. This is illustrated by the problems associated with describing the level of class voting in a country with a sizable proportion of independent farmers and an agrarian party.

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⁸ An example of such groups are military officers which in a Swedish study were found to support the bourgeois block to 96% and the most conservative party to 85% (Abrahamsson 1968).

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Crime and the Division of Labor: Testing a Durkheimian Model¹

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This study provided an empirical examination of an unsubstantiated but generally accepted proposition found in Durkheim's *The Division of Labor in Society*. The proposition that deviance or crime will increase concomitant with an increasing division of labor has found wide acceptance in both deviance and organizational studies without having been empirically verified. In order to test this proposition and four corollary suppositions, an examination was made of collective life utilizing data similar to those relied on by Durkheim in his examination of the etiology of suicide. In Durkheimian terms, then, this study involved an examination of degrees of collective order (operationalized as crime rates) as found within different collectivities (communities) possessing differential degrees of functional integration (division of labor). The primary hypothesis was largely refuted, but alternative interpretations of the findings suggest that the model cannot be discounted.

The consideration of types of societies and their underlying bases of social integration has generally led to a simple dichotomy of types. Various named, but invariably the same, the dichotomy consists of the communal type based on consensus and the corporate type based on differentiation. Similarly, two fundamental types of social integration have received most of the consideration from social theorists. The first, normative integration, is thought to be the basis of social order in the communal type society wherein integration or solidarity is maintained by universal consensus of the populace on a relatively consistent set of norms and values. Functional integration, on the other hand, is generally thought to prevail in the more complex or corporate type societies. The functional integration of an organization is based on a division of labor among its members which makes them mutually interdependent. This interdependence promotes and secures the solidarity or integration of the organization.

Formulating what is essentially a theory of societal evolution, Durkheim derived a similar dichotomy with his mechanical and organic types of

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solidarity, generally thought to be identical with normative and functional integration, respectively. In *The Division of Labor*, Durkheim posited that as a community grows in complexity, size, and density, the dominant mode of integration evolves from the mechanical to the organic type. This theory of societal evolution was meant to explain the growth of social organization and its parallel growth processes, especially the division of labor. Among the processes expected to occur concomitant with these changes were the collective conscience becoming more abstract; law, morality, and civilization generally becoming more rational; a decrease in normative consensus; and an increase in deviant behavior.

The authority of the common conscience is based largely on the authority of tradition, and under the conditions of mechanical solidarity there is a high degree of control over the individual. But as the moral community disintegrates, the sanctions of public sentiment and law are thereby weakened and the basis of integration evolves toward what Durkheim termed organic solidarity. As he stated, "It is the division of labor which more and more fills the role of what was formerly filled by the common conscience. It is the principal bond of social aggregates of higher types" (1964, p. 175).

Durkheim noted that greater individual variability is a necessary correlate of an increase in the division of labor. He contended that "the division of labor can progress only if individual variability increases" (1964, p. xx). In essence, the authority of the collective conscience diminishes and individual variability increases as the community moves in the direction of increasing size, heterogeneity, complexity, and an order of integration or solidarity based on functional or organic interdependence. As these phenomena progress the individual is freed from traditional authority, and much greater variability in his behavior becomes acceptable. As Durkheim stated, "In accordance with the effacement of the segmental type, society, in losing hold of the individual can much less hold divergent tendencies together" (1964, p. xx). Simply put, as the strength of the collective conscience dissipates and the community moves toward an organic or functional basis of integration, it can expect an incipient increase in individual variation or deviance.²

² Nowhere in *The Division of Labor* does Durkheim explicitly indicate that an increased division of labor or differentiation leads to a rise in the rate of crime. His definition of crime, of course, precluded such a conclusion, for he conceived of crime as actions which offend strongly defined states of the collective or common conscience, as represented in repressive law, and the violation of which engenders punishment rather than restitution (1964, pp. 80-132). Organic solidarity, however, is characterized by restitutive law and "these rules are more or less outside the collective conscience" (1964, p. 113). Furthermore, the collective conscience loses its intensity and diminishes as the society becomes more functionally integrated and thus, by definition, there would be less crime with increased differentiation. From Durkheim, "The acts which custom alone must repress are not different in nature from those the law

This, then, constitutes Durkheim's model of the interplay between the division of labor, organic solidarity, and deviance. It needs to be said, however, that these phenomena require an increased population size and greater social and physical density. As Durkheim argues, "The division of labor varies in direct ratio with the volume and density of societies, and, if it progresses in a continuous manner in the course of social development, it is because societies become regularly denser and more voluminous" (1964, p. 262). The interaction of these factors provide a mutually reinforcing scheme in which greater deviance can be treated as an expected consequence. Accordingly, deviance was considered here as the dependent variable, population size and physical and social density were considered as independent or predictor variables, and the division of labor was treated as an intervening variable. This conceptual model is presented below:

Population Size (X_2) →

Population Density (X_3) → Division of Labor (X_1) → Deviance (Y_1, Y_2)

Social Density (X_4) →

The following hypotheses were derived from this theoretical framework.

- I. There is a direct relation between the extent of the division of labor in communities and the extent of deviance within these communities.
- II. There is a direct relation between the number of people residing in communities and the extent of the division of labor within these communities.
- III. There is a direct relation between the social density of communities and the extent of the division of labor within these communities.
- IV. There is a direct relation between the physical density of communities and the extent of the division of labor within these communities.

punishes; they are only less serious. . . . When the weakest sentiments lose their energy, the strongest sentiments . . . cannot keep theirs intact" (1964, p. 301).

Thus, if we extend these arguments to their logical conclusion, in time nothing will offend the collective conscience or else it will cease to exist and there will be no crime. Knowing full well that reported "crimes" were increasing with societal evolution (1938, p. 66), Durkheim ignored these flaws in logic and instead focused on what has been termed deviance or "individual variations." This problem could have been avoided if Durkheim had defined crime as an act in violation of a law, for he clearly recognized that the number and variety of special interest laws were increasing (1964, pp. 302-3), and also the opportunity for violating these laws would naturally increase with societal evolution (1964, p. 153). Thus the conceptual leap in this research from Durkheim's "individual variation" and "divergent tendencies" to deviance, which becomes operationally defined as crime.

- V. There is an indirect relation between the size and physical and social density of communities and the extent of deviance within these communities.

These hypotheses were tested using the following operationally defined variables as epistemic correlates of the theoretical variables.

Deviance.—Deviance was operationally defined using two separate indices—the property and the personal or violent crime rates of a community. The property crime rate is composed of the number of “serious” property crimes, that is, burglary, larceny (\$50 and over), and auto theft. Composing the personal crime rate are the offenses of criminal homicide, forcible rape, robbery, and aggravated assault. In each case the rate is composed of the number of crimes known to the police in each community as reported in the 1960 *Uniform Crime Reports*.

It can be argued that property crimes are a more valid index of basic structural changes—and by extension, social integration—than personal or violent crimes. As Clinard has noted, “the influence of functional changes in a society is reflected far less in personal crimes than in property offenses which involve the acquisition of things and not necessarily any personal or fortuitous situation” (1942, p. 204). The violent or expressive crime is generally spontaneous or an act of passion, committed without reflection or premeditation. The property or instrumental offense, on the other hand, is generally the result of rational and motivated behavior, and as such seems a more valid index of the offender’s normative orientations, which are in turn a reflection of the underlying social structure.

Yet Durkheim did not provide an explicit rationale for the examination of only property offenses; rather, he dealt with crime or deviance as a generic concept. For this reason it was thought that testing the hypotheses using different operational indicants of the criterion would provide a more valid test of the conceptual model.

Division of labor.—Following Gibbs and Martin (1962) and Labovitz and Gibbs (1964), a definition of the division of labor was used which differs somewhat from that suggested by Durkheim. In essence, the division of labor refers to the degree of functional interdependence among population elements in regard to their sustenance and economic activities (Clemente 1972). Hauser has argued that “one of the most fruitful ways of getting at the sustenance and functional economic relations within a community and between communities lies in the analysis of the labor force of the community classified by industrial and occupational composition” (1956, p. 486). There are two basic components of the division of labor measure. First is the number of different industries found in a population,³

³ Although important, this characteristic is of little concern in this study since a constant number of categories is used.

and second is the differential distribution of individuals within these industrial categories (Labovitz and Gibbs 1964, p. 6). The more even the distribution of individuals among the industrial categories, the greater the division of labor. For example, given 10 industries with 90% of the population in one category or 10 industries with 10% in each, the division of labor or industrial diversification is much greater in the latter case. As Labovitz and Gibbs have demonstrated, given an equal number of industries, "the more even the distribution among them, the greater the division of labor" (1964, p. 6).

The formula for the division of labor is $D = 1 - [\Sigma X^2 / (\Sigma X)^2]$, "where D is the degree of division of labor for the population, and X is the number of individuals" in each industrial category. It is further indicated that "the minimum value of D for any distribution is always 0," while the maximum value is dependent on the number of industrial categories utilized (Labovitz and Gibbs 1964, p. 6). "In other words, the D value . . . reflects the degree to which persons are evenly distributed throughout a given industry structure" (Gibbs and Browning 1966, p. 88).

The industrial categories utilized were those enumerated by the U.S. Bureau of the Census (1963-64, table 75). A measure of the degree of industrial diversification based on the number of individuals employed in each industrial category was computed for each urban place, and it was this measure which was used as a correlate for the division of labor.⁴

Population size and physical density.—As mentioned above, Durkheim suggested that among the conditions accompanying the rise in the division of labor and the general trend toward organic solidarity were an increasing population size and an increasing physical density of the society. In fact, both population size and physical density were considered prerequisite to the development of a complex society based on organic rather than mechanical solidarity. Commenting on the relation between size, density, and deviance, Durkheim stated, "Insofar as a society is extended and concentrated, it envelops the individual less, and, consequently, cannot as well restrain the divergent tendencies coming up" (1964, p. 297).

In this study, population size was simply defined as the number of individuals inhabiting a particular community, that is, making it their usual place of abode. Physical density of a community refers to the population per square mile of land area (U.S. Bureau of the Census 1961).

Social density.—While the conditions of increasing size and physical density are correlates of a diminishing collective conscience and increasing

⁴ The measure of the division of labor may also be based on occupations, but which of the two measures is the "better" indicator of Durkheim's concept is unknown. As Gibbs and Martin point out, "The distinction between occupation and industry in the analysis of the degree of the division of labor has evidently not been determined, conceptually or empirically" (1962, p. 669).

deviance, there is no necessary relation between them. In order for physical density to have an effect on the division of labor or the incidence of deviance, it must be accompanied by an increased social density. Durkheim suggested that the lack of means of transportation and communication demonstrates the exclusivity of the community and larger society and thus is an indication of mechanical solidarity (1964, p. 291). Paralleling Durkheim's thesis, Hawley has argued that in order for an organization to "progress with population growth beyond the limits set by a few hundred there must be an increase in the social density or, specifically, an increase in the frequency and range of interhuman contacts." This conceptualization of interaction is crucial to the definition of social density. For as Hawley went on to note, social density "can be achieved only through the facilitation of movement. The term 'movement' is used here in a broad sense to include all forms of transportation through space, whether of individuals, materials or ideas as such" (1950, pp. 199-200).

Without this increase in social density, an increase in physical density will have little or no effect on the division of labor or the other processes and conditions of functional integration. The degree of transportative and communicative efficiency, then, can be used as an index of social density, and the extent of their presence can demonstrate the extent of interaction within the community as well as the degree to which the community approaches the polar collective type. Following these suggestions, social or dynamic density was operationalized as the proportion of a community's labor force employed in the following industries: railway and railway express, trucking service, other transportation, and communication.⁵

Community.—Communities were defined as those political, urban places composed of 25,000 or more inhabitants. This included incorporated and unincorporated places as well as "the towns, townships and counties classified as urban" (*Census Summary 1960*, p. xxix). In 1960 there were 765 such areas, but complete criminal statistics were available for only 691 of

⁵ This operational definition was conceived and first utilized by Clemente (1969). Land, on the other hand, concluded that there was "no satisfactory measures of the efficiency of the technology of communication and transportation" and relied on the rationale of the *interchangeability of indices* to derive the measure "percentage of population in urban places" as an epistemic correlate for dynamic density (1970, p. 274). It seems that, in this case, Land is guilty of forcing his theory to fit the data for, in fact, the referent of his definition might better be conceptualized as a consequence of dynamic density rather than a measure of it. It is argued, therefore, that the operational definition utilized in this study is a more valid measure of Durkheim's original construct of dynamic density than that utilized by Land. Commenting on the difficulty of operationally measuring this construct, however, Brown-ing and Gibbs concluded, "if 'social or moral' density is taken to be the independent variable, Durkheim's theory is untestable, because it is difficult to imagine a method of measurement, let alone securing the requisite data" (1971, p. 240).

them. Nine areas were eliminated because of incomplete census data. Thus the study population consisted of those 682 communities submitting complete criminal statistics to the Federal Bureau of Investigation and for which there was census data available for all the indices required by the research propositions.⁶

DATA SOURCE

The 1960 *U.S. Census* and the 1960 *Uniform Crime Reports* constituted the two sources of empirical data utilized in this study. There are many apparent limitations in the use of such secondary sources, among which are the problems of urban definition and the impossibility of assessing the adequacy of the methodological procedures used in the original data collection. These limitations as well as numerous problems with definitions of terms and categories make the *Uniform Crime Reports* data especially difficult to interpret.⁷

PROCEDURES

Pearson product-moment correlations were used to test the hypotheses, and a multiple-partial correlational analysis was performed. The division of labor was entered into the regression equation first, and the remaining independent variables were added in the order of their relative contribution to the reduction in the error sum of squares. This allowed for the determination of the percentage of the variance in the criterion they jointly accounted for, over and above that accounted for by the division of labor. According to the Durkheimian model, the effect of the three causally antecedent variables should cancel out when variation in the division of labor is controlled. To the extent that they do not, their effects on the criterion are direct, and Durkheim's model may be modified accordingly. This procedure, coupled with the correlations of the independent variables with the criterion, partialled on the division of labor, allowed for the empirical testing of Durkheim's model.

⁶ It might be argued that SMSAs would prove equally useful as the operational unit of analysis. There are, however, important theoretical and methodological objections to the use of SMSAs which are not encountered—to the same degree at least—with urban places. One effect of using SMSAs would be to eliminate many urban places with populations under 50,000, which would serve to truncate the variance and explanatory capacity of the independent variables. Whereas the narrow range of the variable "social density" is noted as a problem in a later part of this paper, the use of SMSAs could only augment the problem. Finally, for 1960 there are crime data available for only 184 of the 212 SMSAs, exacerbating the problem of truncated variance.

⁷ For additional comments on the adequacy of the data reported in the *Uniform Crime Reports*, see Black 1970; Beattie 1955; Cressey 1957; and Wolfgang 1963.

In the case of the independent variables "population per square mile" and "population size," a logarithmic transformation of the original indices was used in computing the correlations. The rationale for using logs was based on an expected wide range of values for both physical density and population size and their anticipated extremes which were expected to produce less effect on the dependent variable.

RESULTS AND ANALYSIS

The first hypothesis—predicting a direct relation between the measure of industrial diversification (division of labor) of communities and the number of known serious crimes per 10,000 inhabitants—was modestly supported with a zero-order correlation (r) of .21 for property crimes and .10 for the personal crime rate (see table 1). Although the ex-

TABLE 1
ZERO-ORDER PRODUCT MOMENT CORRELATIONS FOR ALL VARIABLES
FOR THE UNITED STATES, 1960

| Variables | X_1 | X_2 | X_3 | X_4 | Y_1 | Y_2 |
|---|-------|-------|-------|-------|-------|-------|
| Industrial diversification (X_1) | ... | .17 | .00 | .37 | .21 | .10 |
| Population size (X_2) | ... | ... | .26 | .13 | .35 | .44 |
| Population per sq. mile (X_3) | ... | ... | ... | .08 | .18 | .13 |
| Proportion in transportation and communication (X_4) | ... | ... | ... | ... | .05 | .11 |
| Property crime rate (Y_1) | ... | ... | ... | ... | ... | .60 |
| Violent crime rate (Y_2) | ... | ... | ... | ... | ... | ... |

pected relationship was supported, it manifested a relatively weak association. A factor which almost certainly contributed to the low degree of association was the examination of only those cities with populations of 25,000 or larger. Durkheim set no such limits in his theoretical scheme, and certainly if smaller urban units were used it is likely that the measures which showed so little variability in this study might exhibit considerably more range. Certainly, smaller urban units would have lower measures of industrial diversification, and this alone would increase the range of the measure and thereby possibly increase the predictive accuracy of the variable. Unfortunately, the inclusion of smaller urban places was not possible, since criminal statistics were available only for those communities over 25,000.

The remaining hypotheses predicted direct relationships between the independent variables and the intervening variable, industrial diversifica-

tion, and by extension indirect relationships with the criterion. With the exception of the association between physical density and diversification, the relationships are all in the predicted direction, giving general support to the secondary hypotheses.

Table 2 presents a summary of the regression analysis for each criterion. The magnitudes of the standardized regression coefficients indicate how much change in the dependent variables is produced by a standard change

TABLE 2
STANDARDIZED REGRESSION COEFFICIENTS AND CORRELATIONS OF NUMBER OF
KNOWN SERIOUS PROPERTY CRIMES (Y_1) AND PERSONAL CRIMES (Y_2)
AND PREDICTOR VARIABLES FOR THE UNITED STATES, 1960

| REGRESSION COEFFICIENTS | | | | | R | R ² | MULTIPLE- PARTIAL COEFFICIENT* |
|---|-------------------------------------|---|---|---------|-----|----------------|--------------------------------------|
| Industrial Diversifi- cation (X_1) (B) | Population Size (X_2) (B) | Population per Square Mile (X_3) (B) | Proportion in Trans- portation and Communica- tion (X_4) (B) | | | | |
| Y_1 | .1724 | .3073 | .1019 | — .0573 | .40 | .16 | .1250 |
| Y_2 | .0131 | .4245 | .0165 | .0486 | .44 | .19 | .1818 |

* This is the multiple correlation between the dependent variable and the independent variables controlling on the intervening variable, or r_{12}^2 (234) .1.

in each predictor, controlling on the other predictor variables. With the exception of Y_1 regressed on X_4 , all the coefficients are positive and in the predicted direction. In only three instances, however, does it seem that an independent variable exerts a substantial impact on the relative crime rates, and it is obvious that population size (X_2) is the better predictor of either criterion. The effect of all other variables on Y_2 is negligible, but for Y_1 , both X_1 and X_3 exert some impact although X_2 still has the greatest influence. Thus population size is clearly the strongest predictor of either criterion in this model.

The multiple correlation coefficients provide a measure of the degree to which the criterion can best be predicted from the sum effects of all independent variables acting together. For the United States, multiple correlations of .32 and .44 were obtained, indicating a moderate relationship between property and personal crime rates and the combined effect of the predictor variables. A large part of the relationship, however, is explained by the single factor of population size. The coefficient of determination indicates that at most only 16% and 19% of the total variation in property and personal crime rates can be explained by the combined action of the independent variables.

While these figures allow only a moderate degree of prediction of the dependent variable, it must be emphasized that a linear model is being assumed and that *any* degree of prediction using population parameters is theoretically significant. When the additional limitations of random and nonrandom measurement error and limitation of city size to 25,000 and over are considered, the multiple correlations do not seem to lack substantive significance.

In order to examine the relative contribution of each variable to the reduction of the residual variance, industrial diversification was entered into the regression equation first with the remaining predictor variables allowed to "float." This procedure allows us to determine if in fact the antecedent variables actually operate "through" the intervening variable, as Durkheim postulated (see fig. 1). The implication of this procedure is that when variation in the division of labor is controlled, the effect of the three causally prior variables will cancel out. To the extent that they do not, direct effects may be attributed to the antecedent variables and Durkheim's model modified accordingly. Industrial diversification accounted for less than half of the variance explained regardless of which criterion was utilized. Of greater importance, however, is the multiple-partial coefficient which represents the proportion of the variation explained by the antecedent variables over and above that explained by the intervening variable, industrial diversification. As is readily apparent in table 2, the antecedent variables jointly explain most of the variance in the property and personal crime rates. This indicates that there is a direct association between the independent variables and the criterion regardless of the division of labor. In order to further clarify these relationships, the correlations between each independent variable and each criterion were partialled on industrial diversification. The partial correlations vary only slightly from the zero-order correlations, providing additional support for the argument that direct effects exist between the independent and dependent variables.⁸

It can be concluded, then, that industrial diversification does not operate as an effective intervening variable, as postulated in the Durkheimian model. It is apparent that population size is more closely related to crime rates than any other variable.⁹ This conclusion casts additional

⁸ These relationships were also examined within each of the nine major regions of the United States. The multiple R ranged from .44 to .67 with Y_1 dependent and from .27 to .72 with Y_2 dependent. The multiple-partial coefficients for each region, however, were similar to those reported in table 2. That is to say, independent variables accounted for most of the variance explained.

⁹ These observed associations between population size and crime rates imply that as communities experience an increase in number of inhabitants, they may also experience an increase in the rate of crimes per number of inhabitants as well as in the absolute number of crimes. This may have important consequences for a community,

doubt on the Durkheimian model, for although he recognized size as an important variable his emphasis was on density, both physical and social. Increased size was seen as necessary but not sufficient for the advancement of functional differentiation.

On the other hand, if we alter Durkheim's model and think of the division of labor as sharing the same time realm as the other independent variables, rather than as intervening between them and deviance, the model has somewhat more utility. Even then, however, the sum effects of all the independent variables in predicting the dependent variables was only moderately successful.

SUMMARY AND CONCLUSIONS

The primary hypothesis—that deviance or crime increases concomitant with an increasing division of labor—was substantiated, albeit at only a low level of predictive efficiency. Of more importance, the Durkheimian model, postulating the division of labor as an intervening variable between population size and physical and social density and the dependent variable, crime, has been largely rebutted.

Perhaps the best test of Durkheim's conceptual scheme was provided by the regression analysis which allowed determination of the total variance which was explained by the united action of the predictor variables, and by the calculation of a multiple-partial coefficient, which demonstrated that the antecedent variables had direct effects on the criterion. These findings imply that Durkheim's model must be modified to include independent effects of population size, physical and social density on the criterion, deviance.

Thus Durkheim's model can neither be said to be verified nor can it be discarded, for when all the variables are allowed to operate jointly, they have differential but direct impact on the dependent variables. These findings, then, raise the question of why the model did not allow for greater prediction than was attained.

Among the usual explanations for the failure of a model to attain perfect prediction are measurement error, an erroneous or incomplete conceptual model, and an inappropriate test model. The latter reason requires some elaboration in explaining the failure of this model to approach unity. As mentioned earlier, the coefficient of determination (R^2) is affected by the heterogeneity of the universe examined. The necessity of ignoring urban units under 25,000, therefore, served to truncate the variance and the explanatory capacity of the independent variables. Thus it must be empha-

for it highlights the fact that a community can expect its crime rate to vary directly with the size of its populace.

sized that this test was limited to a particular part of and not the entire universe in question. Clearly, Durkheim was concerned with ecological communities of all sizes and set no size limits in his theoretical scheme.

The single most important limitation of this study, however, involves a somewhat different conceptualization of the evolutionary process wherein a community progresses toward functional integration and an increased division of labor. Durkheim, of course, was concerned with the evolution and organizational development of individual communities through time, whereas this study examined a universe of several cities—of varying size, density, and diversity—at one point in time and assumed this to be a valid surrogate for the examination of a single city at several points in time. This, of course, is an important limiting assumption, and to the extent it is erroneous it may at least in part be responsible for the failure of the model to be wholly verified. However, until sufficient longitudinal material is available to establish single city trends the present approach seems to provide the only alternative for examining the propositions in question.

These limitations notwithstanding, an inappropriate operational model is probably only partially responsible for the imperfect unity predictions. Of equal importance is the possibility of an erroneous or incomplete conceptual model. Certainly the model examined here is a very simple one, assuming linear relationships between a very few variables. It may well be that several relevant variables are not included in this model and the additive effects of these unknown factors might allow a substantial reduction in the residual prediction error. On the other hand there are those who argue in favor of quite modest working theories (see Gordon 1968, p. 593; Blalock 1968, pp. 196–97).

While Durkheim's model is quite probably "incomplete," this research also raises a question of the validity of its emphasis on density. As noted earlier, Durkheim posited that population size was of consequence to deviance only insofar as the density (physical and social) of the community increased. And in addition, he argued that size and density affected deviance through the division of labor. Yet the findings presented here demonstrate that size is more closely related to crime rates than either density or industrial diversification.

There is a substantial amount of literature which suggests that population size is somehow directly related to crime rates. This relationship is usually couched in terms of an urbanism/urbanization framework in which urbanization is construed as the processual increase in the proportion of the population residing in urban areas. While there is little doubt that most crime rates generally increase with city size, it is the explanation of this association that is of greatest interest. The criminological literature is replete with "explanations," most of which deal with the characteristics

of the social milieu (e.g., high mobility, impersonality, normative conflict, differential opportunities for crime, etc.). In these cases, urbanization is considered primarily as an antecedent variable with the "real" explanation of deviance residing in the characteristics of "urbanism as a way of life."¹⁰

Other explanations of urban crime are also found throughout the literature. Some would have it that high urban crime rates are simply an artifact of differential reporting procedures between rural and urban areas. Alternately, some have attributed the higher urban rates not so much to the criminogenic characteristics of these areas as to the tendency of "deviant" persons to drift to these areas. Perceived differential deterrence may also operate to inflate urban crime rates; that is, the certainty of arrest and sanction being inversely related to population size. Whatever interpretations are utilized to "explain" the association between urban size and deviance, however, most are subsumed under the generic rubrics of urbanism and urbanization. Yet there seems to be little macrosociological research treating the obvious relationship between crime and the increasingly complex form of social organization inherent in and fundamental to urban development (see Webb and Clemente 1972).

In essence, while any or all of the above factors may account for the failure of the model to be completely verified, in reality this failure is most likely a result of all these conditions working together with varying degrees of influence.

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¹⁰ For an excellent review of this literature, see Clinard 1964, 1968.

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Structural Change and the Durkheimian Legacy: A Macrosocial Analysis of Suicide Rates¹

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This paper presents a further specification of Durkheim's theory of suicide. A propositional theory is developed relating population growth, technological development, changes in the division of labor and social integration, and suicide rates. Results are mixed, but are generally as predicted. Strongest support results from the relationships between changes in level of technology, the division of labor, social integration, and suicide rates. Results contrary to the theory were obtained when the effects of population change were considered.

Sociology can claim few instances in which a theoretical formulation has been pursued rigorously over a period of time such that its predictive power, scope, parsimony, and logical consistency are progressively enhanced. The reasons underlying this failure are beyond the scope of the present inquiry, though they may be related to an inordinate emphasis on "unique contributions" in the contemporary sociological ethos and a corresponding lack of reward for explication and synthesis of existing theories. Be that as it may, the present effort may be viewed as an attempt to further specify one of the few sociological theories which has received continuous and constructive attention since its initial presentation, that is, Durkheim's (1951 [1897]) theory of suicide.

The overpowering impact of Durkheim's ideas on subsequent studies of suicide has been noted by a number of writers (Martin 1968). But to acknowledge the widespread recognition and acceptance of Durkheim's seminal contribution is not to say that we cannot benefit from periodic reassessment of this great work. Indeed, recent and enlightening evaluations have been made from the points of view of methodology (Selvin 1965), conceptualization (Dohrenwend 1959; Johnson 1965), and theory construction (Gibbs and Martin 1958a, 1964). The argument to be presented here is, like the efforts of Gibbs and Martin, an attempt to add further precision to Durkheim's exposition of the relation between social

¹ We would like to express our appreciation to Warren Breed for his helpful comments on an earlier version of this paper.

structure and the suicide rate. However, rather than begin with Durkheim's basic theme that "suicide varies inversely with the stability and durability of social relationships" as these writers have done, we shall try to enter the causal chain at an earlier point. We propose to draw from Durkheim's writings an argument that identifies some of the major components of change in social structures which result in variations in the degree of social integration—in the stability and durability of social relationships. The resulting theory will be tested utilizing data for a series of selected countries.

DERIVATION OF THE THEORY

Examination of an argument presented in *Suicide*, considered in the light of *The Division of Labor in Society* (1964 [1893]), can yield a testable theory of the relationship between the rapidity of change in social structure and suicide. In book 2 of *Division*, Durkheim attempted to explain the conditions under which mechanical solidarity gives way to organic solidarity. Population increase was held to play a large part in bringing about this transformation, although its role was a necessary rather than a sufficient cause. Segmentation breaks down in the face of increased moral density. Moral (or dynamic) density refers to an increased frequency of social interaction within a given social unit; physical density refers to the absolute number of people per unit of space. The division of labor, or degree of differentiation, increases as the rate of interaction increases. The rate of interaction (moral density) may increase with increased concentration of population, especially in cities, and with the development of more rapid and numerous means of transportation and communication, which can produce increases in effective moral density, even though no increase in size or concentration is involved. (See Schnore [1965], pp. 8–10, for a discussion of this point.) Differentiation accompanies growth provided that interaction increases concomitantly; but interaction can increase without growth if transportation and communication improve.

Several writers have pointed out that *The Division of Labor in Society* contains the seeds of all Durkheim's later work (Nisbet 1965, p. 44; Schnore 1965). Certainly he did not neglect the topic of suicide in *Division*. In chapter 1 of book 2, entitled "The Progress of the Division of Labor and Happiness," increases in suicide rates are said to be related to the progress of the division of labor. In attempting to demonstrate that happiness does not increase as a result of increased division of labor, Durkheim proposes to regard the suicide rate as a measure of average unhappiness, and contends that "suicide scarcely appears except with civilization. . . . It is rare in lower societies" (1964, p. 246). There are of course many

difficulties with this "suicide-civilization" thesis, not the least of which is the fact that there are substantial differences in the suicide rates of countries at about the same level of civilization. This thesis is not seriously considered in *Suicide*.

Durkheim does not hold that the division of labor is the direct cause of suicide. Further on, we find the following: "Is that to say that it is necessary to impute these sad results to progress itself, and to the division of labor which is its condition? This discouraging conclusion does not necessarily follow from the preceding facts. It is, on the contrary, very likely that these two orders of facts are simply concomitant. But this concomitance is sufficient to prove that progress does not greatly increase our happiness, since the latter decreases, and, in very grave proportions, at the very moment when the division of labor is developing with an energy and rapidity never known before" (1964, pp. 249-50).

There is a suggestion here that the rapidity of the development of the division of labor is connected with an increase in suicide. This might be interpreted to mean that with the degree of division of labor held constant, the suicide rate would vary directly with the rate of change in the division of labor (cf. Moore 1963, pp. 1-44).

A statement with this implication can be found in *Suicide*: "Our social organization, then, must have changed profoundly in the course of this century, to have been able to cause such a growth in the suicide rate. So grave and rapid an alteration as this must be morbid; for a society cannot change its structure so suddenly. . . . Thus, what the rising flood of voluntary death denotes is not the increasing brilliancy of our civilization, but a state of crisis and perturbation not to be prolonged with impunity" (Durkheim 1951, p. 369). It should be noticed here that Durkheim is reasoning in reverse, from "the rising flood of voluntary deaths" to profound changes in social organization. Nonetheless, the following proposition is implied: "Suicide varies directly with the rate of change in the division of labor."

This raises a crucial question. If "suicide varies inversely with the degree of integration of the social groups of which the individual forms a part" (Durkheim 1951, p. 209) and directly with the progression of the division of labor, does the division of labor weaken the integration of society? Durkheim implies that it does. There is a strong resemblance between his description of the mechanical mode of organization in *The Division of Labor* and his description of strong social integration in *Suicide*. (Compare, for example, *Division*, p. 130 and *Suicide*, p. 209.) Further (and more importantly for the present argument), there is some similarity between conditions associated with rapid increase in the division of labor and the "suicidogenic" conditions of weak integration. In *Suicide*, excessive indi-

viduality is regarded as one of the precipitating factors in egoistic suicide, and rapid development of the division of labor is held to produce excessive individuality.

Nisbet (1965) points out how Durkheim changed his original conception of *The Division of Labor* in the course of writing it: "Durkheim could see that, although the conceptual distinction between the two types of solidarity or association was a real one, the institutional stability of the second had to be deeply rooted in the continuation—in one form or another—of the first" (p. 36). Organic solidarity is built upon mechanical solidarity, rather than displacing it.

This suggests an answer to a question raised by the analysis in *Division*. Why do rapid advances in the division of labor predispose toward higher suicide rates? Put simply, this rapid increase tends to break down the mechanical basis of organization which forms the substratum for specialization and functional interdependence. In the terminology of *Suicide*, weak social integration results from such rapid advances.

It is thus possible to piece together, from *Division* and *Suicide*, a theory of the relationships among population growth and technological development, the division of labor, social integration, and suicide.

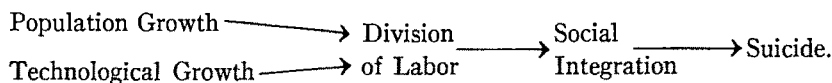
Population growth and technological development produce increased moral density, which contributes to "the progress of the division of labor." The rate at which the division of labor develops is an important variable because it influences social integration. Social integration, which "has to do . . . with the strength of the ties of individuals to society" (Gibbs and Martin 1964, p. 16), is strongest when the society is characterized by mechanical solidarity. Rapid development of the division of labor tends to disrupt the mechanical basis of organization and to weaken the ties of the individual to society. Thus, if suicide varies inversely with social integration, then it should vary directly with the rate of increase of the division of labor.

Stated in propositional form, these relationships are as follows:

- Proposition I: The higher the rate of population growth, the greater the rate of increase in the division of labor.
- Proposition II: The higher the rate of technological growth, the greater the rate of increase in the division of labor.
- Proposition III: The greater the rate of increase in the division of labor, the greater the rate of decrease in social integration.
- Proposition IV: The greater the rate of decrease in social integration, the greater the suicide rate.
- Corollary I: The higher the rate of population growth, the higher the suicide rate.
- Corollary II: The higher the rate of technological growth, the higher the suicide rate.
- Corollary III: The greater the rate of increase in the division of labor, the higher the suicide rate.

In their present form these propositions are framed in terms of rates of occurrence of specified events (e.g., "population growth," "increase in the division of labor," and "suicide" considered over a fixed interval of short duration. There is, however, an alternative form in which they might be stated, that is, in terms of rates of change in the basic measures themselves over a longer span of time. For example, Corollary I would read: "The greater the increase in the rate of population growth, the greater the increase in the suicide rate." For this latter form one might wish, were the requisite data available, to examine the relationships posited by the theory over a 20- or 30-year interval. With a shorter period of time, however, where there is little variation likely in the basic rates, differences in results from the two alternative forms of the theory will be minimal. (For a consideration of some of these issues see Micklin 1970.)

In schematic form, the theory appears as follows:



In this diagram we have not specified the logical connections between social integration and suicide, but would adhere to those specified by Gibbs and Martin (1964). These are discussed below in the section on measuring social integration. It should be further noted that we have not indicated the expected relationship between population growth and technological growth. For one reason, this is not central to our theory, and for another, there are too many potentially confounding variables to be considered here. The following sections of the paper are concerned with operational measurement of the variables and an attempt to test the theory using data derived from several countries.

DATA AND METHODS

The theory has been tested using data from three kinds of units: nations, individual states within the United States, and U.S. SMSAs (Miley 1970). This paper, however, will report only one series of tests: those for countries. The measurement procedures for the variables are discussed below.

Population growth.—Population growth is the only independent variable for which direct measures are available, and these can be expressed in at least two ways: as the average annual or the percentage growth between two points in time. If the time interval is the same for every case, either of these measures is adequate; their rank order will be the same. However, census periods vary from country to country, so that average annual percentage growth was used in this case.

Technological development.—Technology involves the use of energy, and increasing levels of technology are accompanied by increasing use of nonhuman energy sources. This is the rationale for using per capita consumption of energy as the operational definition of technological development. The use of per capita energy consumption has become a fairly standard index for this variable (Saunders and Reinhart 1967; Gibbs and Browning 1966; Gibbs and Martin 1958*b*, 1962; and Marsh 1967). Consumption of energy has been expressed in several ways. Two of the most common are in terms of metric tons of coal or in terms of megawatt hours of electricity per capita. This study uses data for estimated per capita consumption of commercial sources of energy expressed in terms of metric tons of coal. The time interval used is the 10-year period, 1950–60. The rate of change in technological development for countries is operationally defined as the difference in this index between 1950 and 1960. It is not necessary to compute a formal rate of change for this measure since the time interval in each case was the same.

The division of labor.—An increased division of labor involves at least the following elements: a proliferation of specialties; an increase in the ratio of formal to informal norms; an increase in the proportion of occupations devoted to coordination (Mott 1965); and as a consequence of these, an increased probability of role conflict, which provides the theoretical link between the division of labor and the social suicide rate.

The measure of change in the division of labor which was adopted is the "index of dissimilarity." This index has been used by Duncan and Duncan to measure the spatial distance between occupational groups (1955), by Taueber and Taueber (1965) to measure the extent of residential segregation in cities; and by Gibbs (1968) to measure change in industry structure. The computation of this measure is shown in the hypothetical example in table 1. The formula is: $D = A - B/2$. The index of dissimilarity (D) "is then one-half the sum of the absolute values of

TABLE 1
COMPUTATION OF INDEX OF DISSIMILARITY

| AREA | OCCUPATIONS | | DIFFERENCE (%) |
|-------------|-------------|-----|-------------------|
| | A | B | |
| | (%) | | |
| 1 | 10 | 15 | 5 |
| 2 | 20 | 15 | 5 |
| 3 | 40 | 25 | 15 |
| 4 | 30 | 45 | 15 |
| Total | ... | ... | 40 |

SOURCE.—Duncan and Duncan 1955.

the differences between the respective distributions, taken area by area" (Duncan and Duncan 1955, p. 494). In this example, the index is 20 (40/2). This is interpreted as a measure of displacement: 20% of the workers in either occupation would have to move to a different area in order to make the distributions identical.

Taueber and Taueber (1965) have interpreted the measure in essentially the same way, calling it an "index of segregation." Here it is used to show the proportion of Negroes or whites who would have to move to another tract to achieve a racial balance.

Gibbs (1968) has utilized this measure as an index of change in industry structure by substituting occupational categories for areas and the distribution of workers in these categories at two points in time for occupations. The result is an index of change in occupational distribution between two points in time. The computation of the measure for one country is shown in table 2. This measure should be an adequate index of change in

TABLE 2

INDUSTRY COMPOSITION OF ECONOMICALLY ACTIVE MALES, 15 YEARS OF AGE AND OVER, AND INDEXES OF DISSIMILARITY, VENEZUELA, 1950 AND 1961

| INTERNATIONAL NOMENCLATURE | 1950 | | | 1961 | |
|--|---------------|-----------------------|------------------|---------------|-----------------------|
| | Number (1) | Proportion (X) (2) | $ X - Y $ (3) | Number (4) | Proportion (Y) (5) |
| 0. Agriculture, forestry, hunting and fishing | 704,704 | 0.413 | 0.089 | 773,650 | 0.324 |
| 1. Mining and quarrying . . . | 44,509 | 0.026 | 0.006 | 46,675 | 0.020 |
| 2-3. Manufacturing | 172,493 | 0.101 | 0.023 | 294,975 | 0.124 |
| 4. Construction | 91,104 | 0.053 | 0.001 | 128,125 | 0.054 |
| 5. Electricity, gas, water, and sanitary services | 5,219 | 0.003 | 0.008 | 25,425 | 0.011 |
| 6. Commerce | 149,678 | 0.088 | 0.040 | 304,375 | 0.128 |
| 7. Transport, storage, and communication | 52,329 | 0.031 | 0.014 | 106,575 | 0.045 |
| 8. Services | 342,114 | 0.200 | 0.040 | 571,700 | 0.240 |
| 9. Activities not adequately described | 144,171 | 0.084 | 0.028 | 134,725 | 0.056 |
| Total | 1,706,171 | 0.999 | 0.248 | 2,386,225 | 1.002 |

SOURCE.—*Yearbook of Labour Statistics*, vols. for 1955 and 1965. The formula for the index of dissimilarity is $(\Sigma |X - Y|)/2$. For the data in this table this amounts to $.248/2 = .124$.

the division of labor. It utilizes the categories contained in the International Standard Industry Classification (ISIC), though these may be too gross since they do not reveal the emergence of subindustries. Moreover, the measure may reveal the amount of change but not its direction. However, Gibbs's data indicate that "substantial changes in industry structure tend

to take the same direction in all countries" (1968, p. 42). This change is presumably in the direction of more complexity. If this is so, the differences among countries are in the amount of change rather than its direction, and this would mean that the measure is satisfactory in terms of change in industry structure.² Whether it is an equally satisfactory index of the various aspects of the division of labor mentioned above is a different question and one to which we unfortunately have no answer.

Social integration.—Social integration is the most difficult variable to deal with. Landecker (1951) and Gillin (1948), among others, have suggested that several different variables are involved, and each suggests four different types of integration. None of these appears to be measurable, with the exception of Landecker's fourth type, "functional" integration, which refers to interdependence through the division of labor. In the present study this is treated as an independent variable which influences integration rather than as a type of integration.

The decision was made to use Gibbs and Martin's (1964) measure of status integration as an index of social integration. The reasoning behind

² An alternative measure of the division of labor was computed after the initial analysis was completed. This measure was one proposed by Gibbs and Martin (1962), for which the formula is $1 - [\sum X^2 / (\sum X)^2]$, where X is the number of people in each of the nine industry categories of the ISIC. This measure was computed for 1950 and 1960 for each country, and then the 1950 measure was divided into that for 1960 ($D2/D1$). The results were compared with the index of differentiation (I.D.) scores used in the initial analysis, with results which were entirely unanticipated. First, the rank-order correlation (ρ) between the two measures was almost nonexistent ($-.04$). Second, the correlation between the $D2/D1$ measure and the 1960 suicide rates is also nil ($-.03$). Third, the correlations between this measure and the other variables are in the opposite direction from our predictions and the findings of our initial analysis. The correlation between the $D2/D1$ measure and technological development is $-.19$, while that using the I.D. is $.36$. That between the $D2/D1$ measure and the status integration measure for 1960 is $.46$, while that using the I.D. is $-.26$. (These correlations are not strictly comparable, since one is ρ while the other is r ; nonetheless the differences in direction are significant.) Comparing specific examples of the two measures is instructive. For example, Guatemala ranked twenty-fourth on the I.D., but third on the $D2/D1$ measure. Looking at the data for this country, the I.D. ranking makes more intuitive sense, since there was no change in most of the industry categories and small amounts of change in the others. What looks like a small amount of change in the raw data—and the I.D.—turns out to be a large amount when measured by $D2/D1$. Japan, however, ranked first on both measures. In general we find that when the rankings are different, the I.D. rankings jibe more closely with the data and with what we know about the countries in question. However, we have no theoretical reason to believe either measure to be clearly superior to the other. A basic difference between the two is that the I.D. reflects the magnitude of change but not its direction, while the $D2/D1$ measure does reflect the direction of change. Apparently this is related to the lack of correlation between the two measures, since eight of the 24 countries had $D2/D1$ values below unity, which presumably indicates change toward less division of labor. With respect to the correlation with suicide, it may be that the fact of change is more important than the direction of change in the determination of suicide rates. However, we have no substantial explanation for these inconsistent findings.

this decision is as follows. Suicide is related to social integration (Durkheim 1951). Integration "has to do . . . with the strength of ties of individuals to society" (Gibbs and Martin 1964, p. 16). The ties of the individual to society are the strongest when the probability of predicting all of the individual's statuses by knowing one of them is greatest. This probability is highest when the society is characterized by mechanical solidarity and thus has a minimal division of labor. Conversely, it is lowest with an elaborate division of labor.

The status integration measure is essentially an index of the predictability of status occupancy. The measure is highest when all individuals who occupy status X also occupy status Y —that is, when the occupancy of status Y is completely predictable from the occupancy of status X .

The computation of the status integration index involves summing the squared proportions for each column within a tabular cross-classification of statuses (X^2). Separate measures for each column are simply the X^2 s for those columns. An example of the computation of this measure is given in table 3.

TABLE 3
INTEGRATION OF LABOR-FORCE STATUS WITH SEX IN CHILE, 1960 AND
VENEZUELA, 1961 FOR PERSONS 15 YEARS OF AGE AND OVER

| LABOR-FORCE STATUS | CHILE | | VENEZUELA | |
|---|-----------------------|---------|-----------|---------|
| | Males | Females | Males | Females |
| Economically active (in labor force) | $X = 0.851$ | 0.227 | 0.895 | 0.211 |
| | $X = 0.149$ | 0.773 | 0.105 | 0.789 |
| Not economically active (not in labor force) | $\Sigma X = 1.000$ | 1.000 | 1.000 | 1.000 |
| | $\Sigma X^2 = 0.746$ | 0.650 | 0.812 | 0.666 |
| | $\Sigma \Sigma X^2 =$ | 1.396 | | 1.478 |

NOTE.—The model for this table may be found in Gibbs and Martin (1964), table 41, p. 122. The source of data for these countries is the *Demographic Yearbook* (1964), table 8, p. 190.

Suicide.—Suicide rates for countries are published by the World Health Organization and are also given in some issues of the *Demographic Yearbook*. The specific sources of suicide rates are cited in table 4. The adequacy of official suicide statistics has been questioned by Douglas (1967). The argument presented there is neither refutable nor demonstrable, since it is based on plausible conjecture rather than evidence. Actually, there is very little evidence bearing on this question. As a result, an assessment of the reliability of suicide rates in general is not possible; only particular rates may be assessed, and these may or may not be found adequate. But the adequacy of the suicide rate for a particular country or city at some

point in time has little bearing on the adequacy of any other rate at any other point in time. Likewise, the possibility that officials in charge of classifying deaths may have quite different definitions of suicide, a point which Douglas belabors, has a completely unknown influence on the suicide rate of any population. The position we have taken is that the question is still open. If a theory of the present type is to be tested, official suicide statistics must be used. There is no alternative.

Test of hypotheses.—Hypotheses are tested primarily using the Pearsonian product-moment coefficient of correlation (r). This statistic has the advantage of allowing the computation of multiple and partial correlations, for example, the relationship between population growth and the division of labor, controlling for level of technological development, which would provide a test of Propositions I and II in combination. The disadvantages of the use of this measure in the present context are: (1) r assumes a linear relationship, whereas there may be curvilinear relationships between the variables in the theory; (2) tests of significance for this measure assume random sampling, not utilized in this study, since availability of data was the main criterion used in the selection of the sample. In a few instances we have utilized Spearman's coefficient of rank-order correlation (ρ). For discussions of both these statistics see Blalock (1960).

INTERNATIONAL COMPARISONS: SELECTED COUNTRIES

The theory was tested using the data shown in table 4. For these data the matrix of zero-order correlations is shown in table 5. The status integration measures used in these tests are measures of the integration of labor-force status with sex. The computation of this measure is shown in table 3 above. The figures from 1950 were drawn from Gibbs and Martin (1964), while we computed the figures for 1960 based on the information in table 6. For purposes of comparison we began with the countries used by Gibbs and Martin (1964) in testing the status integration theory at that level. Data limitations reduced the original list of 32 countries to the 24 shown in table 4.

Proposition I predicts a direct relationship between population growth and the division of labor. For the indicators used here, the correlation (r) is $-.16$, which is a reversal of the prediction. This reversal is discussed below. It should be noted here, however, that for the present series of tests, there is practically no relationship between population growth and technological development ($r = .02$). We had expected these variables to be directly related, although this was not stated in the theory. It was felt that their combined influence would be responsible for change in the division of labor. This is a direct extension of Durkheim's argument in *The*

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TABLE 4

AVERAGE ANNUAL POPULATION CHANGE, CHANGE IN PER CAPITA CONSUMPTION OF ENERGY, INDEX OF DISSIMILARITY SCORES BASED ON LABOR-FORCE DISTRIBUTIONS, INTEGRATION OF LABOR-FORCE STATUS WITH SEX, AND SUICIDE RATES

| COUNTRIES | ANNUAL AVERAGE POPULATION CHANGE, 1950-60 | CHANGE IN PER CAPITA ENERGY CONSUMP- TION, 1950-60 | INDEX OF DISSIMI- LARIY | INTEGRATION OF SEX AND LABOR-FORCE STATUS | | SUICIDE | |
|--------------------|---|--|-------------------------------|---|-------|---------|------|
| | | | | 1950 | 1960 | 1950 | 1960 |
| Mexico | 3.1 | 0.32 | .075 | 1.553 | 1.747 | 1.0 | 1.8 |
| Guatemala | 3.2 | 0.03 | .035 | 1.664 | 1.639 | 1.5 | 3.0 |
| Ireland | -0.5 | 0.74 | .060 | 1.340 | 1.410 | 2.5 | 2.7 |
| Costa Rica | 3.8 | -0.02 | .073 | 1.623 | 1.544 | 2.6 | 2.4 |
| Greece | 1.0 | 0.35 | .066 | 1.493 | 1.230 | 3.6 | 3.8 |
| El Salvador | 2.8 | 0.04 | .054 | 1.605 | 1.550 | 4.1 | 11.3 |
| Chile | 2.5 | 0.08 | .046 | 1.394 | 1.396 | 4.4 | 7.6 |
| Venezuela | 3.7 | 1.85 | .124 | 1.521 | 1.478 | 4.7 | 6.4 |
| Netherlands | 1.3 | 0.73 | .117 | 1.355 | 1.362 | 7.0 | 6.8 |
| Norway | 0.9 | -1.63 | .068 | 1.375 | 1.350 | 7.2 | 7.5 |
| Canada | 2.7 | -0.81 | .097 | 1.347 | 1.240 | 7.4 | 7.5 |
| Israel | 5.3 | 0.44 | .061 | 1.361 | 1.244 | 8.6 | 7.0 |
| Australia | 2.3 | 0.80 | .107 | 1.404 | 1.344 | 9.7 | 11.3 |
| Portugal | 0.5 | 0.12 | .057 | 1.442 | 1.554 | 10.1 | 8.8 |
| New Zealand | 2.2 | -0.30 | .051 | 1.338 | 1.344 | 10.3 | 9.7 |
| United States | 1.8 | 0.31 | .081 | 1.255 | 1.210 | 10.9 | 10.7 |
| France | 0.9 | 0.39 | .077 | 1.255 | 1.202 | 12.4 | 16.4 |
| Belgium | 0.6 | 0.47 | .143 | 1.317 | 1.256 | 14.7 | 14.2 |
| Sweden | 0.6 | 0.27 | .075 | 1.336 | 1.224 | 15.6 | 17.6 |
| Finland | 1.0 | 0.48 | .116 | 1.284 | 1.234 | 16.4 | 20.6 |
| Japan | 1.1 | 0.39 | .168 | 1.224 | 1.248 | 17.9 | 23.2 |
| Austria | 0.2 | 0.62 | .095 | 1.236 | 1.176 | 23.4 | 23.7 |
| Denmark | 0.7 | 0.74 | .076 | 1.295 | 1.284 | 23.9 | 20.8 |
| West Germany ... | 0.9 | 1.12 | .118 | 1.251 | 1.238 | 17.9 | 18.8 |

Source.—Population data from Davis (1969), table D, pp. 139-60. Energy consumption figures from *U.N. Statistical Yearbook* (1953, table 127; 1964, table 131); 1950 status integration measures from Gibbs and Martin (1964), table 42, p. 124. Status integration measures for 1960 computed by us from labor-force data in the *Demographic Yearbook* (1964), table 8. These data are shown in table 6. Suicide rates circa 1960 from *Epidemiological and Vital Statistics Report* (1962), table 2. Rates are three-year averages for 1958, 1959, and 1960. The 1960 rates for Mexico and Chile are from the *Demographic Yearbook* (1966), table 20, for 1960 only. Index of dissimilarity scores were computed from labor-force composition data in *Yearbook of Labour Statistics* (1955, 1965, 1966). See table 2 for the computation of this measure.

Division of Labor, but the present results are inconsistent with that position.

Turning to Proposition II, which predicts a direct relationship between technological change and change in the division of labor, we find that for these two variables the correlation (r) is .36, which is in the predicted direction.

Proposition III predicts an inverse relationship between change in the division of labor and change in status integration. The correlation between the indexes of dissimilarity for the various countries and the 1950 status

TABLE 5
MATRIX OF CORRELATIONS BETWEEN SEVEN VARIABLES FOR 24 COUNTRIES

| | POPULATION GROWTH | TECHNOLOGICAL DEVELOPMENT | INTEGRATION OF SEX AND LABOR-FORCE STATUS | | SUICIDE | | CHANGE IN DIVISION OF LABOR |
|--|----------------------|------------------------------|--|--------|---------|--------|--------------------------------|
| | | | 1950 | 1960 | 1950 | 1960 | |
| | | | | | | | |
| Mean | 2.27 | 1.94 | 1.38 | 1.35 | 9.90 | 10.98 | 0.08 |
| SD | 1.35 | 0.63 | 0.12 | 0.15 | 6.52 | 6.71 | 0.03 |
| Correlation with: | | | | | | | |
| Population growth | 1.0000 | ... | ... | ... | ... | ... | ... |
| Technological development | -.0210 | 1.0000 | ... | ... | ... | ... | ... |
| Integration of sex and labor-force status, 1950 | .5672 | -.0999 | 1.0000 | ... | ... | ... | ... |
| Integration of sex and labor-force status, 1960 | .4048 | -.0575 | .8345 | 1.0000 | ... | ... | ... |
| Suicide, 1950 | -.5090 | .2154 | -.7658 | -.6749 | 1.0000 | ... | ... |
| Suicide, 1960 | -.4872 | .2170 | -.7144 | -.6354 | .9416 | 1.0000 | ... |
| Change in division of labor | -.1612 | .3611 | -.4440 | -.2635 | .4102 | .4414 | 1.0000 |

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TABLE 6

ECONOMICALLY ACTIVE OVER 15, BY SEX, FOR 24 COUNTRIES CIRCA 1960

| Country | Year | Lower Age | Male | Female |
|---------------------|------|-----------|------|--------|
| Mexico | 1960 | 15 | 92.4 | 19.7 |
| Ireland | 1961 | 15 | 84.2 | 28.9 |
| Costa Rica | 1963 | 15 | 90.8 | 17.5 |
| Greece | 1961 | 15 | 80.6 | 35.5 |
| El Salvador | 1961 | 15 | 92.4 | 19.2 |
| Chile | 1960 | 15 | 85.1 | 22.7 |
| Venezuela | 1961 | 15 | 89.5 | 21.1 |
| Netherlands | 1960 | 15 | 82.6 | 22.6 |
| Norway | 1960 | 15 | 82.6 | 23.8 |
| Canada | 1961 | 15 | 78.1 | 29.7 |
| Israel | 1961 | 14 | 76.7 | 27.5 |
| Australia | 1961 | 15 | 85.7 | 28.9 |
| Portugal | 1960 | 15 | 91.0 | 17.0 |
| New Zealand | 1956 | 15 | 83.7 | 26.0 |
| United States | 1960 | 15 | 78.9 | 35.1 |
| France | 1962 | 15 | 78.6 | 36.2 |
| Belgium | 1961 | 15 | 76.0 | 25.5 |
| Sweden | 1960 | 15 | 78.6 | 32.7 |
| Finland | 1960 | 15 | 84.2 | 48.5 |
| Japan | 1955 | 15 | 85.2 | 50.6 |
| West Germany | 1961 | 15 | 83.3 | 41.1 |
| Austria | 1961 | 14 | 79.1 | 44.5 |
| Denmark | 1960 | 15 | 85.2 | 36.5 |
| Guatemala | 1964 | 15 | 92.8 | 13.6 |

SOURCE.—*Demographic Yearbook* (1964) table 8, p. 190.

integration measures is $-.44$. The correlation between the indexes of dissimilarity and the 1960 status integration measure is $-.26$. These relationships are both in the predicted direction and indicate that change in the division of labor is negatively associated with the measure of status integration. However, the proposition predicts that the higher the rate of increase in the division of labor, the greater will be the decline in social integration. The differences in the status integration measures were for the most part quite small. For this reason, no change rates were computed in the initial analysis. A later analysis, however, has shown that there is essentially no relationship ($\rho = -.06$) between change in the integration of labor-force status and sex and the indexes of dissimilarity.

The prediction from Proposition IV is that "the greater the decrease in social integration, the higher the suicide rate." For this period the mean status integration declined from 1.38 to 1.35, while the mean suicide rate increased from 9.90 to 10.98. The correlation between the status integration (S.I.) scores and suicide rates for 1950 and 1960 are fairly strong and provide additional support for Gibbs and Martin's status integration theory. The correlation for the 1950 figures is $-.77$, while that for the 1960 figures is $-.64$.

The procedure used to test the proposition was simply to indicate which countries showed an increase in status integration and which showed a decrease. Increases or decreases in the suicide rate for the same time period were also noted. Each case in which the status integration measure declined while the suicide rate increased, or vice versa, was regarded as supporting the proposition, while each case in which an increase in status integration was accompanied by an increase in suicide, or vice versa, was regarded as not supporting the proposition. Fifteen cases supported the proposition, while nine did not. Of the 15 supporting cases, Australia, Austria, Canada, El Salvador, Finland, France, Greece, Guatemala, Norway, Sweden, Venezuela, and West Germany showed a decrease in status integration and an increase in suicide, while the Netherlands, New Zealand, and Portugal showed an increase in status integration and a decrease in suicide. Of the negative cases, Chile, Ireland, Japan, and Mexico showed an increase in both variables, while Belgium, Costa Rica, Denmark, Israel, and the United States decreased on both variables.

Turning to the corollaries, we find that the correlation designed to test Corollary I is in the opposite direction from that predicted. The prediction is for a direct relationship between rates of population growth and suicide rates. However, the correlation between population growth and suicide for 1960 is $-.49$, a direct reversal of the prediction.

The tests of Corollary II are in the predicted direction. The correlation (r) between technological change and suicide for 1960 is $.22$. A multiple coefficient of correlation combining the first two corollaries was computed, treating the 1960 suicide rates as the dependent variable and population growth and technological development as the independent variables. The resulting R is $.54$.

With respect to Corollary III, which predicts a positive relationship between the rate of increase in the division of labor and suicide rates, the present test resulted in a correlation of $.44$ between the index of differentiation scores and the 1960 suicide rates. This is in the predicted direction, and appears to give some support to this corollary.

SUMMARY

The testing of this theory was launched by a reversal of the prediction of Proposition I for a positive correlation between population change and change in the division of labor.

Proposition II, predicting a direct relationship between change in technological development and the division of labor, was supported at the level of $r = .36$.

With respect to Proposition III, a rank-order correlation showed essentially no relationship between change in the division of labor and change

in status integration, although there were positive r 's between change in the division of labor and level of status integration.

The prediction from Proposition IV was an inverse relationship between change in social integration and suicide. The correlations between status integration and suicide for 1950 and 1960 were quite strong, but did not provide a direct test of the proposition. To provide such a test, the direction of change in status integration and suicide was noted for each country. Each case in which these variables changed in the opposite direction was regarded as supporting the proposition. There were 15 such cases among the 24 countries.

The prediction of Corollary I for a direct relationship between population change and suicide was reversed. The prediction of Corollary II was supported by a low positive correlation between technological development and suicide. Corollary III was also supported by positive r 's between change in the division of labor and suicide rates.

The general picture presented by these tests is mixed. Two of the propositions are supported (II and IV), one is reversed (I), and one is partially reversed (III). Of the corollaries, one is reversed (I) while two are supported (II and III).

It is interesting that both predictions (Proposition I and Corollary I) containing population change as the independent variable are reversed. For Corollary I this may be related to the unexpected positive relationship between population change and status integration.

DISCUSSION

One important difference between the present theory and the Gibbs-Martin status integration theory is that while both are based on causal chains, the Gibbs-Martin theory deduces only one testable proposition from a series of untestable ones, whereas the present theory is testable at every point. This, however, is not an unmixed blessing.

If the variables of the theory operate in the posited causal sequence, and if there are uncontrolled variables operating at every point in the chain, then the correlations between adjacent variables (e.g., technological development and the division of labor) should be higher than those between nonadjacent variables (e.g., technological development and suicide). However, the highest correlation in the predicted direction is between two nonadjacent variables—change in the division of labor and suicide. Thus with respect to this logical expectation, our results are ambiguous.

An additional implication of this form of theorizing is that there are actually several theories contained in the causal chain: the theory of suicide is not the only output from the present set of propositions and corollaries. Durkheim's theory of the development of the division of labor

(or more accurately, our interpretation of it) is contained in this chain and is tested by the observed correlations. Additionally, there is a theory of social integration within the theory of suicide. The only variables which are not dependent in any of the propositions and corollaries are population growth and technological development.

It should be noted that all the possible derivations are not contained in the formal statement of the theory. For example, the corollary "the higher the rate of population growth the greater the decrease in social integration" may be logically derived from Propositions I and III. It was thought more advisable to limit the derivations to the three corollaries, all of which have suicide rates as their dependent variable, and otherwise to view the relationships among the variables in terms of a sequential chain in which each variable is influenced by the one immediately before.

In conclusion, the present paper has attempted to deduce from the writings of Durkheim a propositional theory of variation in suicide rates and to put the theory to a series of tests. The theory has conceived rates of change in the relevant variables to be crucial in the explanation of variation in rates. The tests were largely positive, though some were not direct tests of the theory.

As a final note, it should be apparent that the present testing procedure is not the only way of testing the theory. Many other procedures are possible, some no doubt superior to the one used. It is our hope that the results of the present tests will stimulate others.

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Commentary and Debate

COMMENTARY ON RACIALLY CHANGING NEIGHBORHOODS

Observers have long believed that whites tend to flee when blacks move into their neighborhoods. Harvey Molotch (1969), followed by Avery M. Guest and James J. Zuiches (1971), presented a refreshing alternative interpretation of the flight of whites from the inner city, namely, that these areas generate a good deal of outmigration for other reasons.

Molotch deserves ample credit for opening this area of inquiry with his case studies of Chicago's South Shore and Rogers Park. Guest and Zuiches are to be commended for their application of statistical controls to a sample of census tracts in Cleveland, Ohio. Unfortunately, they committed the very sin for which they criticized Molotch. That is, they biased their results in favor of the interpretation that white outmigration could be attributed to nonracial causes by failing to consider black movement into white areas as an independent variable until after they had allowed all other variables to have maximum effect. If anything, they should have biased the analysis in favor of the "invasion" hypothesis, since, as critics, the burden of proof was on them.

Unwittingly, the authors also made several serious errors of causal specification. First, their regression model made the unwise specification that high population density and more industrial land use make people remain in a neighborhood. Second, they specified in their model that the decline in the percentage white collar in inner-city communities caused higher mobility from these neighborhoods, when indeed the causation ought to work the other way around, since our theory tells us that lower-status people follow higher-status people in ecological succession. Third, their other "control variables" are the very factors which caused racial succession by engendering high mobility rates in the past. Removing the effects of its causal antecedents naturally reduces the remaining variance to be explained by racial change.

The Guest-Zuiches article was based entirely on census tract data. However, urban segregation patterns do not have to follow the specific boundaries drawn by the Census Bureau. A partly black census tract can be rigidly segregated by block, though the tract data will mask this. Any microanalysis of neighborhood segregation ought to examine units small enough to reveal the patterns. The Guest-Zuiches article is still valuable, providing that authors and readers bear in mind these limitations.

Further, the authors committed *the fallacy of misplaced sampling inference*. They drew a sample of largely stable, segregated census tracts

and made inferences to a population of unstable, invaded census tracts. Examination of only the 10 tracts which they label as "invaded" leads to very different conclusions. The mean mobility rate of these tracts was 1.186 standard deviations above the mean mobility rate of the whole sample. After the other independent variables were allowed to have their full effect, these tracts were still 0.412 standard deviations above the predicted mean mobility rate. If the "invasion" hypothesis had a fair chance, it would have fared still better. After presenting their statistical analysis, Guest and Zuiches proceed to inspect the 10 changing tracts. They failed to realize the significance of the fact that the two racially changing tracts with the largest number of blacks had the highest mobility rates: 2.619 and 2.495 standard deviations above the mean. Even after allowing the other variables to have their maximum effect, these tracts had mobility rates 1.536 and 1.804 standard deviations above what was predicted. For these tracts, the other variables left very large unexplained residuals. Clearly, a larger sample of changing tracts is needed to make more general inferences.

The correlation between the dummy variable "racial change" (based on coding the 10 "invaded" tracts as 1 and the other tracts as 0) and the migration rate was computed as only .27. This confirms the conclusion that black penetration explains little of the variance in migration from 189 central-city census tracts in Cleveland. However, that should not lead anyone to conclude that black penetration has little effect on white migration from changing neighborhoods. In fact, penetration explained more of the migration from changing tracts than any other variable. The only reason the correlation between racial change and migration was relatively small *over the whole sample* was that there were very few changing tracts.

From these data, we can hardly conclude that racial integration is at hand or that racial segregation occurs by coincidence. On the contrary, it appears that segregation is so rigid that ecological competition between the races does not determine overall migration rates in the inner city. It also appears that whites and blacks can get fairly close before the former are motivated to flee. Guest and Zuiches do teach us something important, namely, that *anticipatory* white flight does not occur in Cleveland very much¹ and that ecological competition between the races does not greatly affect overall migration rates in the inner city. But black movement into white areas, once it has occurred in a specific neighbor-

¹ That is, anticipatory white flight does not take place in adjacent census tracts. However, within a given census tract, anticipatory white flight might be very important in blocks adjacent to "invaded" blocks.

hood, does lead to white flight.² If this were not so, how could cities become and remain so segregated?

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REPLY TO FELSON

The utility of extended debate over the emphasis on a minor correlation or an admittedly problematic inference seems questionable. On the other hand, if sociology is to benefit by cumulative empirical research of its substantive problems, this continued dialogue is justified.

In our article (Guest and Zuiches 1971), we argued that Molotch (1969) failed to show that white flight did not occur in South Shore, a racially changing community in Chicago. Given white racial fears, it is possible for whites to move quickly out of a neighborhood, more quickly than would be predicted by a knowledge of the demographic and housing characteristics of the area. Rapid turnover could be encouraged by any variety of incidents, such as panic selling of a few homeowners or the organized actions of realtors.

Our attempt to test the hypothesis of no difference between neighborhoods beyond the conditions of housing and nonracial social characteristics in another metropolitan context provided an extension of Molotch's work from a case study to a multivariate analysis of numerous subareas in a large city, albeit not defined by participant observation; and, in addition, a replication, in a broad interpretation of the word, that could be repeated in other cities; and finally, a generalizability beyond South Shore, Chicago. We showed, moreover, that much of the residential turnover in Cleveland's racially changing neighborhoods could be explained by the demographic and housing character of the areas. Some neighborhoods, however, had unusually high mobility even after controls for the demographic and housing characteristics. We left open the question of why these areas had "excess" turnover.

² I leave open the question of *how much* black penetration in a particular neighborhood is necessary to spark a white exodus.

Most of Felson's comments are statistical and methodological, and these will be discussed in the first part of our reply. We shall also discuss our views on the question which seems to be implicit in many of Felson's comments, particularly: what implications do our findings have for the achievement of racial integration in cities?

First, as Felson points out, it is true that we tested the hypothesis that all mobility in racially changing tracts could be explained by nonracial features of the neighborhood. This test of Molotch's hypothesis meant that we determined the mobility rates of changing tracts based on their nonracial characteristics and then compared them with the actual rates to determine whether excessive housing turnover (indicative of white flight) might exist. Although we did not report our results in the paper, we also tested the hypothesis which he suggests—that mobility rates could be explained by both nonracial and racial characteristics (dummy variables for stages of racial change). We determined whether the stages of racial change had any statistical relationship to mobility after we controlled for the nonracial variables. They did, but the conclusions were roughly the same regardless of our manipulation of the data. Felson criticizes us for being unfair to Molotch, but we were trying to be fair by testing Molotch's hypothesis. Moreover, it is not a question of fairness, but of replication and extension of a significant research endeavor by Molotch.

Second, we do not believe that several serious errors of causal specification were made. Contrary to Felson, we made no claim that high population density and a large amount of industrial land keep people in a neighborhood. The data in our paper and ecological theory would suggest otherwise. We also find arcane the meaning of Felson's contention that high mobility rates cause a decline in the status of an area. We prefer our interpretation that a neighborhood with declining social status should generally become less attractive to its residents.

Furthermore, on the matter of causal specification, Felson seems to be suggesting a "correct" causal model which involves the effects of our "control" variables on the mobility rate and, in turn, the effect of the mobility rate on racial succession (or high mobility causes racial succession). This is a possible model. And, in fact, we would argue that racial change can occur because of high white outmovement in an area. If this is Felson's model, then racial succession becomes the dependent variable, and the last sentence in his paragraph on causal models seems to be completely confused to us. Our own modest empirical work involved the test of another possible model (Molotch's)—that most white outmovement could be explained by nonracial factors.

Third, we agree strongly with Felson that researchers ought to bear in mind the limitations of their ecological units of analysis. In fact, we criticized Molotch for that very point. During our extensive correspon-

dence, Molotch suggested that the next study in this area should be based not on housing units (or areal units) but on individual residential histories, and we would pass this suggestion on to Felson. Straits (1968) analyzed residential succession in this fashion, with some interesting Chicago-specific results.

Fourth, Felson points to a discrepancy between the sample of tracts and the subset that were under consideration as "invasion" tracts. In the first place, only 89 of the 189 tracts were stable, white tracts, and we provided detailed description of our criteria for including and categorizing tracts; second, by including tracts with a full range of racial compositions, we have as a standard the average mobility for the entire community, irrespective of racial composition or housing characteristics. It is true that the racially changing tracts had high mobility rates, even after controls for nonracial variables, in comparison with the dispersion of mobility rates (standard deviation) for all tracts. But emphasis on this point is deceptive because there was little dispersion of mobility rates in the first place. The mean mobility rate was 54.3%, with a standard deviation of only 9.7. In absolute percentage terms, the predicted mobility rates of racially changing tracts were not very different, on the average, from the actual rates for those tracts, that is, only four percentage points lower.

Fifth, it is true that the two tracts with the greatest excess in mobility also had the highest percentage nonwhite population in 1960. Contrary to Felson, we do mention this in the last paragraph on page 465. There is, however, little systematic relationship with the percentage nonwhite in 1960 and the differences between actual and predicted turnover.

Sixth, we did not conclude that black movement into a tract had little effect on mobility in relationship to any other variable. We made no statement on that point. Our concern was primarily with the effect of racial versus nonracial variables in explaining mobility. Furthermore, we made nothing in the paper of the low correlation (r) between the dummy variable for "invasion" tract and the mobility rate. Felson's interpretation of that correlation is correct; we never argued otherwise.

Seventh, Felson points out that "we should not conclude that racial integration is at hand." We did not conclude that. However, our results do have some implications for understanding the possibilities of achieving racial integration.

Implications

Molotch's paper and ours suggest that greater racial integration of cities is a possibility given the existence of structural conditions in racially changing neighborhoods. Both papers also made thinly veiled references in the last paragraphs to other factors which might affect the possibility

of "white flight" in neighborhoods and the speed of racial change. We would briefly present a framework for understanding how more neighborhood integration might occur in cities, based on the general goal of stabilizing racially changing neighborhoods. Using the racially changing neighborhood as a partial focus of racial integration schemes is not a farfetched idea, since the process of ghetto expansion, particularly in the North, occurs primarily through movement into older white-occupied areas. The outline of our scheme is presented in table 1.

Any goal of neighborhood racial integration must deal with four components of population movement: the inflow and outflow of blacks and the inflow and outflow of whites into a neighborhood. To maintain at any point in time a racially integrated neighborhood, we would want to maximize the inflow of whites and minimize their outflow. High inflow of whites into racially changing neighborhoods is particularly important, since it appears that much of racial change is due to the failure of whites to move in rather than any panic outmigration. This is suggested both by Molotch's article and by our article, and is also implicit in work by Wolf and Lebeaux (1969) and Mayer (1960) on Detroit neighborhoods.

As a general rule, we would want to minimize but not eliminate the inflow of blacks into specific transition neighborhoods. Goals for the fourth population component, the outflow of blacks, are more difficult to determine. If the outflow of blacks became high, the neighborhood would become all white again. On the other hand, if the outflow became very low, a minimal inflow of blacks would still lead eventually to an all-black area.

There are, in turn, general types of policies which could be used to achieve these types of population movement in transition neighborhoods. Some are readily applicable in today's society. Some of these policies are also discussed by Wolf and Lebeaux (1969, pp. 501-21).

In the area of housing policies, it appears that much of the black inflow could be stemmed by the development of alternative housing opportunities. As the Taeubers (1965, pp. 4-5) point out, housing in transition areas tends to be better in quality and space than housing in the established ghetto. Black immigration into transition areas is particularly selective of higher-status families and families of child-bearing age (Edwards 1972; Guest 1970), and thus alternative housing policies should be directed toward these groups. This type of goal would seem to be relatively feasible in our society. Hopefully, these alternatives, assisted by open-housing policies in new suburban developments to eliminate dependence on the "filter down" effect in the real estate market and increased access to mortgage financing, would be developed so they would also lead to residential integration in other neighborhoods.

It also appears that housing in transition neighborhoods is less attrac-

TABLE 1
POSSIBLE POLICIES TO ACHIEVE NEIGHBORHOOD RACIAL STABILIZATION

| Population Flow | Goal | Housing Policy | Institution Policy | Demographic Policy |
|---------------------------------|---------------------|---|---|---|
| 1. White immigration | Maximize | Neighborhood rehabilitation, rent supplements | Improve stores, schools, recreation areas | None feasible |
| 2. White outmigration | Minimize | As above | As above | None feasible |
| 3. Black immigration | Minimize | Create more housing opportuni- ties outside changing neighbor- hoods, particularly for certain types of families | Possibly increase facilities outside transition neighborhood | Disperse black population to more cities |
| 4. Black outmigration | Moderate amounts | As above | As above | As above |

tive to whites than alternative housing opportunities. As a result, the outflow of whites could be stemmed by decreasing the amount of new, quality housing available to the white population outside the changing area. However, this goal is clearly not feasible in a political sense. Nevertheless, it would be possible to *increase* the attractiveness of white housing in changing neighborhoods relative to other white housing through special policies such as rehabilitation, liberal mortgage terms, and special rent supplements to families willing to live in integrated neighborhoods. Some of these factors are discussed by Rapkin and Grigsby (1960, pp. 114-21).

A second policy area would involve community institutions. Some evidence (Mayer 1960) suggests that whites leave changing areas due to the lack of good stores, shopping centers, and schools. A prime goal would thus be the improvement of these facilities in racially changing areas, benefiting all residents in the area.

A third policy area, the demographic, would focus on the flows of blacks and whites into the city itself. As the Taeubers (1965, p. 4) point out, neighborhoods must change racial character if the black population of a city continues to expand, since almost all housing possibilities are located in changing neighborhoods. In 1970, four out of every 10 Negroes in the United States were living in the 30 cities with the largest Negro population (U.S. Bureau of the Census 1971, p. 17). This percentage of the total Negro population of the United States residing in the 30 selected cities has shown a steady increase from 30% in 1960. Thus, a feasible policy for residential integration would attempt to shift the flow of the black population to other cities so the pressure on housing supply does not grow. This policy, of course, could be implemented by such grandiose schemes as "new towns" or by more mundane alternatives such as creating employment opportunities in already existing (primarily white) cities.

Of course, these proposals apply primarily to the stabilization of racially changing neighborhoods. The stabilization of these neighborhoods would make only a small dent in the pervasive pattern of racial residential segregation in American cities. And the achievement of real residential integration, on racial grounds alone or on both racial and socioeconomic grounds, will require much broader policies than we have sketched here. Racial residential segregation is in the final analysis only one aspect of broad types of American racial practices which must be ended. Anthony Downs (1968) has developed, with a much wider scope than simply transition neighborhoods, alternative programs encompassing both dispersal and enrichment policies for the American ghetto.

Since publication of our original work on Cleveland, we have been investigating the speed of neighborhood racial change and rates of white

outmovement in various changing neighborhoods. There appear to be some striking variations in these patterns over cities. Perhaps eventually we shall be able to report in more detail the nature of these patterns and the reasons for them.

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WHY NEIGHBORHOODS CHANGE: A REPLY TO WHOM IT MAY CONCERN

I tried to demonstrate (Molotch 1969) that black immigration, once it does occur, does *not* necessarily lead to white flight—even when it does lead to relatively rapid racial transition. Based on a single case study, my findings are necessarily bounded by time, place, and other circumstances. I took my results to indicate that, unlike the 1940s and 1950s, the period upon which the bulk of our racial-change literature is based, new interethnic dynamics were at work in the middle and late 1960s which inhibited certain manifestations of white racism. The Guest and

Zuiches study is based on 1950s data. That their results seemed to confirm my original findings came to me as a (happy) surprise.

Felson's arguments lead me to think that indeed, the South Shore pattern represents a new trend in racial-change processes. But Felson's final question leads me to think that he considers this new pattern a logical impossibility. For Felson, whites must continue to flee changing communities, or else "how could cities become and remain so segregated?" I want to attempt an answer to this interesting question.

Segregation persists not because whites flee but because blacks are discriminated against. Discrimination against blacks means that there are two separate, parallel housing markets in American cities, one white, one black, each with its own supply-demand relationship. The two markets are not symmetrical in that while whites have access to the entire housing stock of a metropolitan area, blacks are more severely curtailed in their purchasing/renting opportunities.¹ Thus, the supply-demand ratio is higher for whites than for blacks; that is, prices (rents and purchases) are more costly to blacks than to whites by virtue of the fact that a large portion of the available housing stock is arbitrarily removed from the Negro market. A changing area is, by this perspective, an arena where the Negro and white housing markets merge. In this kind of setting, any units which may fall vacant, and which are available to both races, will fetch higher prices from blacks than from whites by dint of market circumstances. Thus, such units will become disproportionately occupied by blacks, and hence the community will, over time, undergo a complete transition from white to black. White flight will accelerate this process by increasing the number of vacancies and hence the number of units subject to this dynamic, but such flight is not necessary for the basic process to occur.

Under present conditions, a situation of stable racial integration can come about only when arbitrary racial quotas are fixed and enforced through administrative intervention (as is common in urban-renewal areas) or where there is a *peculiar* attractiveness of a community to whites but not to blacks. In such a setting (e.g., Hyde Park adjacent to the University of Chicago), integration is made possible because, in effect, everybody pays Negro prices.

The reason this model may not seem obvious is because it rests upon the sometimes forgotten fact that urban communities are highly mobile

¹ This disparity between the two housing markets is documented through a survey of Chicago-area real estate personnel, as well as other methods described elsewhere (Molotch 1972). The model of racial change described here is also consistent with the arguments and data of Taeuber and Taeuber (1965, esp. p. 25) and, as argued in Molotch (1972), with the great bulk of relevant evidence on housing prices and race (e.g., Laurenti 1960; Muth 1969) and consumer racial behavior (e.g., Grier and Grier 1960).

places, the protestations of a relatively small number of community "old-timers" notwithstanding. Approximately one out of every five American households change location every year. If such moving behavior were evenly distributed among the population, a complete transition of a community could occur over a mere five-year period without anybody fleeing from anybody. It takes somewhat longer because such moving behavior is not evenly distributed and because a declining proportion of the replacement purchasing and renting is by whites moving into a changing area (cf. Rapkin and Grigsby 1960). I suspect that some "racial moving" does occur, but less and less. This is the racial-change pattern which I think I witnessed in South Shore and which I think may be the general emerging pattern of the 1970s. To get back to Felson's question, segregation will persist but white flight may not be the cause. As I argue elsewhere (Molotch 1972, chap. 10), the policy implication is that stable racial integration is essentially contingent upon elimination of discrimination and all other forces which inhibit blacks from gaining access to good housing. I doubt there are any shortcuts.

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REMARKS ON RUSHING'S PAPER ON HOSPITAL COMMITMENT

Publication of "Individual Resources, Societal Reaction, and Hospital Commitments" by William Rushing in the November 1971 issue of the *AJS* places the author and me in an embarrassing situation. The article is a replication and extension (although it is presented as original analysis) of my previously published article, "Who Shall Be Excluded: The Influence of Personal Attributes in Community Reaction to the Mentally Ill," which appeared in the July 1970 issue of *Social Psychiatry*.

I feel the need to point out what I consider to be the major points of similarity between Professor Rushing's article and my own:

a) Rushing's major hypothesis, which he refers to as his "resource-deprivation hypothesis," is stated as follows: "Individuals with limited social and economic resources have limited power and are more apt to be involuntarily confined than individuals whose resources and power are less limited." This hypothesis is further specified in the next paragraph: "Social and economic resources are assumed to be inversely related to an individual's socioeconomic status" and, "As for marital status, resources are assumed to be highest for the married and lowest for the single, with the disrupted-estranged (divorced, separated and widowed) in between" (Rushing, p. 513).

Compare with "Hypothesis One and Two," from my earlier article (Linsky 1970*b*): (1) "Communities Have a Greater Propensity to Exclude for Deviance Lower Class Persons and Members of Low Status Racial-Ethnic Groups," and (2) "Communities Have Greater Propensity to Exclude for Deviance Persons Who Lack Close Social Ties in the Community." The latter hypothesis is explained further in the paragraph following: "Isolated individuals and those without families would be expected to be institutionalized more readily than others because they lack social ties and sheltering primary type relationships to fall back on in difficult situations" (pp. 167, 169).

b) Community rejection of deviants, the dependent variable throughout the study (in both articles) is operationalized by Rushing by computing the ratio of involuntary to voluntary admissions (pp. 512, 519). This index forms the major basis of his analysis. Although it is identical with the index I used in "Who Shall Be Excluded" (p. 167), it is not acknowledged. I developed this measure named the "Exclusion Index" in a much earlier report (Linsky 1966), and have used it recently in another study of societal reaction to mental illness (Linsky 1970*a*, p. 307).

c) Rushing has added some measures of independent variables not included in my earlier paper. He uses occupational status, including farm and nonfarm categories, and combines them into five socioeconomic strata based upon occupational prestige as indicators of powerlessness and marginality (pp. 518, 519), whereas my earlier article used educational level and racial-ethnic status to get at the same thing (Linsky 1970*b*, pp. 168-69). Rushing uses the categories "married, disrupted-estranged, and single," (p. 513) compared with "married and non-married" and "family living situation vs. non-family living situations" in my paper (1970*b*, p. 169). He goes on to consider the influence of marital and SES status taken jointly as well as separately. These additional measures are quite useful but they do not change the basic framework of the study.

d) Rushing reports essentially the same findings as that reported in the

earlier article. This is reassuring but hardly surprising since he tests essentially the same hypotheses with the same design on *part of the same patient population used in the earlier study*. Rushing's data is based on male first admissions to the three Washington state mental hospitals, 1956 through 1965 (p. 518), whereas the patient population for my study was based on all first admissions to the same three Washington state hospitals for the period 1957-64 (1970*b*, p. 167).

Rushing does reference my paper (pp. 511-12), but the reference in no way communicates that his study is a repeat or at the outside, a refinement of my earlier study. It is presented instead as an original analysis.

I believe Rushing should have indicated that his article represents in part a repeat of my analysis based on the male portion of the patient population and in part an extension by the introduction of additional independent variables beside those included in my analysis.

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RUSHING REPLIES

In my recent paper (1971*b*) I failed to acknowledge that in an earlier paper Arnold Linsky (1970) investigated some of the same bivariate relationships that I investigated. In particular, Linsky investigated the relationships of marital status and education to the involuntary/voluntary ratio for patients in the state mental hospitals in Washington. I also investigated the relationship for marital status, and my measure of occupational status, based on Duncan's transformed NORC values, is probably correlated with education. Although I did note that Linsky had dealt with the same dependent variable (the involuntary/voluntary ratio), I failed to acknowledge the other similarities. This was due to an oversight for which I am embarrassed. (Professor Linsky need not be embarrassed by this, however; it is *my* error.)

At the same time, Linsky seriously overstates the case. There are important differences between our papers which he does not acknowledge. They should be reported just to set the record straight.

Theoretical objectives and interpretation.—In my paper I investigated the resource deprivation hypothesis which I derived from the hypothesis of cumulative social disadvantage (Lipset and Bendix 1959, p. 196); this hypothesis has been central in my work for several years (1970 and 1971*a*). I conceptualize class and marital status as *one* variable, individual resources, and argue that there are good theoretical and empirical grounds for believing that this variable is related to hospital commitment, as the hypothesis of cumulative social disadvantage implies. Over one-third of my paper (pp. 513–18) was devoted to these issues. In addition, my paper dealt with rates as well as ratios, and I argued that the influence of community isolation is different in the two cases. None of these issues is addressed in Linsky's paper. The intellectual sources of the two papers are clearly different; nothing Linsky said in his paper had any influence on anything I said in mine.

Analysis of the data.—As in a previous publication on hospitalization rates (1969), my concern in examining the bivariate relationships between resource deprivation and involuntary/voluntary ratios was to show the presence of *two* patterns in the relationship; the ratio continuously decreases as resources decrease, but is disproportionately high when resources are extremely low. An even greater concern was to investigate the resource deprivation hypothesis in terms of the interaction effects between the two individual resource variables (class and marital status) and between individual resources and community isolation visibility. None of these issues is dealt with in Linsky's paper. They are central in mine. Almost one-fourth of my paper deals with interaction effects alone (pp. 522–24).

The data.—Similarities of data in our papers are less than Linsky claims. Linsky's data for education are derived from official statistics and reported for only 36% of the cases, whereas I reported the occupation for 84% of the cases, based on the examination of patients' original hospital records; he includes males and females in the analysis; and he includes an unknown number of persons over 65 and under 21 (my findings are for persons 21–65 only). Consequently, it is not possible to know how much Linsky's results reflect errors in official statistics, the failure to distinguish between males and females (his *own* analysis reveals sexual differences in the ratio [Linsky 1970, table 3]), and the inclusion of older persons who probably contribute disproportionately to the hospitalization rate (persons over 65 probably have less formal education and higher rates of widowhood than persons 65 and less). Linsky does not recognize the importance of my

distinction between the single and the disrupted-estranged; if the hospitalization rate were higher for the latter, the resource deprivation hypothesis as I formulated it would be called into question.

So despite certain surface similarities in our two papers, there are major differences which most readers will recognize. This does not justify my failure to make appropriate reference to the similarities, of course. But to consider my paper a mere replication of Linsky's is an exaggeration, to say the least.

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ERRATUM TO THE WERTS-LINN COMMENTS ON BOYLE'S "PATH ANALYSIS AND ORDINAL DATA"¹

Werts and Linn (1971) pointed out that Boyle (1970) had implicitly assumed that the causative variables were measured without error. Further study of literature relating to this problem (e.g., Cochran 1968; Evans 1970; Anderson 1959) indicates that the Werts-Linn procedure for dealing with categorical errors of measurement is incorrect. The purpose of this note is to set the record straight.

As a basis for generalization to polychotomous variables, first consider the case of three independent fallible dichotomous measures X_j ($j = 1, 2, 3$) of an underlying true dichotomy (T). The observed categories will be labeled $k = 1, 2$ and the true categories $l = 1, 2$. The relationship between X_j and T can be expressed as a function of the conditional probabilities $P(X_j = k|T = l) = \theta_{jkl}$ for each combination of k and l :

$$P(X_j = 1|T = 1) = \theta_{j11}, \quad P(X_j = 1|T = 2) = \theta_{j12},$$

$$P(X_j = 2|T = 1) = \theta_{j21}, \quad \text{and} \quad P(X_j = 2|T = 2) = \theta_{j22}; \quad \text{where}$$

θ_{j21} is commonly labeled the proportion of false negatives and θ_{j12} the proportion of false positives. The sum of the conditional probabilities for a fixed value of l is unity (i.e., $\theta_{j11} + \theta_{j21} = 1$ and $\theta_{j12} + \theta_{j22} = 1$). Define $P_{T_l} = P(T = l)$ and $P_{jk} = P(X_j = k)$ where

$$\sum_l P_{T_l} = 1$$

and

$$\sum_k P_{jk} = 1.$$

The model parameters to be estimated are the conditional probabilities for each observed measure and the true proportions in each category. Since each object is categorized by each different measure, the proportion of objects for each combination of observed categories can be computed. Define $P_{jk,j'k',j''k''} = P(X_j = k, X_{j'} = k', X_{j''} = k'')$ where $j \neq j' \neq j''$. In the three-measure case the observed data consist of eight joint probabilities $P_{11,21,31}, P_{11,21,32}, P_{11,22,31}, P_{11,22,32}, P_{12,21,31}, P_{12,21,32}, P_{12,22,31},$ and $P_{12,22,32}$. The next step is to relate these observed probabilities to the model parameters. Starting with $P_{11,21,31}$ we obtain:

$E(P_{11,21,31}) = P(X_1 = 1, X_2 = 1, X_3 = 1, T = 1) + P(X_1 = 1, X_2 = 1, X_3 = 1, T = 2)$. Expressed in terms of conditional probabilities, the proportions are $P(X_1 = 1, X_2 = 1, X_3 = 1, T = 1) = P(X_1 = 1|X_2 =$

¹ The research reported herein was performed pursuant to grant OEG-2-700033(509) with the U.S. Department of Health, Education, and Welfare and the Office of Education.

1, $X_3 = 1$, $T = 1$) $P(X_2 = 1|X_3 = 1, T = 1)P(X_3 = 1|T = 1)P(T = 1)$, and $P(X_1 = 1, X_2 = 1, X_3 = 1, T = 2) = P(X_1 = 1|X_2 = 1, X_3 = 1, T = 2)P(X_2 = 1|X_3 = 1, T = 2)P(X_3 = 1|T = 2)P(T = 2)$.

The assumption that the measures are independent implies that

$$P(X_1 = 1|X_2 = 1, X_3 = 1, T = 1) = P(X_1 = 1|T = 1) = \theta_{111},$$

$$P(X_2 = 1|X_3 = 1, T = 1) = P(X_2 = 1|T = 1) = \theta_{211},$$

$$P(X_1 = 1|X_2 = 1, X_3 = 1, T = 2) = P(X_1 = 1|T = 2) = \theta_{112},$$

and

$$P(X_2 = 1|X_3 = 1, T = 2) = P(X_2 = 1|T = 2) = \theta_{212}.$$

Thus, by substitution:

$$E(P_{11,21,31}) = \theta_{111} \theta_{211} \theta_{311} P_{T_1} + \theta_{112} \theta_{212} \theta_{312} P_{T_2}. \quad (1)$$

While this process could be repeated for each of the observed joint probabilities, for identification purposes it is better to replace these by the following set:

$$P_{11,21} = P_{11,21,31} + P_{11,21,32},$$

$$P_{11,31} = P_{11,21,31} + P_{11,22,31},$$

$$P_{21,31} = P_{11,21,31} + P_{12,21,31},$$

$$P_{11} = P_{11,21,31} + P_{11,21,32} + P_{11,22,31} + P_{11,22,32},$$

$$P_{21} = P_{11,21,31} + P_{11,21,32} + P_{12,21,31} + P_{12,21,32},$$

and

$$P_{31} = P_{11,21,31} + P_{11,22,31} + P_{12,21,31} + P_{12,22,31}.$$

Following the procedure used for $P_{11,21,31}$ it may be shown that:

$$E(P_{11,21}) = \theta_{111} \theta_{211} P_{T_1} + \theta_{112} \theta_{212} P_{T_2}, \quad (2)$$

$$E(P_{11,31}) = \theta_{111} \theta_{311} P_{T_1} + \theta_{112} \theta_{312} P_{T_2}, \quad (3)$$

$$E(P_{21,31}) = \theta_{211} \theta_{311} P_{T_1} + \theta_{212} \theta_{312} P_{T_2}, \quad (4)$$

$$E(P_{11}) = \theta_{111} P_{T_1} + \theta_{112} P_{T_2}, \quad (5)$$

$$E(P_{21}) = \theta_{211} P_{T_1} + \theta_{212} P_{T_2}, \quad (6)$$

and

$$E(P_{31}) = \theta_{311} P_{T_1} + \theta_{312} P_{T_2}. \quad (7)$$

Note that even though we started with eight joint probabilities, we have only seven equations because of the condition that all the observed prob-

abilities sum to unity. If the model parameters are identifiable, then it should be possible to solve these equations for each parameter in terms of the expected probabilities. For this purpose it is convenient to define:

$C_{jk,j'k'} = E(P_{jk,j'k'}) - E(P_{jk})E(P_{j'k'})$, $C_{jk,j'k',j''k''} = E(P_{jk,j'k',j''k''}) - [E(P_{jk})] C_{j'k',j''k''} - [E(P_{j'k'})] C_{jk,j''k''} - [E(P_{j''k''})] C_{jk,j'k'} - E(P_{jk})E(P_{j'k'})E(P_{j''k''})$, and $Q_{T_1} = 1 - P_{T_1}$. For the dichotomous case $Q_{T_1} = P_{T_2}$. Solving equations (1) through (7) for P_{T_1} we obtain:

$$\frac{Q_{T_1}^2 - P_{T_1}^2}{\sqrt{P_{T_1} Q_{T_1}}} = \frac{C_{11,21,31}}{\sqrt{C_{11,21} C_{11,31} C_{21,31}}} \quad (8)$$

Equation (8) shows that P_{T_1} and $P_{T_2} = 1 - P_{T_1}$ are identified. Further analysis yields:

$$\theta_{112} = E(P_{11}) - \sqrt{\frac{C_{11,21} C_{11,31}}{C_{21,31}}} \left(\frac{P_{T_1}}{Q_{T_1}} \right), \quad (9)$$

$$\theta_{212} = E(P_{11}) - \sqrt{\frac{C_{11,21} C_{21,31}}{C_{11,31}}} \left(\frac{P_{T_1}}{Q_{T_1}} \right), \quad (10)$$

and

$$\theta_{312} = E(P_{11}) - \sqrt{\frac{C_{11,31} C_{21,31}}{C_{11,21}}} \left(\frac{P_{T_1}}{Q_{T_1}} \right). \quad (11)$$

Since P_{T_1} and P_{T_2} are identified, equations (9), (10), and (11) show that θ_{112} , θ_{212} , θ_{312} , and therefore $\theta_{122} = 1 - \theta_{112}$, $\theta_{222} = 1 - \theta_{212}$, and $\theta_{322} = 1 - \theta_{312}$ are identifiable. Given P_{T_1} , P_{T_2} , θ_{112} , θ_{212} , and θ_{312} identified, equations (5), (6), and (7) show that θ_{111} , θ_{211} , and θ_{311} and therefore $\theta_{121} = 1 - \theta_{111}$, $\theta_{221} = 1 - \theta_{211}$, and $\theta_{321} = 1 - \theta_{311}$ are identifiable. Since the model consists of seven equations in seven unknowns (i.e., just identified), parameter estimates can be obtained which will exactly reproduce the observed probabilities, that is, the observed joint probabilities would equal the expected joint probabilities estimated from the parameter estimates. The above analysis shows that the true proportions may be identified given three independent dichotomous measures, a point which Werts and Linn (1971) failed to discover. The right-side numerator of equation (8) is the expected value of the triple covariance between X_1 , X_2 , and X_3 , which is the crucial piece of information neglected in the Werts-Linn path approach. Furthermore, path analysis usually ignores variable means, which would result in neglect of equations (9), (10), and (11) which involve means (P_{jk}).

Next consider the trichotomous case in which $k = 1,2,3$, $l = 1,2,3$ and $j = 1,2,3$ given the assumption of independent measures. The relationship between the j th observed trichotomy and the true trichotomy involves nine

conditional probabilities: θ^*_{j11} , θ^*_{j12} , θ^*_{j21} and θ^*_{j22} as defined previously plus $P(X_j = 1|T = 3) = \theta^*_{j13}$, $P(X_j = 2|T = 3) = \theta^*_{j23}$, $P(X_j = 3|T = 1) = \theta^*_{j31}$, $P(X_j = 3|T = 2) = \theta^*_{j32}$, and $P(X_j = 3|T = 3) = \theta^*_{j33}$. By definition: $\theta^*_{j11} + \theta^*_{j21} + \theta^*_{j31} = \theta^*_{j12} + \theta^*_{j22} + \theta^*_{j32} = \theta^*_{j13} + \theta^*_{j23} + \theta^*_{j33} = 1$. Let K = total number of categories and J = total number of independent measures. The observed data consists of the $K^J = 27$ joint triple probabilities $P_{11, 21, 31}$, one of which may be expressed as a function of the other 26. There are $JK^2 = 27 \theta^*_{jkl}$, JK of which can be stated as a function of the others because for a fixed l the θ^*_{jkl} sum to unity and $K = 3 P_{T_l}$, one of which it can be stated as 1 minus the sum of the others. Therefore there are a total of $JK(K - 1) + (K - 1) = 20$ independent parameters to be estimated from the $K^J - 1 = 26$ independent observed joint probabilities (i.e., the model has six overidentifying restrictions). This does not necessarily mean that all parameters are identified, and in principle the expected value of each $P^*_{jk,j'k',j''k''}$ should be derived as done previously and the equations solved for each parameter. Rather than attempt this directly, it can be seen that if category 3 were collapsed into category 2, then the analysis would be identical with that shown for dichotomous variables. The relationships would be (* refers to probabilities prior to collapsing categories):

$$P_{T_1} = P^*_{T_1}, \quad (12a)$$

$$\theta_{j11} = \theta^*_{j11}, \quad (12b)$$

and

$$\theta_{j12}(1 - P_{T_1}) = \theta^*_{j12} P^*_{T_2} + \theta^*_{j13} P^*_{T_3}. \quad (12c)$$

From our previous analysis we know that the parameters in the right side of equations (12a)-(12c) can be identified from

$$P_{11,21,31} = P^*_{11,21,31},$$

$$P_{11,21,32} = P^*_{11,21,32} + P^*_{11,21,33},$$

$$P_{11,22,31} = P^*_{11,22,31} + P^*_{11,23,31},$$

$$P_{11,22,32} = P^*_{11,22,32} + P^*_{11,23,32} + P^*_{11,22,33} + P^*_{11,23,33},$$

$$P_{12,21,31} = P^*_{12,21,31} + P^*_{13,21,31},$$

$$P_{12,21,32} = P^*_{12,21,32} + P^*_{12,21,33} + P^*_{13,21,32} + P^*_{13,21,33},$$

$$P_{12,22,31} = P^*_{12,22,31} + P^*_{12,23,31} + P^*_{13,22,31} + P^*_{13,23,31},$$

and

$$P_{12,22,32} = P^*_{12,22,32} + P^*_{12,22,33} + P^*_{12,23,32} + P^*_{12,23,33} + P^*_{13,22,32} + P^*_{13,22,33} + P^*_{13,23,32} + P^*_{13,23,33}.$$

These eight $P_{jk, j'k', j''k''}$ could be entered into the analysis shown for dichotomies and the corresponding parameters in (12a), $N(12c)$ identified. In a similar fashion if we collapse category 1 into 3 then:

$$P_{T_2} P^*_{T_2}, \quad (12d)$$

$$\theta_{j22} = \theta^*_{j22}, \quad (12e)$$

and

$$\theta_{j23} (1 - P_{T_2}) = \theta^*_{j21} P^*_{T_1} + \theta^*_{j23} P^*_{T_3}. \quad (12f)$$

The right-hand parameters in (12d) $N(12f)$ would be identified from:

$$P_{12,22,32} = P^*_{12,22,32},$$

$$P_{12,22,33} = P^*_{12,22,31} + P^*_{12,22,33},$$

$$P_{12,23,32} = P^*_{12,23,32} + P_{12,21,32},$$

$$P_{12,23,33} = P^*_{12,23,33} + P^*_{12,21,33} + P^*_{12,23,31} + P^*_{12,21,31},$$

$$P_{13,22,32} = P^*_{13,22,32} + P^*_{11,22,32},$$

$$P_{13,22,33} = P^*_{13,22,33} + P^*_{13,22,31} + P^*_{11,22,33} + P^*_{11,22,31},$$

$$P_{13,23,32} = P^*_{13,23,32} + P^*_{13,21,32} + P^*_{11,23,32} + P^*_{11,21,32},$$

and

$$\begin{aligned} P_{13,23,33} = & P^*_{13,23,33} + P^*_{13,23,31} + P^*_{13,21,33} + P^*_{13,21,31} + P^*_{11,23,33} \\ & + P^*_{11,23,31} + P^*_{11,21,33} + P^*_{11,21,31}. \end{aligned}$$

These eight $P_{jk,j'k',j''k''}$ could likewise be entered into the analysis for dichotomies where the two categories are $k = 2,3$ instead of $k = 1,2$ as shown in our original analysis. We can conclude that $P^*_{T_1}$, $P^*_{T_2}$, and $P^*_{T_3}$ are identified from (12a) and (12d) and θ^*_{111} , θ^*_{211} , θ^*_{311} , θ^*_{122} , θ^*_{222} , and θ^*_{322} from equations (12b) and (12e). The remaining 12 parameters in equations (12c) and (12f) have six conditions imposed by equations (12c) and (12f), so we need six more equations for identification. The simplest set, which is independent of information used in the dichotomous analyses is:

$$P_{11,22} = P^*_{11,22,31} + P^*_{11,22,32} + P^*_{11,22,33},$$

$$P^*_{11,32} = P^*_{11,21,32} + P^*_{11,22,32} + P^*_{11,23,32},$$

$$P^*_{12,21} = P^*_{12,21,31} + P^*_{12,21,32} + P^*_{12,21,33},$$

$$P^*_{12,31} = P^*_{12,21,31} + P^*_{12,22,31} + P^*_{12,23,31},$$

$$P^*_{21,32} = P^*_{11,21,32} + P^*_{12,21,32} + P^*_{13,21,32},$$

and

$$P^*_{22,31} = P^*_{11,22,31} + P^*_{12,22,31} + P^*_{13,22,31}.$$

Application of the procedure used to derive equation (1) yields:

$$\left. \begin{aligned} E(P^*_{11,22}) &= \theta^*_{111}\theta^*_{221}P^*_{T_1} + \theta^*_{112}\theta^*_{222}P^*_{T_2} + \theta^*_{113}\theta^*_{223}P^*_{T_3}, \\ E(P^*_{11,32}) &= \theta^*_{111}\theta^*_{321}P^*_{T_1} + \theta^*_{112}\theta^*_{322}P^*_{T_2} + \theta^*_{113}\theta^*_{323}P^*_{T_3}, \\ E(P^*_{12,21}) &= \theta^*_{121}\theta^*_{211}P^*_{T_1} + \theta^*_{122}\theta^*_{212}P^*_{T_2} + \theta^*_{123}\theta^*_{213}P^*_{T_3}, \\ E(P^*_{12,31}) &= \theta^*_{121}\theta^*_{311}P^*_{T_1} + \theta^*_{122}\theta^*_{312}P^*_{T_2} + \theta^*_{123}\theta^*_{313}P^*_{T_3}, \\ E(P^*_{21,32}) &= \theta^*_{211}\theta^*_{321}P^*_{T_1} + \theta^*_{212}\theta^*_{322}P^*_{T_2} + \theta^*_{213}\theta^*_{323}P^*_{T_3}, \end{aligned} \right\} \quad (13)$$

and

$$E(P^*_{22,31}) = \theta^*_{221}\theta^*_{311}P^*_{T_1} + \theta^*_{222}\theta^*_{312}P^*_{T_2} + \theta^*_{223}\theta^*_{313}P^*_{T_3}.$$

Equations (13) in combination with previously identified parameters and equations (11), (12c) and (12f) identify the remaining parameters. Note that six equations have not been used, these representing the six degrees of overidentification. The method which appears appropriate for estimating parameters when the observed variables are independent polychotomous measures is discussed in Anderson (1959, sec. 3.6) and Cochran (1968, sec. 6). In this procedure a χ^2 function involving the observed and estimated expected joint probabilities is minimized as a function of the model parameters. The resulting χ^2 with degrees of freedom equal to the number of overidentifying restrictions is a measure of the fit of the model to the data. Our analysis indicates that given three independent polychotomies ($K = 3$), all model parameters are identifiable. The number of overidentifying restrictions is equal to $(K^J - 1) - (JK + 1)(K - 1)$ where J = the number of independent measures and K = the number of categories.

We may now consider exactly why the Werts-Linn analysis was inappropriate to the problem. For this purpose it is helpful to put the conditional probabilities into matrix form where columns refer to observed categories and rows to true categories:

$$\theta^*_j = \begin{bmatrix} \theta^*_{j11} & \theta^*_{j21} & \theta^*_{j31} \\ \theta^*_{j12} & \theta^*_{j22} & \theta^*_{j32} \\ \theta^*_{j13} & \theta^*_{j23} & \theta^*_{j33} \end{bmatrix}. \quad (14)$$

As noted earlier, each row in θ^*_j sums to unity. If the true categories 1, 2, and 3 actually form an ordered set of classifications such that category 1 is closer to 2 than to 3, then we would expect that classificatory errors would be more likely for neighboring categories (i.e., $\theta^*_{j12} > \theta^*_{j13}$ and $\theta^*_{j32} > \theta^*_{j31}$). In contrast, if the true categories are basically unordered, it would be more reasonable to expect the likelihood of misclassification to be similar for any of the other classes (i.e., $\theta^*_{j12} \cong \theta^*_{j13}$, $\theta^*_{j21} \cong \theta^*_{j23}$,

and $\theta^*_{j31} \cong \theta^*_{j33}$). In other words the probability of misclassification is a function of the underlying scale or true category in the case of ordered categories and is not in the case of unordered categories. Werts and Linn implicitly assumed that the errors for one category were uncorrelated with the underlying true dummy variable for the same and for other categories, which translated into the present framework corresponds to the analysis for an unordered scale, that is, for an ordered scale the errors would be correlated with the true dummy variables for other categories. It can be algebraically shown that the Werts-Linn procedure leads to incorrect formulae for the expected value of the observed joint probabilities when the categories are ordered. Since Boyle (1970) was examining the problem of ordered categories (i.e., scales) the Werts-Linn approach is not relevant to his problem.

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Erratum

In the May 1972 issue of the *Journal*, Ira I. Reiss's book, *The Family System in America*, was listed incorrectly as \$19.95. The price is actually \$9.95.

Review Symposium

Beyond Freedom and Dignity. By B. F. Skinner. New York: Alfred A. Knopf, Inc., 1971. Pp. 225. \$6.95.

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One sometime wonders what all the fuss over B. F. Skinner is about. Take his behavioral psychology itself. Many persons do not realize that they have been talking prose, that is, behavioral psychology, for a very long time, indeed for endless ages. It did not require Skinner to teach people that if they wanted someone to do something, it helped to offer him an inducement, or that a man who had performed an action which was successful in particular circumstances was apt to repeat that action when similar circumstances recurred. This sort of common sense embodies perfectly sound behavioral principles. What modern behavioral psychology has done is state the principles explicitly, free them from unnecessary assumptions, test them rigorously in experiments largely carried out on animals, and begin to establish the crucial quantitative relationships. All this has been a very great achievement, and Skinner has been the leader in it, but it is an achievement in systematizing what men and women have long known intuitively and unsystematically. They could not have helped knowing it. Their own behavior and that of others were what they were most familiar with, and success in social life required that they use behavioral principles in some haphazard fashion, even if they did not formulate them.

I myself am fully persuaded of the truth of the principles of behavioral psychology as so far formulated, though I think there may be principles we have yet to formulate clearly. To this extent, I am a Skinnerian and have long been known as one. But the issue raised by his present book is less the intellectual truth of behavioral psychology than its application in a systematic technology for controlling human behavior. Skinner's central argument is that the views put forward by the ideologists of human freedom and dignity may prevent men from adopting a technology which might be more powerful in changing human behavior and changing it for the better than anything we use now. On this issue, I believe both Skinner and his opponents are much more touchy than the reality warrants, and that the most serious impediment to the adoption of a behavioral technology is one that Skinner fails to recognize. Neither Skinner nor his opponents will make as much practical difference as either party seems to think.

Let me turn first to the famous problem of "the freedom of the will." Its practical import is limited, whatever its philosophical interest may be. As a behaviorist, I must believe that my actions are wholly determined, that the choice I make between alternatives is predestined—but what earthly

difference does it make? My intellectual conviction does not, as I wish it would, relieve me of the necessity of making a decision; and in this thoroughly operational sense I remain a free, autonomous man. Not until a machine is invented that will always and correctly predict my action in advance, even when I know that it is designed to predict my action, shall I lose my *de facto* freedom and autonomy. I shall then also become superfluous. But even if at some time in the future machines of this sort can be produced, their production is always going to be much more costly than the production of ordinary men like myself by what I shall call conventional methods, and therefore men are never going to loose their loophole to freedom and autonomy. No one need fear that behavioral psychology is going to rob him of his free will, so what is all the fuss about?

Freedom is really a moral demand, not an intellectual issue. Though the philosophic freedom of man's will may be preserved by cost considerations—to prove that the behavior of every man is utterly determined will always cost more than the proof will be worth—nevertheless, men will always in fact attempt to determine, that is, to control, the behavior of others, and their attempts will often be successful. The demand that men have made for themselves and for others that they should be free has usually been a demand not that they should be spared attempts to control their behavior, but only that they should be spared certain kinds of attempts—adversive control, as Skinner calls it—control by threats, deprivation, and force. In this demand Skinner himself concurs. Few men have argued, on the other hand, that men should be free from control in the sense of being spared the temptation of receiving positive rewards for their actions, unless the actions themselves were believed to have adverse consequences in the long run, like Adam's eating the apple.

True, some silly things have been said, and some persons have argued that men should be freed from all control, but the arguments soon get into hopeless difficulties. First, for one man to refrain from attempting to control the behavior of another does not mean that the other's behavior is left uncontrolled: it only means that it comes under the control of different variables. Second, and more interesting, it is always possible, indeed it is easy to show, that the very persons who declaim against efforts to control others—"manipulate" is the word often used—do in fact attempt to control others. Accordingly, they are hypocrites, though no doubt unconscious ones, if that is not a contradiction in terms. Remember what Mr. Justice Holmes had to say about the philosophical objections to "treating man as a thing, and the like. . . . If a man lives in society, he is liable to find himself so treated." He might have added "and so to treat others" (O. W. Holmes, *The Common Law*, ed. M. DeW. Howe [Cambridge, Mass.: Harvard University Press, 1963], p. 38).

Take the case of Arthur Koestler, who ought to know better. Skinner quotes him (p. 165) as referring to behaviorism as "a monstrous triviality." It represents, he says, "question-begging on an heroic scale." It has spun psychology into "a modern version of the Dark Ages." Triviality though it is, one could quite easily show that it has been exceedingly important to

Arthur Koestler, for he uses it every day of his life, for instance when he asks anyone else to do anything for him—for pay or not for pay. "Manipulation" always seems to refer to someone else's attempt to control the behavior of others, not one's own.

Skinner argues that the literature of freedom tends, in fact and contrary to its intent, toward perpetuating adverse control. Thus, if a man is free in the sense that his behavior is not determined by external circumstances, then he may personally be held responsible for his behavior, and if he is responsible, then he may fairly be punished if he does ill.

For myself, I would not hold the literature of freedom so much to blame for perpetuating either aversive or other relatively ineffective methods of control. (Incidentally, Skinner never makes clear what his alternative "strong" or "effective" methods would be.) The fact the very men who most decry behavioral psychology as a threat to freedom will use its principles to control others when they are able to offer these others positive rewards, suggests that something else is at stake. It is easy, for instance, to say that the juvenile delinquent should not be punished but rather that the environment that produces him should be changed. That, indeed, is a reasonable conclusion from behavioral psychology. But we may not be able to change the environment, or the cost of doing so may be more than we are willing to pay. We may, on the other hand, be able to punish the delinquent. Our action at least satisfies our emotional need for vicarious revenge, and it may have some little effect for the good on his future behavior and that of others. So we punish him, and we cite his freedom in order to rationalize our action. That is, it may not be the literature of freedom that perpetuates aversive and other weak forms of control but, rather, our aversive and other weak forms of control that perpetuate the literature of freedom. And, as I shall try to show later, the fact that control is carried out according to the principles of behavioral psychology need not exempt it from a crucial kind of weakness.

I feel even more oversold in what Skinner says about dignity than in what he says about freedom. I think both he and those who oppose him exaggerate the dangers each presents to the other. What Skinner means by dignity is the credit men give to others (and take for themselves) for the good deeds they have performed. Skinner argues cogently that men are apt to give others credit for actions that have the following two properties: they reward the credit givers, and the conditions that determine them are not easily recognizable. (Thus, we would not give a man credit for bravery in jumping from a high place if we could see that he had been pushed.) I myself would state in a somewhat different fashion the characteristic that makes an action praiseworthy: the action must be rare, in short supply relative to the demand for it. But I suspect that this amounts to saying the same thing, for the actions whose determinants are readily recognizable are just the ones that are easily produced, that many people can and do perform.

Accordingly, the more clearly one can demonstrate the conditions and laws that determine the actions of men, the less excuse one has for praising

them for their actions. Yet Skinner says (p. 58), "What we may call the literature of dignity is concerned with preserving due credit. It may oppose advances in technology, including a technology of behavior, because they destroy chances to be admired and a basic analysis because it offers an alternative explanation of behavior for which the individual himself has previously been given credit."

It is an interesting point, like all of Skinner's, yet I remain skeptical. First, Skinner offers very little evidence that "the literature of dignity" has in fact opposed advances in behavioral technology, though logically it ought to have done so. Second, it is not as easy as he thinks to destroy reasons for admiring others or being admired oneself. Presumably, for instance, people have always known that the beauty of a beautiful woman is not her fault. Accordingly, she should get no credit for being beautiful. But we all know that she does damn well get that credit, and usually is ready to accept it too. Or take an example closer to home. As I have said, I believe firmly that every single bit of my behavior has been predestined from ages eternal, and therefore I give intellectual assent to the proposition that, not being an autonomous individual, I should be given no credit for anything I do. Yet I observe that in fact I much enjoy being praised for articles and books that I write, and I believe that the praise (when I get it) makes it more likely that I shall write further articles and books. I believe that the same is true of Skinner himself. That is to say, being given credit is, paradoxically, part of the very determinism which discredits giving credit. In the words of Emerson's poem, "When me ye fly, I am the wings."

On this issue, it is curious that a behavioral psychologist like Skinner should rely so heavily on a purely intellectual and logical argument and so little on observation of actual human behavior.

Let Skinner be very careful in arguing that the literature of human dignity stands in the way of further human achievement. His own type of literature might be more likely to have that effect. If it really persuaded people—which it will not—that they ought not to praise others, since these others are not autonomous individuals and so deserve no praise, he would rob mankind of one of the most powerful of positive reinforcers. But to get achievement, we must have reinforcers, and they are not so plentiful that we can afford to forgo any of them. To quote Mr. Justice Holmes once more, this time his elegant statement of the paradox of behavioral determinisms, "The way in which the inevitable comes to pass is through effort" (*The Holmes-Einstein Letters*, ed. J. B. Peabody [New York: St. Martin's Press, 1964], p. 5). But if effort is to be sustained, it requires, by Skinner's own argument, reinforcement.

For the reasons given, I doubt that the freedom and dignity will much impede the systematic application of behavioral principles. What is much more likely to impede it is something Skinner gives little thought to. Again and again he argues that behavior is under the control of contingencies set by its environment. He is quite right to do so, for that insight is the heart of behaviorism. Accordingly, if the environment can be appropriately

arranged, the behavior will be forthcoming. As he says (p. 149), "The outlines of a technology are already clear. An assignment is stated as behavior to be produced or modified, and relevant contingencies are then arranged."

Are then arranged! I like that use of the passive! *Who* does the arranging, and is he *able* to do it? Skinner knows better, but he sometimes talks as if "the environment" were always readily manipulable. Usually by far the most important part of the environment of behavior is the behavior of other men, so that when we try to manipulate the environment to change the behavior, we find that what must be manipulated is still more behavior, and we are right back where we started. That is, we must manipulate the environment of a social relationship between two or more persons, and so on in an infinite regress. The task may easily become expensive.

Skinner hardly gives a thought to the crucial question of cost. He does not appear to realize that one may have developed a technology that is capable of securing particular results but still not apply it, because the cost of applying it is too high—cost in the sense of alternative and rewarding results that must be forgone if it is applied. For instance, he speaks (p. 6) of "making it possible for everyone to be gainfully employed and, as a result, to enjoy a higher standard of living." He implies that this result is not now feasible. I suggest that it is wholly feasible to employ everyone, but that its costs, in inflation for one thing, may be higher than the government and citizens are willing to bear. Again, it may well be feasible to plan an economic system in detail, but the costs of planning may be greater than the results are worth. In the same way, the cost of a systematic behavioral technology may provide the chief obstacle to its application.

As Skinner himself says (p. 150), "The technology has been most successful where behavior can be fairly easily specified and where appropriate contingencies can be constructed—for example, in child care, schools, and the management of retardates and institutionalized psychotics." But note that these are all situations in which the persons who are to apply the technology already have so much control over others that arranging appropriate contingencies is relatively inexpensive. The persons to be controlled are so little in a position to exercise countercontrol that they need not, so to speak, be bought off. Outside of such situations, the costs of applying a sophisticated behavioral psychology *in detail* are apt to become prohibitively expensive, especially if the number of highly trained behavioral psychologists, needed to apply the technology but otherwise unproductive, must be high in proportion to the rest of the population. Then it might be that our old, hit-or-miss, but relatively cheap methods of controlling one another might, on analysis, prove to get us more for our money than the best of behavioral technologies. At any rate, it is a question that Skinner nowhere raises.

Much more likely will be the gradual extension of the kind of control that makes use of existing institutions and envisages gross and only statistically significant results, achieved through altering in some degree the payoffs of a large number of persons. The kind of control that produced the recent devaluation of the dollar is an example. Such controls are just

as much based on the principles of behavioral psychology as any that Skinner proposes, though the persons who employ them may not be aware of the fact. They come quite cheap for the results obtained. And no one seems to raise against them the moral issue that all control is wrong.

Skinner regularly helps to create the fuss that surrounds him by seeming to claim too much for behavioral psychology. He writes (p. 19), "A technology of operant behavior is, as we shall see, already well advanced, and it may prove to be commensurate with our problems." The technology may indeed be on hand, but that does not mean it can easily be applied. At times, Skinner talks too glibly about "the design of a culture," yet he always keeps returning to sobriety (p. 156): "Perhaps we cannot now design a successful culture as a whole, but we can design better practices in a piecemeal fashion." What nervous conservative ever claimed less? It is even pathetic to find him writing (p. 158), "We have the physical, biological, and behavioral technologies needed 'to save ourselves'; the problem is how to get people to use them." What reformer for the last 5,000 years has ever uttered any other complaint? What is so powerful about a behavioral technology that is not powerful enough to get people to use it? If Skinner is right, they would use it if appropriate contingencies in the environment could be arranged. But where is he to place the fulcrum from which the world is to be moved?

Finally, Skinner must ask the inevitable question (p. 103): "If a scientific analysis can tell us how to change behavior, can it tell us what changes to make?" Many people would agree on the changes that ought to be made, if possible, in the behavior of a retarded child—and that is one reason why a behavioral technology can be applied in this case at relatively low cost. But it is not a scientific analysis that brings about the agreement, and there are many cases in which no agreement of any sort is in sight.

In effect, Skinner proposes that "the survival of a culture" (p. 129) be the criterion according to which changes in behavior should be evaluated: those changes should be made which help a culture to survive. Such a criterion appears to make good sense, if only for the reason that men hold many of their values just because these values helped mankind as a species to survive. These values had survival value.

Unhappily for the criterion, it is not as unambiguous as it looks. Skinner to the contrary (p. 129), a culture does not really correspond to a species. Many species have failed to survive, but very few cultures. They are not killed off but are absorbed by, and melted into, other cultures, changing and developing over time. The Roman Empire failed to survive, but much of its culture still forms part of our culture today. The survival of a culture is a rubber criterion.

Suppose, for the sake of argument, that we know what "the survival of a culture" means. Skinner then specifies the behavior that contributes most to the survival of a culture, and therefore the behavior most to be promoted by a behavioral technology (p. 136): "The simple fact is that a culture which *for any reason* induces its members to work for its survival, or the survival of some of its practices, is more likely to survive."

This may be a necessary condition; it is not a sufficient one. For it is not just a question of whether the members work for the survival of their culture, but what they work *at*. Suppose they decided that the best way to ensure the survival of their culture was by destroying all other cultures and worked hard to that end. They might indeed ensure the survival of their culture, but then again they might not, and I doubt that any scientific analysis now existing could demonstrate they were either correct or incorrect in advance of the event. The fact is that we do not know just what practices will ensure the survival of a culture—except a return of all cultures to the level of the hunting-gathering band. We know that cultures can survive at that level, for they did so for hundreds of thousands of years. If we do not return to the hunting band, it will not be because a scientific analysis told us not to.

Skinner even says (p. 135), "Practices which induce the individual to work for the good of others presumably further the survival of others and hence the survival of the culture the others carry." But do they further the survival of the culture that the individual in question carries? I wish I could believe, as a universal generalization, that they did, but I find that I cannot and that I must discriminate. I can think of cases in which the statement holds good, but unhappily I can think of others in which it does not. One of them is the case of shipwrecked mariners adrift in an open boat, short of food. If each works for the good of the others, all will soon starve. If some work against the good of others and kill them, a few may survive and their culture with them. With the present increase in world population, the cultures of the world increasingly resemble the shipwrecked mariners. If some refrain from killing the others, it will not be because a scientific analysis has forbidden it. Consider what else might do so!

In short. Skinner's procedure for deciding what changes in behavior ought to be made turns out to be facile. It fails to take account of the actual difficulties. Yet he has thought to some purpose about the problems of applying a behavioral psychology when most of the opponents of its application have been quite thoughtless. He has asked the right questions and has provided an immense stimulus toward finding better answers than those he proposes himself.

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As Skinner himself has said, "No one goes to the circus to see the average dog jump through a hoop significantly oftener than untrained dogs" (*American Psychologist* 11 [May 1956]: 228). If you want a dog to do something, you train him until he does it on call. That's what the paying customers demand.

In this new book, he asks why we should settle for much less in our

demands on the methods of social management. We face terrifying problems in the world today. We must, he says, contain a population explosion, stave off world famine, prevent a nuclear holocaust, provide housing and transportation, eliminate ghettos, cure disease, and stop pollution. "In short, we need to make vast changes in human behavior" (p. 4). What we need is a technology of behavior. "We could solve our problems quickly enough if we could adjust the growth of the world's population as precisely as we adjust the course of a spaceship, or improve agriculture and industry with some of the confidence with which we accelerate high-energy particles" (p. 5).

The reader who believes with Skinner that we are moving "inexorably" toward "catastrophe" (p. 5) will not gain heart from the pages that follow. We are far—very far—from the technology required. It takes long, hard work to train a single dog. How much more difficult will it be to train mankind—especially since we are not certain of which behavior we really want to produce! Nonetheless, in that way lies our hope. Perhaps with a large enough program of federal grants-in-aid. . . .

But, even if it could be accomplished, would not such training be intolerable—an ultimate totalitarianism? Not at all, says Skinner. We are fully protected by our genetic endowment. This protection operates in two ways. First, we are so constructed that we act to free ourselves from harmful contacts and consequences—from what he calls negative or aversive stimuli or reinforcers. Second, the new behavior that our crises require can be attained only through the use of positive reinforcement. To overcome our crises, men must come to do the right things and not merely to avoid doing the wrong things. It so happens that animals, men included, can be trained to do the right things only if they are given positive reinforcement, only, that is, if they are so trained that the desired behavior leads to positive consequences. When a bit of behavior is followed by a positive consequence, that behavior is more likely to occur again.

To summarize, we are innately so equipped that we try to free ourselves from what is harmful and we can learn to behave only in ways that we will, by innately given standards, "want" to repeat. We cannot be other than true to our genetic endowment. It guarantees that we cannot be trained to behave in ways that are harmful (pp. 103–26, 128–29).

We need, so Skinner urges, to distinguish between (a) the comprehensive control of behavior and (b) totalitarianism. There is nothing wrong with control itself. It is the means, and the only one, by which we acquire our most valued patterns of behavior. The problem with totalitarianism is not that it entails control and is comprehensive, but that the control exercised is aversive.

What about human freedom and dignity, you ask? What about responsibility and goodness? No problem, Skinner answers, providing you will use these words in the way that makes sense. Unfortunately, such usage is not common among scientists, scholars, or ordinary men. When they talk about man's freedom, they mean that his behavior is uncaused (p. 19). That meaning is patently untrue. The tenable meaning of freedom

is success in freeing oneself from negative reinforcement (pp. 26, 44). By the same token, dignity is the ability to attain positive reinforcement (pp. 44-59). Both are maximized in the new world Skinner designs. By his definition of that world, it cannot be otherwise.

As for responsibility and goodness—as commonly defined—no one in the new world would want or need them. They refer to a man's behaving well despite the absence of positive reinforcement that is obviously sufficient to explain it. Where such reinforcement exists, "no one needs goodness" (p. 67).

Skinner insists that man's very effort to believe that his behavior is free—in the meaning of "uncaused" behavior—has helped to bring us to the precipice. It has worked against our developing the technology of behavior that we so desperately need. To place a high value on our being responsible and good is also to work against ourselves, because these words are meaningful only if we live without sufficient positive reinforcements. To value these words is to preserve a social order that depends primarily upon punishment, that is, upon the application of aversive reinforcements or, what is also, if differently, aversive, the removal of positive reinforcements, this in an effort to remove undesired behaviors. One learns from punishment how to avoid it—not how to behave well. When, Skinner asks, will we stop trying to do the impossible—leaving it up to each person to learn how to behave without positive reinforcement?

I have lifted out what seems central and distinctive in Skinner's new presentation. I do this because it needs to be examined for its own qualities and, to the extent one can, apart from his position on the study of behavior. That position has frequently been evaluated. It embodies, I think, great powers over an important if narrow domain.

One can judge what seems newest in the present book even apart from the rest of his work. I take what is new to be his extended elaboration—and application to the ends of social action—of a thesis he has enunciated before: the thesis that what is positively reinforcing is right for the animal whose behavior is controlled by that reinforcement. He suggests that this thesis was forced upon him (1956, p. 233):

In my early experimental days [the one idea in my life] was a frenzied, selfish desire to dominate. I remember the rage I used to feel when a prediction went awry. I could have shouted at the subjects of my experiments, "Behave, damn you, behave as you ought!" Eventually I realized that the subjects were always right. They always behaved as they ought. . . . What a strange discovery for a would-be tyrant, that the only effective technique of control is unselfish.

We need not trouble ourselves about possible confusions here in Skinner's use of "is" and "ought" or the limitations of his philosophical position or the adequacy of his sociology. He himself tells us that his recommendations turn on a simpler point. For those recommendations to be workable, it must be the case that people will find positively reinforcing that which will save them. And he agrees that this may not prove to be true. What

people find reinforcing is a *joint* product of their biological capacities and the options that their environment provides. That environment may reward indolence rather than effort, deceit rather than honesty, withdrawal rather than courage, mindless consumption rather than the wise conservation of resources, aggression rather than affection, and so on. Skinner himself will not call such positive reinforcements right or good or, at the least, will not say that they are as right or good as he wants (nor does he draw the inescapable conclusion that our genetic endowment is not sufficient in itself to produce or protect the good). He agrees that every living man may be hopelessly incapacitated to be reinforced by what he should:

The problem is to design a world which will be liked not by people as they now are but by those who live in it. "I wouldn't like it" is the complaint of the individualist who puts forth his own susceptibilities to reinforcement as established values. A world that would be liked by contemporary people would perpetuate the status quo. It would be liked because people have been taught to like it, and for reasons which do not always bear scrutiny. A better world will be liked by those who live in it because it has been designed with an eye to what is, or can be, most reinforcing. [P. 164]

Skinner does not suggest who would be competent to decide what "is, or can be . . . right," or the means by which that decision would be made or validated, or the means by which recalcitrant people would be brought to the way they surely ought to go. That being so, the paying customers will behave as they ought to and will keep their money in their pockets.

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Like any good social scientist, B. F. Skinner knows that feeling and behavior are shaped by conditions external to the organism, and he has spent much of his professional life attempting to discover the precise sets of conditions ("contingencies of reinforcement") sufficient to produce the behavior society regards as desirable, and the by-products of that behavior—feelings and other subjective states. He is now at the point where he thinks he knows how to accomplish this. Although he is uncharacteristically reticent about exactly what they are, the technologies of behavior control are available, he says, to create the kind of social environment (and hence the kind of people) we want. Standing in the way of the effective utilization of these technologies are a couple of widely shared positive sentiments: "freedom" and "dignity," a pair of words with which society rewards and honors (i.e., positively reinforces) those people who seem successfully to resist or transcend the conspicuous constraints which attempt to control behavior.

Skinner is irritated and impatient with this situation. Rather like a Mad Scientist from some old low-budget movie, prevented by the ethics

and conventions of his profession from carrying on his Weird Experiments to save mankind from itself, he is driven to attack its most sacred cows. "Autonomous man," then, is the villain of this book and "the literature of freedom and dignity" which supports him in his delusion that it is he alone, the "inner man" (and not environmental contingencies), who deserves the credit and the blame for his good or bad behavior.

Given Skinner's disdain of freedom and dignity and his belief that persuasion ("the attempt to change minds") is merely a weak positive reinforcer, the most obvious logical question that occurs is why he bothered to write the book at all—a question raised by at least a few earlier reviewers. There is an answer, although it is a disquieting one: Skinner says that persuasion is effective only if there is already some predisposition to behave and that "we persuade someone by making a situation more favorable for action." By giving his explicit expert sanction for programs of behavior control to those in high places (the president, the attorney general), the book supports those who are already favorably predisposed toward behavior control. The book is obviously addressed to those Established enough to be able to make authoritative decisions (the royalist "we" is the book's most pervasive rhetorical device, as in, "What we need is a technology of behavior," etc.), and Mr. Nixon is exactly the man who, with his lust for perfect clarity—if not for logical consistency—could affirm freedom while acting to institute behavior control.

Why would Skinner be interested in persuading men in high places to initiate effective programs of behavior control? Under his system, he says, it would be environmental contingencies (not the inner man) which got the credit for the good behavior of individuals. But in a human society, this could mean only that the *designers* of the system of environmental controls (i.e., Skinnerians) would get the credit; and the fact that Skinner does not make public in this book *how* behavior is to be controlled suggests both why the book is addressed to an elite and why there is reason to be apprehensive about it.

These, of course, are political apprehensions, and it is remarkable that in a book about redesigning a culture in ways that would control behavior more benevolently—a project which, as Skinner says, involves "altering the conditions under which men live"—there is hardly a word about power or political struggle. It is not likely that this is an accidental omission. Struggles over the alteration of the conditions under which people live is exactly what politics is about, and powerful people do not often yield to others the right to alter the conditions of their lives. Powerless people are not so fortunate, and in Skinner's system, in which status would be explicitly a function of the specially advantageous environments to which one had been exposed (it does not seem to occur to him that in fact, if not in ideology, it already is), the underadvantaged would apparently be denied even the small compensation of the myth of their dignity and autonomy.

When questioned or interviewed, Skinner typically pooh-poohs the felt apprehensions about the totalitarian, 1984-ish implications of his programs

for behavior control with reassuring phrases like, "It is not the benevolence of a controller but the conditions under which he controls benevolently which must be examined." But he mentions only two contingencies: (1) that all control is reciprocal and (2) that the controller himself must be controlled by his system of controls. Neither is in fact very reassuring. Social control is no doubt reciprocal, but the reciprocities are seldom equitable, or there would be no problem of exploitation. Moreover, democracy (a system in which legislators live under their own laws) has never been any insurance against injustice.

If, as an essay in persuasion, Skinner's book begs some important logical questions and is frightening with its answers to others, are there other grounds on which "we" (ordinary people, that is) might find him persuasive? Skinner, of course, is a utopian, and if his plans for redesigning the culture contained some morally ennobling vision of the future, "we" might be persuaded in spite of "our" apprehensions. But Skinner is a utopian without a moral vision, and therein lies a problem. Listen to him: "Our task is not to encourage moral struggle or to build or demonstrate inner virtues. It is to make life less punishing." Life is made less punishing in Skinner's faith by moving away from reliance on primitive negative reinforcements (in which people accede to controls for fear of the consequences of not acceding: "Do it or I'll kill you") to greater reliance on positive reinforcements (which move people to behave in ways that are rewarded: "Try it, you'll like it"). Now there is no question that seduction is less punishing than rape, but as an ennobling vision, it sort of lacks something. Skinner says not a word about love.

Skinner is not merely for the reduction of environmentally caused pain, he is also for greater efficiency in the design of a culture in which behavior likely to be punished seldom or never occurs. "We" have already designed such a world for "babies, retardates, or psychotics, and if it could be done for everyone, much time and energy would be saved." The efficient husbanding of time and energy is a quality much admired in managers and administrators, but *for what* is a question that groups have been killing each other over for a very long time, and if the best answer that Skinner can come up with is to render the rest of us as painless as babies, retardates, and psychotics, the answer is again less than ennobling; in any case, I should prefer opium.

Skinner has trouble with "for what?" questions because he does not believe in the possibility of moral choice. For him, statements of value preferences are all reducible to "if, then" propositions. "Thou shalt not kill" *means* if you do not want to go to jail, do not kill anybody. The closest he comes to any statement of faith is in his affirmation of a kind of Darwinian evolutionism in which "survival is the only value according to which a culture is eventually to be judged." But even this is far from persuasive because Skinner has nothing to say about which specific practices do or do not have survival value, and from the perspective of deliberate behavior control, a survival ethic is less than useless because adaptive behavior is often unintentional and intentional survival behavior

often has the opposite consequences. At a time of ecological crisis, it is surprising that we even need to be reminded of those facts.

Moreover, cultures do not induce their members to work for the culture's survival, as Skinner suggests; they induce them to maintain specific practices and to change others (which is not the same thing), to affirm its sacred symbols and to resist their desecration. But in a complex society, the objects of maintenance and change, affirmation and desecration, are themselves matters of controversy; and if, as Skinner says, a culture is a set of contingencies of reinforcement, all people are always working for the preservation of some and the destruction of others, and the adaptive or maladaptive character of the consequences is as much a matter of politics (in which "history is the only judge") as it is of science. It is, indeed, one of those areas of public life in which it is difficult to make the separation.

Beyond Freedom and Dignity, then, is a book that is persuasive neither at the level of logic nor as a visionary appeal for a morally ennobling future. Still, Skinner's arguments warrant attention if for no other reason than that they are not apparently designed to win him popular acclaim. Imagine the effrontery: arguing against freedom and dignity! Unlike those unemployed aging actresses righteously announcing on television talk shows that rather than work nude they would prefer not to work at all, Skinner actually risks something. But the book is disappointing. It reminds me of a remark made in Wright Morris's *Love among the Cannibals*, a novel about two Hollywood songwriters down on their luck and trying to write a hit. "It doesn't have to be a good song," one of them says, "just one that *reminds* you of a good song." *Beyond Freedom and Dignity* reminds you of a good book.

Book Reviews

Law, Society, and Industrial Justice. By Philip Selznick with the collaboration of Philippe Nonet and Howard M. Vollmer. New York: Russell Sage Foundation, 1969. Pp. viii+282. \$7.00.

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At this time the sociology of law is quietly drifting toward a conflict between two schools of thought. The first is pragmatic and sometimes normative; the second strives for detachment and neutrality. The first moves freely between fact and value, seeing a rigid separation as undesirable if not impossible; the second clings to this separation and shows no sign of weakening. The first finds the second naïve, but is in turn criticized as confused. Call the first a natural-law approach, the second a positivist approach. However familiar and tiresome this debate may be in its jurisprudential version as well as in other areas of social science, it is a sign of life in the sociology of law, a field otherwise undisturbed and undistinguished by theoretical discussion. What is more, it would be irresponsible to offer an assessment of Philip Selznick's *Law, Society, and Industrial Justice* without explicitly taking sides in this controversy. Selznick's study is undoubtedly the most erudite and imaginative example of the natural-law approach to appear. Yet as an uncompromising adherent of the positivist approach I must locate his contribution in what Roscoe Pound called "sociological jurisprudence" rather than in the sociology of law. After reviewing the argument of Selznick's book, I shall return to the somewhat delicate but critical question of its intellectual identity.

Selznick argues that legal ideals previously monitoring the exercise of public authority can now be appropriately extended to the regulation of private authority, particularly to that lodged in the industrial employment relationship. This extension of the rule of law, he claims, is a tendency of moral evolution. He concentrates on the very edge of this evolutionary trend, showing how legal doctrine and the social characteristics of industrial employment are dovetailing toward a higher legality, specifically toward an expansion of the legal concept of governance and a recognition that governance is found in industrial as well as political life. Hence, Selznick looks forward to an application of "due process" standards of governance to private associations. He concerns himself with the capacity of private organizations to "establish justice," seeing that capacity as a function of the social dynamics of the organizations themselves (p. 34). Therefore, the legal sociologist finds himself in a position to justify legal reform and chart the course of legal change: "To extend the rule of law is to build it firmly into the life of society, to make the master ideal of legality a true governor of official conduct. . . . In contributing to that effort, students of law and society are confronted with a special intellectual

• problem: how to bring legal ideals to the 'private' sector of community life. . . . The issue is: Can we justify, within the framework of legal theory, the application to private organizations of principles hitherto restricted to public government? That question sets the theme of our study" (p. 35).

• The analysis begins with an overview and critique of the law relevant to the social system of industry—the law of corporations, of contracts, and of property. In each case Selznick finds that these legal frameworks "fail to grasp the reality of association" (p. 63). Property law, for instance, is thing centered, individual centered, and domination centered, while it ignores the power relations between persons that are created by property. Selznick protests that property law views the world as if Karl Marx never existed. In short, today's law of associations operates with outmoded concepts unenlightened about industry as a social arena and unequipped to appreciate the nature of membership and authority in the employment setting.

But Selznick is not satisfied merely to expose the law's incompetence from the standpoint of a sociologist; he also looks for moral pressures originating within the industrial bureaucracy. He finds a "strain toward legality" that is "natural" to bureaucracy (p. 93). Arbitrariness is inconsistent with the very constitution of bureaucratic organization. Drawing on Max Weber's work, Selznick notes that bureaucratic authority is "legal" because its legitimacy is warranted by rational principles and because it is formally committed to an order of rules, with every bureaucratic decision open to criticism in the light of established procedure and explicit purpose. Again, the source of these attributes is internal and the dynamic they create "calls forth the ideals of legality" (p. 81). At this stage of his argument Selznick also presents interview data from a 1958 study of 44 personnel directors, another way of peering inside the bureaucratic system. Examining their conceptions of fairness, he found a preference for limiting management's jurisdiction over employees and for justifying every extension of this jurisdiction. Hence, we see that some of the managers as well as the system of bureaucracy aspire for the rule of law.

After tracing the historical development of the law of employment from its beginnings in the master-servant framework, to employer-dictated contract, to the collective contract based upon bargaining between labor and management, Selznick pauses to consider the character and contribution of arbitration as a system of industrial justice. He reveals how "creative arbitration" in dispute settlement is another empirical element thrusting industry toward a regime of legality, thereby adding another link to his evolutionary argument.

One is struck by the enormous range of material Selznick covers in making his case. He even includes a chapter reporting employee attitudes about industrial justice, based upon a 1961 survey of almost 2,000 men in eight organizations. Here, for example, we learn that employees tend to oppose the use of seniority as a processing principle—a principle often thought

to be progressive and prolabor. There is thus a "significant gap between the subjective preferences of many employees and the rules by which they are governed" (p. 210). One at first wonders why Selznick includes interview data in a legal study. What difference does it make? In fact, it makes a great deal of difference in a natural-law approach. From this standpoint men's expectations and hopes are often part of the law that is becoming. If they are not, they at least indicate the law that ought to be, itself a major force ultimately defining the law that is. Natural law involves a conception of what Lon Fuller called "the law in quest of itself." Attitudes about justice, then, bring Selznick closer to emergent law and the law that is possible, closer to the point where the "is" and the "ought" meet.

The last major generalization in the book is that there has been an "erosion of public purpose" in the law of employment (p. 240). Because this law has been administered passively, with reliance on the private-law system of citizen complaint to select its problems, it has been little inspired by larger democratic ideals and values. Justice in employment relations has not been sought for its own sake as a public policy. Fair employment practices, for instance, have become a tangential concern of a larger civil rights program, not an end in themselves. The quality of governance in industry is hardly a focus of law at all. And yet the evidence seems overwhelming that industry is a "receptive institutional setting" prepared for the advent of legal change (p. 243; *italics omitted*). With his many diverse empirical materials—from case law to attitudes to legal administration—Selznick tries to show where the law has come from, where it is going, and where it could go.

While most of his study emphasizes the social system of employment as a component of industrial law in action, in the final chapter Selznick considers the conceptual system of public law as a resource and solution for industry. As he puts it, "We are interested in the posture of the legal system, its conceptual readiness to seize what we take to be an emergent historical opportunity" (p. 243). He finds the answer in the public-law concept of due process. Due process is a legal framework geared to the limitation of arbitrary power and is basic to the legal control of government officials. It is the heart of the law of governance. In brief, Selznick reasons that this law of governance properly refers to a social function involving the rule of some men by others, not to a particular institution, that is, not to public government alone. Private organizations have government as well. It follows that due-process constraints apply to the governance inherent in the employment relationship and to any other form of governance in private life. The book ends on this conclusion.

What is unique and significant in Selznick's study is his preoccupation with the coming into being of law. He tries to bring order to what he calls "incipient law" and "inchoate law" and more generally to the doctrinal evolution of law. Sociologists often shy away from the law that is unclear or ambiguous, the developing law or the law in dissolution. They look instead to the predictable law, the law in fact. Selznick, by contrast, not only seeks to specify where the law is going; he also tells us where the law

• should go if it holds to its own mission. He interprets the law. In my view, however, this is where Selznick leaves sociology and enters jurisprudence.

Selznick's analysis of the law follows along two axes, each pertaining to whether the law is true to itself. The first asks about the law's inner order, its latent values or ideals. These are the ideals of the legal system itself and of its participants; they may be delineated and applied to the evaluation of the law independently of the sociologist's own preferences, says Selznick. The second axis of his analysis examines the law's implicit conception of the world as it is. Every normative system implies a vision of reality, indeed, a sociology. As Selznick notes, "Norms derive, not from the ideals of the system alone, but also from knowledge of what men and institutions are like. Only thus can we know what norms are required to fulfill the ideal" (p. 28). Here the sociologist evaluates the law's social knowledge of its own jurisdiction. The law, then, is a system of both ideals and intelligence, each central to a sociological study of the law's effectiveness. Let us consider more closely the place of these elements in a natural-law approach.

How does Selznick explain legal change? While he exerts much effort simply trying to catch the pattern of history, and while he posits moving power in the attitudes and reactions of citizens to empirical legal life, at bottom the crucial stimulus to change comes from within the legal process itself. Law changes to meet the demands of justice, and justice is defined, paradoxically, by the latent values of the law itself. Conceptions of justice, moreover, are ever changing as the law confronts a changing world. I find, however, that serious problems arise in the application of this strategy, since what is just in the inner order of the law is far from apparent. If the law's latent conception of justice is even remotely as elusive as it seems, an approach to legal change that attributes explanatory power to this conception may leave something to be desired. What is more, to my mind the law's underlying mission cannot be discovered through the scientific method. Science is unable to divine what is just from the law's own point of view, since this is a moral and political question. Science may tell us what others define as just or right or good, but it alone cannot give us a normatively valid definition of the just. Even if Selznick is correct in observing that justice must be defined within the ongoing legal process, we cannot look at the question from within the process and at the same time remain neutral. Selznick attempted to make a kind of value-free value judgment. He attempted the impossible. Sociology can never discover the law's meaning from a legal standpoint; that is inescapably a normative question. When Selznick finds the law of employment inadequate to the demands of justice and asserts that the law is conceptually ready to extend due-process protections to private settings such as industry, he is far beyond a sociological perspective. He is an advocate. His conclusion is logically independent of his sociological investigation, and it has no empirical grounding. It is saturated with ideology and evaluation and interest. There is no other kind of legal interpretation.

Recall that Selznick's second major focus is the law's implicit image

of the social world. Again and again his argument emphasizes how legal ideas fail to recognize the sociological reality within which legal acts occur. Legal theory abstracts these acts from their social context and treats them as if they occur in a vacuum.

For this reason we may criticize the law and suggest how it might take account of the reality sociology reveals. Thus, for instance, the reality of employment involves power and governance, but the law operates as if power and governance were found in public administration alone. If only the law were realistic the protections of due process would be extended to employees vis-à-vis management. This is much like the argument so often made by sociologists that if the criminal law were only to recognize the sociological and psychological reality of crime it would have to discard the traditional framework that casts the criminal as a free agent who wills his crime and is held responsible as if he could have done otherwise. Similarly it is commonly noted that contract law proceeds as if differences in bargaining power did not influence the nature of contracts. But I wonder why law should present an accurate sociological analysis of its jurisdiction. Law is not science and was never meant to be. Law is normative. A scientific critique of law therefore makes no more legal sense than a scientific critique of religion makes theological sense. But surely Selznick does not think that the social nature of industrial employment is unknown to law-makers and judges. Surely he means something else. He must mean to be making a *case*, a case that employees should have more legal advantages than they now enjoy. That is well and good, but sociology alone will never be enough to compel a normative conclusion of that kind. Selznick, however, tries to reason from his sociological description to the needs of the law, to what he calls "legal implications." But science cannot judge the law. Sociologically, the path of the law is wherever it goes, and no sociologist, as such, can point its proper direction.

Law, Society, and Industrial Justice is an impressive contribution to sociological jurisprudence, a field of legal scholarship concerned with assessing the law in the light of the social system's functional requirements. Accordingly Selznick interprets the law's own aspirations, just as a social critic might interpret a society's cultural values and draw policy implications with respect to how those values could be realized. In both instances the interpretation necessitates social partisanship, since the meaning attributed to the law or to cultural values generally implies differential costs and benefits across the society's membership. Selznick, for example, interprets the law in the interests of labor. His is a liberal jurisprudence. This is also apparent when he assesses the legal requirements of the employment relationship considered as a sociological entity, apart from the question of the law's meaning. I am not saying Selznick is biased; I am saying that he, like any jurist, is normative and political at the core of his analysis. Even if we accept, *arguendo*, that legal criticism can be done without partisanship but with the interests of the whole society alone at stake, it still must be normative and political. Such would be a form of social eudaemonism, an ethic holding that what is right is what advances the well-

being of the whole society. The needs of the whole take priority over all other interests. This would be functional jurisprudence with a vengeance, a kind of sociological totalitarianism. It is hard to tell just what kind of jurisprudence Selznick favors, and that is precisely because he is not explicit about his legal philosophy. Instead he presents his study with an air of innocence, as if it were sociology and nothing more.

In the study of law it is still important to be clear about what sociology is and what it can do. Sociology cannot tell us whether the state should hold employers to due-process standards in their authoritative dealings with employees, and a book that seeks to "justify" that reform is not sociology. Selznick's argument is part of a larger movement toward an ever greater involvement of the state and law in the private affairs of the citizenry. The book may even add to the progress of that evolutionary trend, a trend Lon Fuller once described as "creeping legalism." Selznick both celebrates this trend and applies sociology to its advance. One could as well apply sociology to its reversal on the ground that government intervention is dangerous, an evil to be avoided at all costs, even if intervention would appear to be benevolent. To ask the law's help is to widen its already vast jurisdiction. I do not want to make that antilegal argument at this time, but mention it only to dramatize the distinctively political character of Selznick's study. The use of sociology to sponsor jurisprudential premises is misleading if it is not deceptive. The search for "natural law," for value in the world of fact, is hopeless. *Law, Society, and Industrial Justice* is a very fine, even extraordinary piece of legal scholarship. It displays much craftsmanship, depth of learning, and creativity. It is elegant in style and graceful in presentation. Every legal sociologist should read it knowing exactly what it is—a good example of what the sociology of law is not. To say this about a book by Philip Selznick seems almost outrageous, and it must seem ungrateful as well. But at the beginning I gave warning of my positivist commitment.

Knowledge from What? Theories and Methods in Social Research. By Derek L. Phillips. Chicago: Rand McNally & Co. Pp. xx+204. \$3.95 (paper).

Hugh Mehan

Indiana University

The title of the book reflects the author's critical examination of the measurement processes in social research which produce sociological knowledge. Derek Phillips's discontent with existing measurement techniques has a biographical origin in his own expert survey research in mental health. Phillips found that the survey does not systematically consider the sources and consequences of the factors operating in the data collection process. This realization led him to consider the validity of

existing measurement instruments, to recommend improvements, and suggest alternative sociological conceptions.

To Phillips, a fundamental irony in sociology is that it professes to be the study of human interaction, yet the most frequently used sociological instrument prohibits direct observation of this phenomenon. The vast majority of sociological studies (90% in *ASR* between 1962 and 1969; 92% in *AJS* and *ASR* in 1965-66) utilize the survey which requires respondents to report on their behavior or feelings.

Although measures may be invalidated by showing they correlate highly with a simpler variable or correlate poorly with an independent measure, few survey instruments are examined for systematic invalidity and bias. When studies of respondents' personal characteristics (e.g., age, marital status) and their personal activities (such as voting, deviant behavior) have been checked against official records, a low correlation between self-reports and official records has been found. Despite sociology's concern for objectivity and control, the potential source of interviewer-respondent interactional contamination in data collection (e.g., the way questions are read, the race and sex of researcher) located in psychological lab studies have not been considered as variables in survey research. Phillips says the validity of survey results cannot be assured until the influence of situational factors is proven to be unsystematic.

Phillips suggests improvements and recommends alternative strategies based on his critique of current measurement practices. He urges more validation studies, the inclusion of interviewer and respondent factors in research design, and recommends the simultaneous use of many data-gathering techniques. Although Phillips says the respondents' meaning of the research situation should be a factor, he overlooks the necessity of examining their interpretations of stimulus items and instructions. The recent work of ethnomethodologists (Garfinkel, Cicourel, and his students) have shown the necessity of examining the assumption that the respondent and researcher share the meaning of interview items as a potential source of bias. The limited space which Phillips devotes to the problems that language and meaning pose for survey research is one of the few shortcomings of the book.

He dismisses "non-reactive" or "unobtrusive measures" as alternatives to surveys because they avoid possible interactional contamination at the price of extreme behaviorism. Removing the observer from interaction with the respondent forces the researcher to interpret behavior out of ignorance. Researchers always view respondents' behavior against background assumptions. Because the researcher's knowledge about social behavior is dependent upon his research methods, which in turn, are dependent upon the researcher's social knowledge (p. 53), the more the researcher knows about the everyday life of his subjects, the more confident he can be of the observations he makes.

Phillips recommends more direct study of action in everyday life, which leads him to consider the merits of participant observation. He counters the criticism that fieldwork is subjective by saying all research is sub-

jective in that action produced by a person is interpreted by a person (p. 129). Because all research is interaction, Phillips feels sociology should be studying the interpretive sense-making and decision-making processes which are fundamental activities in everyday life and sociological research, with the goal of understanding this process, not correlating facts external to the member and observer.

Phillips proposes ideal type analysis as an alternative model for sociological theory, which causes him trouble over issues of theory construction, validation, and explanation. Ideal types have been proposed as alternatives of current forms of theorizing before. Phillips does not counter the criticisms these suggestions have encountered (e.g., Paul F. Lazarsfeld, "Philosophy of Science and Empirical Social Research," in *Logic, Methodology and Philosophy of Science*, ed. Ernst Nagel et al. [Stanford, Calif: Stanford University Press, 1962]); he says explanation and intersubjective verifiability will be achieved when the researcher can construct the rules by which action is followed such that another person put in a similar situation would have similar experiences (pp. 161-62). Sociologists "are able to understand people's behavior if they understand their motives, goals, choices and plans" (p. 160). By defining "understanding" as "knowledge of internal states" Phillips has undone the thread of his argument, for as he has said, the survey is the most appropriate instrument to make internal states amenable to study (p. 100). But the sociological topic is not motives and attitudes per se, but the actualization and interpretation of action in everyday social situations.

Phillips's suggestion to define explanation as rule following is weak on three counts. (1) The ethnomethodologist has shown normative rules are incomplete in that they do not tell rule followers how to follow them; "interpretive procedures" are needed to understand surface rules. (2) "Cognitive anthropologists" attempt to specify the cultural rules necessary for interaction. Phillips's discussion of rule following would have benefited from consideration of the literature in that field (e.g., Tylor, *Cognitive Anthropology*; Hymes, *Language in Culture and Society*). (3) To say "X did Y because they are friends" (p. 162) is to say X followed a rule. But Mannheim, on "The Interpretation of Weltanschauung" (*Essays on the Sociology of Knowledge* [London: Routledge & Kegan Paul, 1952]), has shown the interpretation of action can take place on at least three levels, depending on the observer's familiarity with the actor. Phillips does not provide criteria by which decisions about which rule is being followed can be made.

Phillips's recommendations for the alternative to survey research is underdeveloped in that he devotes sentences to topics that require paragraphs and pages. (Only 50 of 200 pages are devoted to "the alternative.") Perhaps this disproportionate coverage reflects the state of the published art of interpretive sociology. Perhaps we must wait for the further publication of ethnomethodological studies to flesh out the interpretive sociological model.

Despite the abbreviated treatment of interpretive sociology, Phillips

has contributed a rich and substantial document to the discussion of measurement. The book should be required reading in methods courses because he has presented a concise and readable portrayal of the existing rationale for survey research, an excellent critique of this technique, and an adequate presentation of alternative sociological conceptions.

Multivariate Model Building: The Validation of a Search Strategy. By John A. Sonquist. Ann Arbor: Institute for Social Research, University of Michigan, 1970. Pp. xiii+244. \$7.00 (cloth); \$5.00 (paper).

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When researchers learn of new techniques or computer programs for analyzing social surveys, too often they are unable to find any careful investigations of the ways the new tools respond to the properties frequently encountered in nonexperimental data, such as multicollinearity, noise, non-normality, small sample size, and complicated causal structure. If at all, it is only after a technique has been widely used over a long period, or has been the basis of a controversial substantive finding, that important guidelines and caveats for its use come to light.

Multivariate Model Building is an important exception to this pattern. In this book, Sonquist undertakes the investigation of the properties and usefulness of the Automatic Interaction Detection technique (AID), which he helped to develop a few years earlier. The major guidelines for the use of AID are derived from an analysis of hypothetical data generated from three underlying models, one without any interaction among the independent variables and two with interaction. Data from these models with different degrees of skewness in the variables are analyzed by both the AID technique and the Multiple Classification Analysis method. Conclusions are drawn from the way each method reveals the true underlying models and relative importance of each predictor used to generate the data.

The AID is a stepwise program which operates on a continuous dependent variable and categoric independent variables. Under the AID algorithm, at each successive step that dichotomous split on any independent variable is found which gives the lowest within group sum of squares for the dependent variable for any other split of the predictors. After each such selected dichotomization, the resulting subgroup with the largest within group sum of squared deviations is then eligible for splitting by the same search strategy. The sequence of splits continues if the eligible groups have at least the number of cases and within group sum of squared deviations specified by parameter cards designated by the program user. In the end, what results is output which can be arranged as a tree of successive subdivisions of the population on categories of the independent variables.

The appearance of the AID tree structure is shown to be an important criterion for deciding whether there are certain kinds of interactions among the independent variables. In the absence of interaction in an additive model, a symmetric tree results where each branch is composed of subgroups based on the same selected independent variables. When certain interactions are present in the model, the predictors do not behave similarly in the various branches of the tree. In interactive models where effects are cumulative or substitutive, at each split a subgroup is identified which is homogeneous to the dependent variable and will not be split any further in subsequent applications of the AID algorithm. The resulting tree structure has only one branch which grows along single subbranches, rather than symmetric splits in every subgroup in the final additive tree.

While the AID technique was found to identify selected interaction patterns, it was not successful in directing the analyst to clear conclusions about the relative importance of the various predictor variables. The order of variables in the splitting process was not an adequate indicator of a variable's importance. Like other stepwise routines, a great deal of attention to intermediate output was required, without always clearing up points of confusion about irrelevant correlates of powerful effects or the primary explanatory variables.

The author concludes that correct unambiguous conclusions about the relative importance of predictor variables are arrived at more successfully with methods such as Multiple Classification Analysis, which present net amounts of variance explained by individual variables in a single analysis of the complete model. Of course, the model analyzed in this way must correctly reflect any interactions among well-conceptualized variables. It is in setting up the correct model that AID has its major use.

In the end, a research strategy is proposed which involves the joint use of the AID technique and Multiple Classification Analysis. AID is used to check on peculiarities of scaling and then to scan for interactions among independent variables. With the results of the AID investigations, new indices are to be constructed to provide interaction terms for subsequent Multiple Classification Analysis (or multiple regressions). Many important insights and details are given on how to go through these individual steps.

In the earlier chapters, Sonquist reminds us that knowledge of interactions in social data is often not merely a refinement in a substantive argument expressed first in additive terms, but is frequently the basis for a very different causal story. With a series of examples from sociological studies and a classification of types of interactions, he argues that the routine search for interactions should occur early in empirical studies to develop theory, rather than as an afterthought late in the research. The strategies suggested from his analyses of the properties of the AID technique give researchers a practical approach for following this dictum. As the author suggests, further work is needed to demonstrate the viability of AID for models with intercorrelated predictors which are typical in nonexperimental studies and with more complicated causal structures. As

an initial validation, this book is an important service to researchers who wish to consider the AID program in their empirical work.

Le littéraire et le social: Eléments pour une sociologie de la littérature. Edited by Robert Escarpit. Paris: Flammarion, 1970. Pp. 315. Fr. 7 (paper).

The Sociology of Art and Literature: A Reader. Edited by Milton C. Albrecht, James H. Barnett, and Mason Griff. New York: Praeger Publishers, 1971. Pp. xiv+752. \$15.00.

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The sociology of art and literature has long floundered between the Scylla of grandiose, vague, and unprovable generalities and the Charybdis of data gathering to no apparent end beyond that of description. What the field needs, and what it has little of, is more solid, substantive work that will steer a course between the two. Here are two books which ought to stimulate interest to do just that. In these days of cultural crisis in and out of the classroom, sociologists might do well to look to literature and the arts for new perspectives. As the first comprehensive collection of work in this area, *The Sociology of Art and Literature* will prove extremely useful in the too few courses on the sociology of art or literature that are given on American campuses (I have used it in such a course). It may even prompt sociologists to offer more such courses or introduce these and other materials into courses on the sociology of knowledge (see the article by A. Kern, pp. 549-56); small-group theory (see S. Forsyth and P. Kalenda on the organization of a ballet company, pp. 221-55); the mass media (see K. Lang, "Mass, Class and the Reviewer," pp. 455-68); or mass communications and mass culture generally (see A. Toffler, "The Culture Consumers," pp. 483-98; J. Bensman and I. Gerver, "Art and Mass Society," pp. 660-68).

If these two works point up the underdevelopment of the sociology of art as a whole, they also illustrate the considerable differences between what one might call the American eclectic approach and the more directed, intense orientation that certain European scholars bring to the study of art and its multiple ties to the society in which it is created, diffused, and consumed.

The Sociology of Art and Literature demonstrates to perfection the range of American eclecticism. Sociologists account for a minority of the contributors, who include art historians, art and literary critics, an artist, anthropologists, philosophers, historians, and free-lance critics. Such diversity means that while all of the articles raise matters of potential concern to sociologists, many, perhaps most, focus on the particular subject rather than on analytical issues. Reference to social structural variables

is apt to be implicit instead of explicit. One article at least expressly eschews "scientific terminology" (p. 195).

For this reason, the majority of articles do not lie within the mainstream of sociology (or even any of its tributaries) that is close to work which might provide points of reference and a more general theoretical framework. (The principal exceptions to this stricture are Milton Albrecht's introductory essay, "Art as an Institution," reprinted from the *ASR*, the articles in the section on methodology, and James Barnett's brief review, reprinted from *Sociology Today*.)

The volume is divided into six sections, each of which has a brief introduction and a commented supplementary bibliography. Part 1, "Forms and Styles," ranges from A. L. Kroeber's general "Style in the Fine Arts" to a detailed analysis of architecture of the Christian basilica and its correspondence to the theological hierarchy of Christianity by the art historian, F. van der Meer. Part 2, "Artists," covers "Socialization and Careers" as well as "Social Position and Roles." Notable here is Mason Griff, "The Recruitment and Socialization of Artists" (from the *International Encyclopedia of the Social Sciences*) and the study by Anselm Strauss of the Chicago Art Institute School.

Part 3, "Distribution and Reward Systems," includes Harold Rosenberg's lively views on the vagaries of "The Art Establishment" and studies of the effects of patronage in the Middle Ages and Renaissance. In part 4, "Tastemakers and Publics," two articles coauthored by Bernard Rosenberg and Norris Fliegel tackle the organization of the art world (museums, critics, agents). Alvin Toffler's assessment of the potential and actual positive contribution of a mass public for the arts should give pause to the more truculent of the "mass culture" critics, who typically concentrate more on the (real and fancied) plight of the artist rather than the attributes of the public (the concern voiced by John Dewey and Kenneth Clark, and Renato Poggioli's appraisal of artists' penchant for self-pity and ostentatious alienation, all in part 6).

Part 5, "Methodology," presents a number of prolegomena to the study of art and society, plus some caveats of what not to do. Here it would have been instructive to descend into the realm of the particular to illustrate a general methodological approach by a specific study. George Huaco does this to some extent in his discussion of films, but his macroscopic model (modified Marxian) is too general and his middle-range model tantalizing but summary. Finally, part 6, "History and Theory," is something of a residual category that ranges from Hugh Duncan on Kenneth Burke to considerations of mass society and its effects on art and the artist.

There is then a tentative air about this reader, due partially to the necessarily short or truncated studies, due as well to the number and variety of topics covered. There are, as there must be, areas left unexplored—nothing, for example, on the influence of art on society, a lack which the editors justify (p. xiii) by the absence of sound studies on the subject. Still, and especially given the exploratory nature of this volume,

something might have been included (on mass communication, studies of propaganda, the mass media, even something by McLuhan). Nor is there anything on content analyses, which are perhaps more typical of what sociologists have done to date (although most deal with literature of doubtful aesthetic quality). In many respects, the lacunae of this reader are those of the sociology of art itself, which is unintegrated at the least and unsystematic at best. Albrecht's introductory essay reviews general classifications of art and its functions in the theoretical schemes of Parsons and others. Relating these to specific studies was beyond his scope and intentions.

The view from France is different indeed. Whereas many of the contributors to *The Sociology of Art and Literature* would be perplexed at the appellation "sociologist of literature" (art), the French practitioners clamor for the title, and this despite the training of most of them in literature. Whatever their provenance or institutional affiliation, those who call themselves sociologists of literature in France are conscious of undertaking *sociological* studies. Despite the lack of agreement as to what this entails and regardless of one's opinion of the divers definitions promulgated, there is no doubt that the work of the French is informed by a "discipline consciousness" missing from the dispersed American endeavors (I say American since, the foreign scholars represented in the Albrecht et al. volume notwithstanding, the perspective has been set by American sociologists).

Le littéraire et le social reflects this more focused orientation. It was produced under the direction of Robert Escarpit, professor of comparative literature at the University of Bordeaux, who, by dint of dogged persistence over many years, has created an institutional framework for researchers of all persuasions, from France and elsewhere, in the Institut de littérature et de techniques artistiques de masse. Escarpit's work over the past 15 years or so has mainly consisted of empirical studies of reading publics, patterns of distribution and consumption, the determinants of commercial success.

The article here entitled "Succès et survie littéraires" touches on problems such as the age of optimum productivity, the ever-increasing importance of book reviews, the writer's similar status in socialist and capitalist systems of government, the nonliterary functions of books. This and his other two articles in this collection (and the one on publishing translated in *The Sociology of Art and Literature*) are generally representative of Escarpit's concerns and the careful, empirical approach he has followed. (See also the appendix on the reading habits of army recruits.) In this he is closer to some of the work of American sociologists, especially in mass communication, than his younger collaborators represented in *Le littéraire et le social*. The considerable data collected tend to lack a unifying perspective, and despite the importance of the problems raised (in particular the article "Littérature et développement" on the cultural imbalance between the developed and the "underdeveloped" countries), one is almost

tempted to ask, data for what? Indeed, G. Mury all but does so (pp. 205-6). Data on who reads whom and what, where, and when needs to be ordered by hypotheses about *why*.

At the opposite end of the spectrum lie the theoreticians for whom in the beginning there was Marx, who begat Georg Lukács, who begat the late Lucien Goldmann (represented in the Albrecht et al. volume). For Goldmann and his disciples, the sociology of literature views the social class origin of a literary work as its genesis. Goldmann exercised considerable influence—almost every contributor to *Le littéraire et le social* felt it necessary to tackle him, for good or for ill.

The articles in *Le littéraire et le social* fall into three categories. First, the review articles of Escarpit, J. Dubois, and P. Orecchioni discuss past approaches, from Goldmann's sociology of creation to the content analyses of certain American sociologists, from qualitative to quantitative analysis, and the functions of each. All three stress literature as a communication process rather than as a product and imply that this is where the future of the sociology of literature lies, not in the Marxist-oriented reductionism of Goldmann. So, too, C. Boazis criticizes Goldmann for failing squarely to confront the issue of causality between literary structure and social structure and calls for research on the mechanisms of interaction between the two in order to go beyond simple parallelism of structures (p. 112). He is less than clear on just how this is to be accomplished, but like G. Mury's exhortation to Marxists to engage in empirical studies to support their theories (p. 220), this perspective at least points research in the right direction.

Like Boazis, H. Zalamansky focuses on the work of literature itself, but unlike him he proposes to study its ideological content as an indicator of the solutions, ideas, problems, and models proposed to the reading public. He, too, criticizes the excessive generality of Goldmann and others who concentrate exclusively on works of recognized aesthetic merit, which may but probably are not representative of a given period in time. The third group of articles centers on the public. N. Robine considers problems of motivation: why people read and what conditions historically favored the development of an extensive reading public. R. Estivals's discussion of "bibliologie" and "biliométrie" treats quantitative indicators of intellectual and cultural production and the potential contribution to the study of innovation, evolution of the market, and changes in intellectual activity over time.

Le littéraire et le social was obviously conceived in a very different spirit from *The Sociology of Art and Literature*. More than an assessment and sampler of past work, it presents working papers for the future. It is more limited, if only because it treats literature alone. More closely tied to French research, the Escarpit volume also presupposes (and essentially requires) a certain familiarity with the debates current in French intellectual circles over structuralism, semantics, linguistics, formalism, Marxism (and an ability to decipher the jargon, especially in Boazis's article).

Furthermore, whatever their own intellectual persuasions, American sociologists will find the French largely ignorant of work they consider significant: of Mannheim in the sociology of knowledge; of the theories of culture of Sorokin, Parsons, Kroeber; the analyses of ideology of Clifford Geertz, and others. The programmatic nature and the inevitable repetitions lend these articles an air of superficiality, although covering the surface—raising problems not solving them—is obviously the goal.

Yet in other respects these works are not so dissimilar. In both each writer tends to start anew, choosing his filiation and affiliation, indeed defining his own brand of sociology of literature, while he neglects or remains ignorant of others not congruent with this project. If it is ever to come into its own, the sociology of art and literature needs improved communication and, ideally, work that will incorporate the best of the traditions represented here. Then perhaps we can avoid the fallacy of misplaced concreteness inherent in unordered data or descriptive material no less than the excesses of unsubstantiated generalities. If these works stimulate interest in the sociology of literature, we may hope that they will also engender new perspectives.

The Cloak of Competence: Stigma in the Lives of the Mentally Retarded.
By Robert B. Edgerton. Berkeley: University of California Press, 1971.
Pp. xv+233. \$2.65.

Walter R. Gove

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This is an ethnographic study of 48 mental retardates living in the community. The subjects were a convenience sample (i.e., geographic residence) from a cohort of 110 mental retardates who were the most competent socially and most able intellectually of the patients who were hospitalized at Pacific State Hospital over a 10-year period. Because of the stringent selection, these subjects were much more likely than most retardates who have been institutionalized to make an adequate adjustment in the community.

The book is well written and intrinsically interesting, although the two sections where the case histories of the subjects are presented are tedious. Edgerton starts with a brief introduction to the problem of retardation and the methods used in the study. He then has a very long chapter (81 pages) devoted to detailed portraits of four members of his sample. The third chapter deals with some of the central concerns of the retardates, namely (1) making a living, (2) sex, marriage, and reproduction; and (3) leisure activities. The fourth chapter deals with the subjects' endeavor to seem

normal to others and themselves. The fifth chapter focuses on the role of "benefactors": one of the most important findings of the study is that even among this relatively competent group of retardates the subjects almost invariably required the assistance of others. These benefactors assisted in the management of daily affairs and/or crises (which the retardates frequently precipitate) and helped the subjects pass as normal. The final chapter summarizes the findings and draws a few straightforward implications for institutional programs.

As with most ethnographic accounts the book is largely atheoretical; however, theoreticians will find it suggestive and heuristic. One area of particular interest is that of self-identity. As Edgerton points out, these former patients are not social deviants who have rejected the normative expectations of the "outside" normal world. Instead their every effort is directed toward effecting a legitimate entry into this world. Because the subjects are so open and their life so relatively simple, the basic components of a positive self-identity among these subjects appear more sharply than they do in normals. The role of a job, of marriage, of children (most of the subjects had been sterilized), of material possessions, and of the many skills we take for granted (e.g., the ability to read, to make change, to keep time, to converse) in maintaining a normal self-identity become particularly clear. It seems to me that the understanding produced by this book of the problems mental retardates have in maintaining a normal self-identity will assist in the understanding of the problems and behavior of other disadvantaged persons.

As someone concerned with societal reaction theory, I was particularly interested in the implications of this study for that perspective. The ex-patients uniformly saw their hospitalization as stigmatizing and attempted to hide their past from others, as the societal reaction perspective would suggest. However, as Edgerton makes clear, hospitalization, in a number of ways, helped to enhance and even aggrandize the self-image of these retardates. Furthermore, upon returning to the community, their past hospitalization served an important function in maintaining a modicum of self-esteem. Briefly, the subjects attributed the problems they had in the community to their experiences in the hospital (they uniformly insisted they were inappropriately hospitalized), and this allowed them to deny that these problems were due to retardation. Societal reaction theory also suggests that having been institutionalized would cause others to treat the subjects as deviants. However, the retardates' benefactors, who almost uniformly knew of their retardation and usually of their hospitalization, were generally involved in a "benevolent conspiracy" which consisted of helping the subjects deny their retardation. The question of societal reaction, stigma, and self-esteem appear, from this study, to be much more complex than the societal-reaction theorists have led us to suspect.

In conclusion, this book will be useful to students of self-identity and should be required reading for persons interested in the concerns and behavior of mental retardates.

The Lively Commerce: Prostitution in the United States. By Charles Winick and Paul M. Kinsie. Chicago: Quadrangle Books, 1971. Pp. ix+320. \$8.95.

James L. Wunsch

University of Chicago

Prostitution, the ancient social evil, in New York City at least, is still controversial. Mayor Lindsay ordered the roundup of streetwalkers in the Times Square area; a self-righteous judge denied bail to one of the alleged prostitutes, and women's liberation protested: why do the pimps and "johns" escape punishment while these unfortunate women are harassed? There is even now talk of legalizing the trade.

Charles Winick and Paul M. Kinsie understandably believe that the problem is too often treated irrationally and without sufficient information. Their new book, a study of prostitution in the United States during the last half century, held promise of answering the need. Mr. Kinsie and Professor Winick, respectively former investigator and former research director of the American Social Health Association, had access to the files of an organization which has been studying the problem for more than five decades. Two thousand interviews were conducted across the country over the past 10 years. The U.S. Department of Justice and the Russell Sage Foundation supplied funds for additional research and typing.

The subject is considered from several point of view, including that of the prostitute and her client, policeman and judge, pimp and madam, even cabdriver and bellhop. Winick and Kinsie suggest that since 1920 overt prostitution has declined. Where the brothel and red-light district were once commonplace, now even the experienced cabdriver cannot find "the action." Streetwalking may have become more concentrated in parts of certain cities (white men will no longer travel to black neighborhoods for women), but this too has declined. The prostitutes of the infamous whorehouse have been replaced by the call girl living in independent isolation in her highrise apartment.

The decline of bold and open prostitution in a large number of cities is attributed to "vigorous law enforcement" (p. 225). Effective measures to suppress the business were taken during the First and Second World Wars when federal officials determined that troops sent to fight overseas should be free from venereal infection. There was also a marked decline in prostitution during the 1950s, while over the past 20 years legislation has become increasingly stringent.

The authors might have given greater stress to factors other than law enforcement to account for this change. Improved methods of birth control and changing moral standards enabling respectable women to have premarital intercourse and more satisfactory relations during marriage may have sharply cut into the demand for prostitutes. John Gagnon ("Prostitution," in *The International Encyclopedia of Social Science* [New York:

Macmillan Co., 1968], p. 593) argues that these and other factors, far more than police action, will explain at least the decline in incidence of contact between prostitute and client.

That law enforcement did have substantial effect on prostitution is a thesis which at least deserves respectful attention. The problem with this book, however, is its complete failure to document and consider facts systematically. The reader is assaulted with bits and pieces of information from dozens of cities, but these are not organized to make effective argument. For example, the closing of red-light districts during World War II, notably in Terre Haute, Indiana, and Hawaii, is allegedly related to the fall at the same time of venereal disease and crime rates. The conclusion is that "when prostitution declines, so do other crimes" (p. 225). But the matter is not pursued in the postwar period where the relationship between the decline of prostitution, VD, and the crime rate is not self-evident. Significantly, it is mentioned that prostitutes, many of whom are addicts, attract to an area the pushers and other addicts who commit crime. But what indeed happens to the prostitute and these others when the red-light district is "cleaned up"? The matter is not discussed. Typically, questions here are considered neither extensively enough to generalize nor intensively enough to illuminate the mechanism of change.

An act of faith is demanded to appreciate the use of statistics. The authors allege that the 95,550 arrests for prostitution and related offenses in 1968, "a typical year" (p. 4), represents the number of full-time working prostitutes. The unwarranted assumption is that the multiple arrests for individuals is equivalent to the number of those who escape arrest. Winick and Kinsie consider their figure to be conservative because some students, unnamed or acknowledged, have offered even higher estimates. Of the many undocumented facts, the determination of the following statistics is left unspecified: "The average full-time prostitute works six days a week and has three clients daily" (p. 4); "many streetwalkers often average twelve hours a day" (p. 26); "the average call girl sees perhaps sixty men a month" (p. 176).

Some sources which are cited further undermine the book's authority. Reference is made to Nell Kimball, "one of the best known madams" (p. 106). In selecting quotations from her recently published memoirs (*Nell Kimball: Her Life as an American Madam*, ed. Stephen Longstreet [New York: Macmillan Co., 1970]), the authors might have detected that portions of her account were paraphrased directly from the works of a popular historian, Herbert Asbury, who died several years ago. It is doubtful whether Nell Kimball ever existed. Her memoir is fake. (A full explanation of this fraud may be found in *Journal of Social History* [June 1972].)

Finally, when we are told that Clarissa Harlowe "works in a brothel" (p. 17), we realize that the authors have neither read Richardson's novel (*Clarissa*) nor even a good plot outline.

Of 2,000 interviews ("actual names of specific respondents and other persons are given wherever possible" [p. vi]), three citations may be found

in the footnotes; there are two references to unpublished material in the files of the American Social Health Association.

In *Prostitution and Morality* by Harry Benjamin and R. E. L. Masters (New York: Julian Press, 1964), a book with a chapter format strikingly similar to *The Lively Commerce*, an entirely different point of view is maintained. Where Winick and Kinsie argue that the brothel and red-light district contributed to high rates of crime and venereal disease, Benjamin and Masters question any such relationship and assert that brothels sanctioned and properly regulated by the state might provide a reasonable service to the community. In the course of their argument, Benjamin and Masters assail the American Social Health Association for "having distorted some facts about prostitution and suppressed others . . . this organization has done more than any other . . . to thwart a sane and scientific approach to the problem in the United States" (p. 341). Benjamin and Masters were unconvincing in proving this, but now Winick and Kinsie with their efforts begin to make the charge seem credible.

On Record: Files and Dossiers in American Life. Edited by Stanton Wheeler. New York: Russell Sage Foundation, 1970. Pp. 25+449. \$12.50.

Ernest van den Haag

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As population density increases so do transactions among people who do not know each other. This creates a mounting demand for accurate records of career and personal data. While contributing to this demand modern technology has also made it possible to supply it, because extensive records can be kept and fairly easily retrieved at low cost now.

In the volume under review expert contributors describe, in almost painful detail, how records are kept. These descriptions will be invaluable to anyone wishing to do further work on the subject. Such work is needed. For while records answer questions, they also raise questions about the legitimacy of questions and answers. What should be recorded? Who should have access to records? Who is allowed, or obligated, to make what information available to whom? What about misinformation and correct, but irrelevantly damaging information? What is in the public domain, and what is private? What about interpretation, misinterpretation, and misuse?

The authors of *On Record* valiantly attempt to answer such questions. Success is necessarily spotty. But they have the great merit of throwing the theoretical problems into relief by describing the actual processes of record keeping and by sketching the distribution of the authority which shapes these processes in numerous institutions.

Collecting compelling information, keeping records, and making them available has become quite controversial as damage from misinformation has become patent and frequent. People are jealous of their threatened privacy. We have become afraid of misinformation, or misused information

which can hinder an individual's legitimate future activities when his recorded actual or alleged past leads people to deny him opportunities.

Some opposition to data collecting is legitimate, but some is misdirected. I can see no justification for compelling people to give census data (utility hardly justifies compulsion). But there are objections also to allowing the government to collect dossiers, by means other than compulsion, on citizens, subversive and otherwise. The fears underlying these objections are understandable and sometimes even justifiable. Yet total opposition seems besides the point. Collecting (though not compelling) information is everyone's right, and some people's duty. Police departments must have information on suspects; business needs information on consumers, employees, and credit risks; government needs information on citizens. Those who try to prevent the keeping of records aim too broadly. However, everyone has an interest in preventing misinformation from being recorded and distributed, or information from being misused.

How can this interest be protected? The law might impose fairly strict liabilities on distributors of information; liabilities which cannot be discharged by disclaimers. Liability should be canceled only upon giving the subject of the information a reasonable opportunity to comment and either using the comment to his satisfaction or adding it to the information. Such a rule might be applied to all information regularly disseminated to third persons or within the government and other institutions when used to decide on promotion, hiring, training welfare matters, etc.

Information can never be harmful; only misuse and misinformation can. Hence efforts should be made toward a framework which minimizes only these. Anyone who wishes to try his hand in elaborating appropriate rules and institutions to safeguard against abuse of records will find in this book not only the materials he must work with, but also many useful hints toward this end.

They're Bringing It All Back: Police on the Homefront. Edited by National Action and Research on the Military-Industrial Complex. Philadelphia: American Friends Service Committee, 1971. Pp. ii+133. \$1.35 (paper).

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In the last few years, a number of radical research groups have helped us to understand better the distribution and exercise of power in America by revealing hidden relationships among corporations, universities, and the government. *They're Bringing It All Back: Police on the Homefront* is a collection of such revelations, focusing on rapidly developing ties between urban police departments, universities and research centers, a number of large corporations, federal law enforcement agencies, and the military. Results of this unholy alliance include the development of exotic weapons for crowd control and the transplantation of weapons and combat tech-

niques used in Vietnam to the homefront for domestic counterinsurgency; a major step-up in domestic intelligence operations, especially but not exclusively directed against black and student groups; "professionalization" of police departments and increased coordination of their efforts with the Justice Department, especially through federal funding or direction of projects carried out by local police, ranging from purchase of new equipment to the arrest of nearly 900 demonstrating black college students in Itta Benna, Mississippi; a substantial increase in federally financed research on crime causation and control and on ways to increase the efficiency of the criminal justice system; and training programs for police departments of American allies, as part of a worldwide Vietnamization program. The documentation is thorough and includes lists of government research grants, training programs for foreign police, and a list of colleges training local police forces. A supplement reprints documents stolen from FBI files in Media, Pennsylvania.

The documentation provided in this volume will be useful in combating the myth often used to defend research of this kind—that it is a neutral, value-free enterprise motivated mainly by the desire to better understand society or to solve social problems, help the poor, etc. Overwhelmingly, the projects funded are "applied" and reflect the needs of those who manage the system, not those it processes. Such themes as how to control riots; detect drug users; catch criminals, process them more efficiently, and control them while in custody, predominate. Only one of the 100 grants awarded in 1970 by the National Institute of Law Enforcement concerned the decriminalization of harmless activity; only one concerned business crime; no grants were awarded on ways to eliminate racial or class discrimination on the part of police, judges or prison officials; none on how to protect or expand civil liberties; none on how to change the social environment thought to be conducive to crime. The essays also answer the other myth—that it is good for the universities to train police because this will liberalize them.

An informative study like this inevitably raises questions beyond the study itself. What should be done? How to do it? Can the changes needed to make the police more responsive to human needs be made short of a social revolution? If not—and this is the inference of a number of the essays—how can that be brought about? There are of course no easy answers to these questions, but the information contained in the volume under review will prove helpful in thinking about them.

The Gift Relationship: From Human Blood to Social Policy. By Richard M. Titmuss. New York: Random House, 1971. Pp. 339. \$6.95.

A. H. Halsey

Nuffield College, Oxford

No American, as Nathan Glazer has said, can read this book without shame. This is not merely because of what everyone knows, that American

health services are expensive, unequally distributed in relation to need, imperfectly coordinated, and ineffective in terms of health standards. Nor is it only because they are inefficient (about 30% of all blood collected in the United States is wasted through technical or administrative inefficiency compared with 2% in England). It is because, and this is the essential burden of Richard Titmuss's attack, they reflect the structure of a society which is inhumane.

The argument, in direct descent from, and easily the most dramatic postwar expression of, R. H. Tawney's socialist critique of capitalist society, is focused on social arrangements for linking supply to demand in the case of blood. From his study Titmuss concludes "that the commercialization of blood and donor relationships represses the expression of altruism, erodes the sense of community, lowers scientific standards, limits both personal and professional freedoms, sanctions the making of profits in hospitals and clinical laboratories, legalizes hostility between doctor and patient, subjects critical areas of medicine to the laws of the marketplace, places immense social costs on those least able to bear them—the poor, the sick and the inept—increases the danger of unethical behaviour in various sectors of medical science and practice, and results in situations in which proportionately more blood is supplied by the poor, the unskilled, the unemployed, Negroes and other low income groups and categories of exploited human populations of high blood-yielders" (p. 245).

These are grave charges. Some sociologists may want to say that as sociologists they are not professionally interested in moralizing. They may expect instead that a review in the *AJS* would address itself to the merits, if any, of the nonevaluative theory and the validity of the empirical propositions which are advanced. For them, let it be said that Titmuss's survey of the attitudes and motives of blood donors in Britain is of high competence, and that the assembly of relevant data from the voluminous scattering of unevenly accurate statistics in the United States and other countries all over the world have been undertaken with minute care and caution. At this level there can be no quarrel with the standards of a superior craftsman.

At the level of sociological theory Titmuss is perhaps more vulnerable. Three criticisms seem to be possible. The first concerns the distinction which Titmuss draws from Marcel Mauss's *Essai sur le Don* (1924) between "economic exchanges" and "non-economic exchanges" where the former are part of self-interested trading and the latter symbolize the social bond between the participants. Not all modern anthropologists would accept this functional distinction. It is, however, crucial to Titmuss's analysis of the gift in advanced industrial societies, which he sees as a mode of integration between individuals and society and all the more altruistic because of the more remote and anonymous connection between giver and receiver in such societies. But whether or not Titmuss interprets Mauss, Lévi-Strauss, and Malinowski correctly concerning donor relationships in primitive societies is a relatively unimportant question compared with his own theory of a kind of Gresham's law of selfishness such that the

institution of the market undermines the social integration of individuals through the erosion of opportunities for the expression of altruism.

The second criticism, which is directly relevant to this last proposition, has been put forward by Nathan Glazer (in *Public Interest*, no. 24 [Summer 1971], pp. 86-94) as a criticism of Titmuss's use of the word "freedom." Glazer questions whether the disincentive to give freely actually operates in fact. He bases his doubt on a casual inquiry among Harvard students who had responded to an appeal for blood donations. It turned out that the fact that others might subsequently make a profit from processing and selling blood was not a consideration which was ever raised among the donors. He argues further that, though the rising proportion of bought blood in the United States might be held to demonstrate a decay in altruism through commercialization, it would still be questionable whether a restriction of freedom was involved. Glazer's point is well taken. Titmuss may have overdone the paradoxical rhetoric of a socialist attack on the freedom of the free market.

The third criticism is fundamental. It is that blood giving and blood markets may not serve adequately as comprehensive indices of all the social determinants of altruism and egotism in a society and may even be misleading. Titmuss has proceeded "from human blood to social policy" on the assumption that blood is the critical case. It is true that blood has traditionally magical overtones; but are these so strong in a modern scientific and secular society as they once were? And in any case are there not more critical tests, for example the proportion of GNP which is allocated to the social services or the degree of inequality of income distribution? Is the British and American comparison so stark on these alternative measures?

Nevertheless, though blood and the private market may not encompass the whole debate, we are still left with a magnificent study of the character of social integration in modern society which may be expected to stay at the center of discussion for years to come.

The Chippewas and Their Neighbors: A Study in Ethnohistory. By Harold Hickerson. New York: Holt, Rinehart & Winston, 1970. Pp. x+133. \$2.65 (paper).

Nancy Oestreich Lurie

University of Wisconsin—Milwaukee

Harold Hickerson demonstrates very effectively that documentary sources handled with rigorous methods of history and ethnology together can elucidate understanding of given topics and allow for the formulation and testing of theoretical propositions far beyond the reach of conventional historiography or ethnographic field research taken alone. This, in effect, is the definition of ethnohistory, Hickerson's basic concern, exemplified in

this book by Chippewa data. Hickerson provides critical documentation for particular problems that have broad theoretical significance in exploring certain aspects of cultural dynamics in general.

Opening with discussions of the nature and methods of ethnohistory, the Chippewa themselves, and historical approaches in cultural anthropology as a whole, Hickerson makes clear that it is not his objective to write a general ethnology or even ethnohistory of the Chippewa. Rather, he has chosen three, fairly discrete, problems regarding culture change among the Chippewa which could not be studied at firsthand in the field at this late date or even explored effectively through collection of oral tradition still available among the Chippewa (which, in any event, Hickerson tends to view with perhaps excessively disdainful suspicion).

The first case involves the assemblages of people designated "nations" in the earliest 17th-century accounts from the western Great Lakes. These groups are usually assumed by later writers to be no more than archaically named versions of the now familiar Chippewa "tribes" and "bands" of the fur trade, treaty cession, and reservation periods. Hickerson makes a convincing case, however, that the first chroniclers actually caught sight of precontact social organization of the Chippewa. His careful review of early nomenclature and other bits of data suggest the presence of single clan settlements with contiguous territories, "confederated" by common language, customs, and affinal kinship links based on clan exogamy. As a doctrinaire cultural evolutionist, Hickerson believes further study, especially in linguistics, will prove these were originally matrilineal clans, but preserves his always commendable impartial adherence to documented fact in discussing the Chippewa for what they were in historic times, that is, patrilineal. When the large clan villages broke up into smaller, more mobile, and territorially expanding bands in response to the demands of the fur trade, they split not along clan lines but across them as groups of families, each representing several to all of the original clans. Thus, they preserved common Chippewa identity and, it should be added, localized pools of eligible marriage partners from different clans despite the wide distribution of the relatively small Chippewa population over an area that ultimately stretched from Lake Huron to the northern plains.

Hickerson's second case concerns the Medicine Lodge or Midewiwin, a society of religious initiates that figures prominently in early, but not the earliest, accounts and is still a going concern among some of the more conservative Chippewa communities and other woodland tribes. Because of its ubiquity, the Midewiwin is widely assumed to be a survival from aboriginal times. Not so, says Hickerson, echoing and documenting more convincingly a generally neglected view first expressed by Paul Radin regarding his study of the Winnebago Medicine Lodge that it was induced by the effects of the fur trade (Paul Radin, *The Road of Life and Death* [New York: Alfred A. Knopf, Inc., 1945], pp. 50-51, 74). While the trade brought material enrichment of which the Chippewa availed themselves eagerly, it also resulted in profound repercussions in the aboriginal culture. The Medicine Lodge may thus have been the first and most successful of

a series of postcontact revitalization or nativistic movements to sweep the Great Lakes Indian peoples in times of crisis, providing supernatural rationale to reintegrate native culture and maintain sociocultural continuity in the face of irresistible or unavoidable innovations having disorganizing and destructive effects.

Finally, Hickerson expands upon a problem he has dealt with in earlier studies to which he refers, that the presumably ancient animosity between the Dakota and Chippewa was, in fact, recent and the result of the fur trade. In a concluding chapter to this argument, "Biome and Warfare . . .," Hickerson draws skillfully on data from the natural sciences regarding floral and faunal successions in historic times in the western Great Lakes region to demonstrate ecologically that there was utility in perennial warfare for both the Dakota and Chippewa. That the Indians understood such biosocial ramifications is clear from reports of their own statements made in resistance to white efforts to induce them to make peace and get down to the important business of trapping an area that could not be exploited for the profit of the fur traders because of the Indians' hostilities. This "debatable zone" ran in a northwesterly direction from the Chippewa River in Wisconsin through the headwaters of the Mississippi in Minnesota to the Red and Big Sioux Rivers. Unlike large sections of the areas occupied by the Chippewa and Dakota, this zone was ideally suited to the habits of the whitetail deer on which both groups of Indians depended heavily for food. By keeping the area constantly and mutually dangerous, the Indians discouraged traders (whose dealings were generally confined to one tribe or the other) from establishing posts that would attract Indians to settle in the vicinity and thus quickly deplete and drive off the supply of large game. "Where there was peace there was starvation. But ironically, success in war must also have meant starvation, for that would have involved Dakota or Chippewa, whichever was successful, overrunning the country at the expense of the other, hence having free use of the once-contested area, and, because of the resulting excessive demands on game, namely deer, exploiting it to depletion. There was, then, a *selective advantage in not winning*, but only through steady attrition maintaining a precarious balance through which the neutral zone could be maintained as a deer reserve to be tapped vicariously on either side, but not exhausted. . . . Better a steady little than nothing" (pp. 117-18).

All in all, Hickerson's book is a solid contribution to the anthropological literature in regard to gaining new value from old data, and as a highly useful teaching manual of ethnohistorical theory and method. We may hope that his example is an inspiration for other scholars, particularly younger people seeking untapped areas for research, to go and do likewise. In addition to the exciting sense of sleuthing that the often denigrated "library research" offers anthropologists, it affords moments of literary delight when one stumbles upon, for example, the ineffably original spelling of a Peter Pond or the pragmatic logic of a Little Crow who admits that war, in principle, is bad, but the occasional loss of a man on either side every year is better than wholesale starvation for all.

Frontier Folkways. By James G. Leyburn. New Haven, Conn.: Shoestring Press, 1971. Pp. x+291. \$9.25.

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Frontier Folkways is an unabridged, unedited version of Leyburn's doctoral dissertation, which was originally released in 1935. It is a study of the growth of new patterns of behavior on frontiers, of the modification of the original customs and institutions that colonial populations brought to this setting. The focus is on changes in the behavior of colonial peoples themselves and not on the effect of the frontier on native populations or the colonial power at home.

Leyburn classifies frontier societies on the basis of their basic exploitative unit and discusses cases relevant to each: small farms (the English in Massachusetts Bay, the French along the St. Lawrence, the English in New Zealand), plantations (the Portuguese in Brazil, English in Australia, and Boers of the Transvaal), exploitative plantations (the Dutch in Java and Spanish in America), and camps (American colonies on the Great Plains and in California). The societies that developed are compared with respect to economic behavior (division of labor, land tenure, standard of living), marital institutions (marriage, divorce, immorality), political behavior (nature of the institutions, legal authority for punishment), social structure (class, rights of women and children), "ethos" or character (temper of political life, foresight, quality of amusement and humor), religion, and demography.

One of the author's goals is to explain the similarities and differences among frontier societies. He closes with a series of "laws," some of which are assumptions and some of which are indeed testable hypotheses. Ecological variables and economic behavior and motives are primary, and the natural environment looms somewhat larger than contact with indigenous populations in explaining the development of new institutions. More generally, Leyburn employs a kind of synthetic evolutionary model of frontiers. Colonial populations are usually an atypical subset of the society from which they came, and the pace of diffusion and invention are accelerated by the frontier situation creating additional variety. New selective pressures determine that those few innovations that survive will give a new character to the society.

Beyond these touchstones, Leyburn's theorizing ranges from the synthetic to the eclectic. He allows that both conscious and unconscious responses to the frontier have had important effects on social evolution. Much of the best that evolutionary and acculturational approaches have to offer is combined. But he is less successful in dealing with psychological factors. Envy and greed are sometimes used as explanatory variables. And there are casual ventures into the most naïve of culture and personality analyses. Among the French in South Africa, "The vivacity, versatility, and dash which one associates with the French was entirely lost to Dutch

phlegm" (p. 106). Leyburn's view of the role of the social scientist is similarly out of date: "A social scientist never feels called upon to ask of an institution, Is it merciful and just? He asks rather, Did it work? or How did it work?" (p. 178).

That such problems should exist in a work written 35 years ago is understandable. That neither Leyburn nor social scientists as a group have solved the major ones is not. On a theoretical level, the many testable hypotheses about frontiers that Leyburn discusses are largely untested. Analytically, we have the social history and the comparative method. While the former is sociologically interesting, its techniques of verification are analytically unsatisfying. And the evidence that cross-cultural, cross-community, and even cross-sectional studies do not produce sequences that can be equated with actual patterns of temporal variation grows by the year.

The frontier literature, as Leyburn's work demonstrates, is a rich source of data on demographic, economic, organizational, and, sometimes, attitudinal change. Not infrequently, quantified or quantifiable data can be obtained. Neither Leyburn's problems nor those of today stem from the nature of the data. Rather, they stem from our approaches to them. At one point, the author observes that the variety in frontier situations represented in the book could have been replicated in a detailed consideration of one historical sequence, that in America. But the preference for spatial over temporal variation and comparison triumphs. And still today, diachronic data, when available, are forced into synchronic molds. Trend and other dynamic analyses are largely unused. Our ability to understand and explain patterns of temporal variation will continue to remain largely unrealized until we accept a fundamental imperative: it is necessary to observe and measure processes of change in order to understand and explain changes.

The Sociology of Occupations. By Elliott A. Krause. Boston: Little, Brown & Co., 1971. Pp. xvi+398. \$7.95.

Jeffrey M. Schevitz

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The Sociology of Occupations is a substantial contribution to a subfield of sociology that needs a great deal of improvement to become theoretically sophisticated, of use to anyone outside of government, business, and the union bureaucracy, and just plain interesting. Elliott Krause's writing has none of the turgid quality that marks the work of so many sociologists who go beyond description to analysis. He put sociological concepts to use, making them clarify rather than obscure general relationships among classes of phenomena.

The author takes a critical perspective throughout his book as he describes how occupations' histories, career patterns, practitioners, and functions are shaped by ruling classes and groups as well as by factors internal

to the occupations. His book indicates "the need for a combination of the field of the sociology of occupations and such fields as political sociology and the sociology of knowledge. A political sociology which does not treat the political action of occupational groups is risking irrelevance, whereas an apolitical sociology of occupations and professions misses what has *always* been a major dimension of these groups' actions" (p. 353).

Krause's analysis of occupations is limited because the book is tied together not by theory but by methodology divorced from theory. Nevertheless, his methodology has the virtue of allowing occupations to be examined from several approaches without making the book an eclectic potpourri of observations and insights. The book's first four chapters develop Krause's methodology: the historical approach to the development of occupations; the biographical approach which traces and compares career patterns within and between occupations; the functional analysis of occupations in terms of the societies of which they are a part; and a "conflict of interest" approach. The following 10 chapters apply the methodology to the analysis of specific occupational areas: law, health, religion, the military, business, science, art, education, and public service. Krause also applies his framework to activist and "illegal" occupations. The summaries provided at the end of each subtopic, topic, and chapter are superb and can profitably be read first to make the sections hang together even better for the reader.

Here is an author in search of Marx. Krause's substantive historical descriptions and summary generalizations could well start to fill the vessels of a sophisticated Marxian theory of American occupational structure, process, and struggle. A more self-consciously Marxian approach might have enabled Krause to overcome the sociological disease of methodological fragmentation. The result might have been a dialectical analysis of the intimate relationships of historical process, group struggle, and personal biography. Nevertheless, the author's feel for a Marxian approach prevents his methodological division of reality from overwhelming him. For instance, in his section on "historical perspective," Krause describes the "deprofessionalization" process during the Jacksonian era as a *struggle* of the working class against the ruling class—this despite his separate section on "conflicts of interest."

Krause's firm but critical command of the sociological literature on occupations and mobility enables him to use that literature to refine, not debunk, Marxian analysis. One result is a much more complex, subtle, and thorough analysis of the contradiction between occupational and class consciousness (pp. 85, 87) than that offered by Dahrendorf in his attempt to debunk Marx in favor of a pluralist model of occupational conflict. Another result emerges from Krause's clarification of the contradiction between individual and group mobility (p. 71). This contradiction underlies the legitimating function of those sociologists who measure a group's mobility by measuring individual mobility.

The chapters on individual occupations clearly reveal that Krause's mission is clarification, not debunking or refutation. For instance, he

repeatedly shows in detail how ideology reflects and legitimizes the power of the dominant class. The occupational ideologies of groups that are a part of the ruling class or whose functions resonate with the needs of that class are more likely to be accepted than those of other groups. Krause does not invoke a vulgar Marxian analysis (which is most often a straw-man position put forward by an anti-Marxian) to explain this phenomenon, but points out that a group's acceptability and support reflect the group's functional importance to the system being examined. In a capitalist system, groups functionally necessary for the successful operation of the system will be made more acceptable by the ruling class-dominated government (which officially recognizes occupational ideologies by delegating control of training, entrance, and practice to the occupation) and mass media.

Krause's consideration of "occupational altruism" not only points out the relationship between the status, material advantages, and autonomy of an occupation's practitioners and its altruism. It also points out that an occupation's altruism generally ends where challenges to its status, material advantages, and autonomy begin. "Occupational altruism, without a major change in the overall economic-political system, may be more a luxury for the haves than a solution for the have-nots" (pp. 101-2). In many places, Krause notes that co-optation and repression prevent occupational altruism from developing into occupational radicalism. There just "weren't [*sic*] any jobs in the system for those who wanted to be paid for opposing it" (p. 354).

Bureaucracy and Participation: Political Cultures in Four Wisconsin Cities. By Robert R. Alford with the collaboration of Harry M. Scoble. Chicago: Rand McNally & Co., 1969. Pp. xvi+244. \$6.50.

Patrick Molloy

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One might ask if four cases constitute an adequate base for a comparative study of urban politics. The merits of *Four Cities* and *The Rulers and the Ruled* could be used in justifying such a strategy. And James Q. Wilson has argued that intensive analysis of a small number of cities enables one to escape many of the measurement problems plaguing more ambitious comparative studies. Present methods of data analysis, in this view, are exploited to proper advantage when the researcher becomes acquainted with the style, ethos, or culture of a few local governments. Robert Alford does not repeat that argument here, although his study is definitely relevant to the continuing discussion. Neither is his work very greatly concerned with the advantages of various numbers of cases.

The Wisconsin cities Alford reports on will nevertheless become familiar to social scientists when this and related reports based upon the same data work their way into several fields of substantive interest. The four cities—Green Bay, Kenosha, Racine, and Madison—ranged in population from

63,000 to 127,000 in 1960. Each used the mayor-council form of government with nonpartisan election by wards. The economic base, to which Alford attributes great weight, differentiates the cities. Green Bay is classified as a traditional trading center; Kenosha and Racine as manufacturing centers; and Madison, the home of the state capital and state university, as a modern professional center. The author provides brief histories of each city, concentrating upon the 1945-62 period, and also devotes attention to such topics as land use and social ecology, business and labor problems, political parties, boards of education, and the community role of businessmen. In some cases, additional topics and events unique to a city are discussed. Although largely descriptive, the chapters on the individual cities are consistent with the central hypothesis developed in the introductory and more theoretical chapters: "The largest and most economically diverse professional center, Madison, should exhibit the highest levels of bureaucratization and participation. The two manufacturing centers, Racine and Kenosha, should fall in the middle" (p. 16).

The first two chapters touch upon numerous topics of interest to sociologists. In broad and direct fashion they stress dominant themes in the study of bureaucratization, professionalization, political participation, and urban government. While the treatment of these matters is very abbreviated, Alford indicates biases or areas of neglect in existing approaches which he proposes to remedy by a more comprehensive consideration of structural and cultural features of communities. Many will appreciate the injection of central sociological concerns into the study of urban politics. Occasionally the attempt to attain conceptual precision leads to less than illuminating clarifications such as, "The persistence of relationships, of course, though not absolutely determinative, is closely correlated to a low probability that actors could change the relationship even if they mobilized all of their resources" (p. 4). But these instances are rare and the reader is far more often rewarded by the introduction of notions like structural conduciveness or the treatment of culture advocated by Bendix and Berger. Not fully developed and too often merely favorably juxtaposed to other traditions of analysis, their presence nevertheless suggests that the author is attempting to enrich the fare of those accustomed only to learning who governs in yet another set of cities. Alford has elaborated upon these subjects in other articles and, as he explicitly mentions, only a few themes could be covered in the empirical chapters.

Even with the narrowing of questions in later chapters Alford cautions the reader about the indirectness of his data and often reminds him that many questions one would like answered are in fact difficult to answer with the present material. One is left wondering, for example, whether "Madison residents are either really more active or [whether] they feel that being active in a party is 'the thing to do' in their community" (p. 133). But in addition to an extensive survey of leaders and voters, the author draws upon public records, unpublished material, and to some extent personal observations, to support his argument.

Madison consistently reveals higher, and Green Bay lower, rates of bureaucratization (or professionalization) and participation than Kenosha and Racine. In one of the few comparisons in which Madison and Green Bay prove similar Alford points out that the correlation of occupation with party identification is low in both cities. Trade union activity in Racine and Kenosha is probably responsible for the higher correlations found there. In other matters, however, Alford finds it difficult to isolate any distinctive political culture to be attributed to the city itself and not due to the socioeconomic composition of the city. Different types of data would be required to answer the question satisfactorily, but it appears at least plausible to the author that public norms of the cities are an important influence in the process of selective recruitment of leaders.

What Alford labels the traditional features of Green Bay and the modern features of Madison seem to form coherent systems. He is sensitive to change in those systems, and he continually points to the potential role of political leadership in modifying the economic base of a city. Yet there is a static aspect to his analysis. In previous periods there seem to have been no changes in the overall ranking of the cities. Kenosha did adopt a "modern" council-manager plan during the 1920s. But this was finally replaced in 1958 by the mayor-council plan, and almost inevitably so in the view of the author, for he views this plan as more consistent with and appropriate to the local political culture.

The distinction between traditional and modern elements occasionally becomes a troublesome one to maintain. "Clearly none of these cities may be expected to conform to the extreme type of modern or traditional system, precisely because they are all fairly bureaucratized cities within a modern society and polity" (p. 204). But other comparative analysts, such as Samuel Huntington, would argue that the very existence of powerful local governments is quite traditional, and Alford himself recognizes "the historic commitment of the American political system to the principle of local autonomy and the preeminent role of the local electorate or its direct representatives in many policy areas which are removed from popular control in other democratic countries such as Britain" (p. 141). Perhaps it is significant that Daniel Elazar, a political scientist concerned with concrete problems of federalism in his *Cities of the Prairie*, found it desirable to develop a more historically grounded notion of traditional political culture, one which emphasizes the heritage of plantation agrarianism characteristic of the South. The mere differentiation of demand and decision-making units, which Alford takes to be the basis for deciding whether a city is modern or traditional, again appears to be too ahistorical and inflexible a criterion, especially since a wide range of policy issues are not framed with those concerns in mind.

The result is that too often Racine and Kenosha become indistinguishable in the intermediate category, and the main comparison involves Madison and Green Bay. And in the matter of innovative public policy it becomes impossible to decide why Kenosha and Racine are not more

• innovative than Green Bay when the former two cities by most measures must be considered more modern. What promises to be a study of four cities frequently becomes a study of two or three cities.

Alford's observations about the shortcomings of existing approaches, his adept presentation of a diversified set of data on leader and mass orientations, and his effort to separate analytical elements of urban politics too often confused makes this an important source for students in a number of fields. The author advises the reader when evidence is too meager to support a proposition adequately, and he is straightforward about the merits and defects of alternative methodological strategies. But for those interested in effective community action the constraints and resources provided by local political cultures will remain barely understood until more adequate notions replace the rather abstract versions of tradition and modernity. The systematic analysis of larger numbers of medium-sized cities will also gain in theoretical strength when the concept of modernity is less closely identified with bureaucratization and participation.

Automation and Alienation: A Study of Office and Factory Workers. By Jon M. Shepard. Cambridge, Mass.: M.I.T. Press, 1971. Pp. 163. \$7.95.

Automation and Behavior: A Social Psychological Study. By John Chadwick-Jones. New York: John Wiley & Sons, 1970. Pp. xi+168. \$8.95.

Ely Chinoy

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Both of these books deal with the same general problem, the impact of technological change upon workers, but they differ markedly in their focus, their methods, and the kinds of conclusions at which they arrive. Neither, unfortunately, adds much to our knowledge.

Jon Shepard's study tries to test a general hypothesis about the relationship between technology and the level of alienation to be found among workers in both industry and the office. He begins with the assumption that "there is a curvilinear relationship between the phases in the man-machine relationship and the degree of differentiation in the division of labor" (p. 9). With greater mechanization the division of labor increases, but with the introduction of automation the division of labor diminishes. Since alienation is presumably related to the division of labor, there should then also be a "curvilinear relationship" between the development of technology and the level of alienation.

The data used to test the hypothesis are drawn from interviews with three groups of manual workers and from questionnaires based on the interview schedule administered to several groups of white-collar workers. The three groups of manual workers were craftsmen (nonmechanized) and assembly-line workers (mechanized) in an automobile plant and operators in an automated oil refinery. The white-collar employees were clerks (non-

mechanized), office-machine operators such as typists or key-punchers (mechanized), and computer programmers, operators, and systems analysts (automated) in several insurance companies and a bank.

Alienation was defined, following Melvin Seeman, in terms of powerlessness, meaninglessness, normlessness, an instrumental work orientation, and a low level of personal involvement in one's work. Scales for each of these dimensions of alienation were then constructed and the various groups compared with one another with respect to the proportion of each whose alienation scores were above the median score for the whole sample.

The findings consistently support the hypothesis that the study was designed to test. On each of the dimensions of alienation the proportion of respondents whose scores were above the median was lower, with few exceptions, for both the nonmechanized workers (craftsmen and office clerks) and for those monitoring or operating automated technology in factory and office than for mechanized blue-collar and white-collar workers.

In confining his analysis to the test of one hypothesis, Shepard ignores important questions suggested by his own data. Why, for example, do only 19% of the craftsmen rank above the median with respect to powerlessness while almost half rank above the median on the other dimensions of alienation? Why, in fact, do almost half of the craftsmen score high on four of the five alienation variables? Similarly, why do almost half of the nonmechanized white-collar workers score above the median on normlessness and on lack of self-evaluative involvement and more than half above the median on powerlessness, meaninglessness, and instrumental work orientation? Such data suggest that however important technology may be in affecting workers' responses to their work, other variables may be equally or even more important sources of alienation.

One cannot, of course, fault a researcher for not doing what he did not set out to do, unless what is left out might affect the results. As Shepard himself notes, "factors affecting attitudes toward work are too many and their interrelationships too complex, to assume that the technologically determined attributes of the job alone constitute an explanatory variable" (p. 126). Yet, except for sex and size of organization, the latter dealt with very briefly, there is virtually no reference in the book to other variables, some of which might not only have an independent impact on work experience but might also affect how workers respond to technology itself. Among these variables are basic institutional and cultural factors whose neglect may stem from the fact that the concept of alienation, which originally referred to a relationship between man and his society and culture, is reduced, following much current practice, to a set of social psychological categories.

Automation and Behavior deals with the impact of automation upon a group of steelworkers. It is concerned with such detailed questions as individual adjustments to changes in the job, levels of satisfaction and dissatisfaction, social relationships on the job, the role of supervision and responses to it, and changes in management. The data are based upon observations made in 1956-57, before automation was introduced, and in

1962-63, after the new technology was in operation, with follow-up interviews that continued until 1966.

There is little in the research that adds or elaborates upon the findings reported in two earlier studies of automation in the steel industry—Charles R. Walker's *Toward the Automatic Factory*, and *Technical Change and Industrial Relations*, by William Scott and his collaborators. Chadwick-Jones's general conclusions, that workers should be prepared for change, that job enlargement would increase workers' interest in their jobs (a conclusion Shepard also arrives at), that social behavior on the job be taken into account in "forward planning," are hardly new, and the detailed data are given in such a pedestrian fashion that one can only plod doggedly through the book looking for the all-too-few nuggets.

Social Fragmentation and Political Hostility: An Austrian Case Study. By G. Bingham Powell, Jr. Stanford, Calif.: Stanford University Press, 1970. Pp. 201. \$7.50.

Dwaine Marvick

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This is a case study of political life in a small industrial Austrian city. In early 1967, the author carried out a sample survey of 200-plus citizens in Hallein. From his base in nearby Salzburg both before and after his survey, he visited the city, talked with party and municipal officials, studied budgetary data, and browsed through newspaper files. From his survey, data were obtained about Hallein citizens: their partisan sentiments, issue orientations, group memberships, social-cleavage positions, and the sense of political hostility and distrust manifested by SPO and OVP adherents toward one another—the so-called *lagerdenken* mentality which others have seen as pronounced in postwar Austria. Much of his text seeks with these data to clarify how "social fragmentation" and "political hostility" affect one another, not only in the way citizens view their community but also in the performance of local political elites. A rather useful analysis of the municipal budget is made, highlighting the ways in which structural constraints affect the locality's "system capabilities." Patterns of city-council and party decision making are described, as exemplified in a small number of political struggles that arose in the 1960s, but his examination of elite cleavage lines does not rest upon any hard evidence. The study is not rich in contextual details; it is neither historical nor developmental in its handling of materials; the data base used is static, thin, and disappointing.

Can a more favorable judgment rest on its theoretical contribution? The basic theory of fragmentation is said to be a flourishing body of knowledge, but "more accurate distinctions and more precise research are necessary before the theory of fragmentation can become fully useful" (p. 5). Only cursory discussion is made, however, either of the theory

itself or of the proposed augmentations thereto. Fragmentation theory appears to be an alias for pluralist theory, which is virtually equated with cross-cutting memberships and their benign effects. By contrast, fragmentation theory focuses on cumulative group memberships and their malign political consequences—the spiraling and destructive forces that can tear a community apart when its subcultures nourish feelings of in-group isolation and when intergroup antagonisms are projected into politics and even exacerbated by the political rivals who champion them.

Mr. Powell's strategy is to use theory to guide his exposition of the single case and to search for unanticipated features of the specific case to "inform" the development of the basic theory. By presenting "basic theory" in its "simplified version," complications do emerge requiring more sophisticated reformulations of that theory. It is difficult, of course, to find authors who can be quoted as sources for the "simplified version," so none are. Instead, in the first discussion, the sequence runs like this. Specific points actually made by Dahl and Verba are briefly examined. Next, reference is made to a line of argument that challenges "pluralist theory on other levels as well" (p. 4) by Converse and Lijphart. In 150 words, four working hypotheses are set forth for discussion. To round out the search for more incisive theory, it is then noted that social fragmentation probably will affect political conflict differentially, depending upon such undefined conditions as subculture awareness, political intensity, social segmentation, cumulative group affiliations, mass hostility, elite perceptions of mass attitudes, system instability, and system malperformance. None of these intervening factors is seriously examined, then or later, either theoretically or empirically, to clarify how they might qualify the link between fragmentation and hostility. Later excursions onto theoretical ice are similarly hard to pin down.

The poverty of his theoretical effort is as distressing as the flimsiness of his data base. Hostility—his key variable—is measured by two questions: distrust of the rival party should it come to power nationally, and/or aversion to cross-party marriages involving the respondent's family. Applied to Hallein, the dangerous implications of *lagerdenken* are difficult to trace. Even for partisans locked into a Socialist or a Catholic subcultural matrix of cumulative group isolation, only one-fourth worried about the rival party coming to power nationally. Fewer still were distressed at what might happen to their sons or daughters. In the sample as a whole, about one-eighth could be scored as politically hostile—at least on the functionally relevant plane of running the national government, where such a level might seem surprisingly low, given the rhetoric of modern politics. Still, partisans under cross-pressures were less likely than those under cumulative pressures to register hostility. With findings like these, since the author is able to assert that they "unambiguously confirm the hypothesis," who needs evidence (p. 39)? Six pages later, after failing to show that either class or religion alters the hostility levels among Hallein citizens, he argues that "these findings provide increased contemporary relevance for Marx's predictions of class conflict."

His closing suggestion is an understatement. "The nature of political hostility itself . . . should be subjected to a careful analysis, using more elaborate and more numerous attitudinal indicators than were used here" (p. 143).

The American Mafia: Genesis of a Legend. By Joseph L. Albini. New York: Appleton-Century-Crofts, Inc., 1971. Pp. xi+354. \$3.95 (paper).

Donald R. Cressey

University of California, Santa Barbara

Unfortunately, it usually takes as much time and energy to write a poor book as to write a good one. The lavish use of footnotes in this book suggests that Joseph L. Albini expended much time and energy on its preparation. He manages, in one way or another, to work in citations of most American books and articles on organized crime, including fast-buck pieces dashed off by free-lance writers. Further, a chapter on "The Mafia and the Camorra" parades familiarity with a portion of the some 200 books written about the Sicilian Mafia. Albini even reviews what some writers of undergraduate criminology textbooks have said about varieties of organized crime. (He misses the very insightful comments made by one such textbook writer—Walter C. Reckless.)

There is evidence, then, that Albini did not rush to press in an attempt to capitalize on the current popular interest in "Mafia books." Nevertheless, he has produced a bad book. It is pedantic without being scholarly. It is not addressed to a general audience, but neither is it addressed to any discernible specialized one. It picks nits, but it also overgeneralizes. Its style clearly suggests that Albini does not know how to write, that the publisher's copyeditors gave the author no help at all, and that the editors of the publisher's Sociology Series (John F. Cuber and Alfred C. Clarke) asked for no manuscript beyond a rough draft.

Albini's basic objective seems to be that of knocking down something of a straw man, what he dubs the "*Evolutional-Centralizational Theory*" of crime syndicates. (He always puts it in upper and lowercase and italics.) At various points he says that this theory pictures syndicate structure in the United States as "highly organized," "rigidly organized," and "rigidly bureaucratized"; as a "nationwide unified criminal organization," and a "centrally directed organization"; as a "secret society," a "tightly knit society," a "centrally organized secret society," and a "national secret society"; and as an organization with "rigidly defined positions which are consistent among the national units, be they 'families,' 'cliques' or what have you."

Against such "rigid explanation" (p. 324) Albini sets what he regards as his contribution, namely a "*Developmental-Associational* approach." This device makes it possible to report a lot of what I can only characterize as yesterday's news. He says there are indeed "syndicates" that

have sold, are selling, and will sell demanded illegal goods and services in the United States; that these syndicates are "systems," "networks," and "a loosely formed system of relationships"; that they have "structure" and "function"; and that as "systems of power" they include "syndicate functionaries." The approach also enables Albini to say, again in very loose language, what others have earlier said more precisely. He announces that there is a "multitude of possible types of combinations of relationships among these syndicate participants," some of whom occupy "a middle position in a hierarchy" (p. 235), while others are "specialists" (p. 268), others are "individuals who have achieved the status of patron" (p. 316), and still others are "clients." Most important, Albini keeps repeating, is the observation, once again, that "syndicate participants" or "syndicate functionaries" engage in "patron-client relationships." He also seems to believe that the *Developmental-Associational* conceptualization" produced his commonsense observation that a "patron" dealing with a "client" in, say, Chicago might have no clients at all in, say, Los Angeles; his mundane conclusion that an important criminal might be a "patron" here (or today) and a "client" there (or tomorrow); and his deduction, known even to the sleepy students, that an organization selling lottery tickets in Chicago must have a different division of labor than an organization smuggling narcotics from Turkey to New York.

Albini could have made an important contribution both to sociology and to a solution of "the organized crime problem" if he had given some attention to the sociologists' distinction between "organization" and "*an* organization," and then in some insightful way had spelled out the implications of this distinction for analysis of criminal conduct. Such analysis is difficult because any discussion of *a* syndicate, of *La Cosa Nostra*, or of *the* Military-Industrial Complex necessarily implies that each of these is *an* organization. Complete abandonment of the articles "a," "the," and so on, would lead to utter confusion and misunderstanding. Retaining them also leads to misunderstandings, especially when doing so is interpreted as evidence of belief in "rigid bureaucracy." Yet systematic manipulation of the articles—as in sociological discussions of "society" and "a society"—seems essential both to understanding criminal organization and to understanding organizations of criminals.

Albini leans toward analysis of organization rather than of an organization or organizations. But, apparently because he was unaware of the distinction, he is unable to say anything new or important about either phenomenon. He is mad at authors who assign names, especially Italian ones, to American crime syndicates, to the nationwide alliances between them (most evident in the "lay-off" betting procedure), and to the status positions, authority positions, and occupationally specialized positions making up the syndicates. This hostility might be justified if he had first demonstrated that there is organization but not organizations, or if he had shown that syndication can exist outside the structure of a syndicate. Missing his golden opportunity, he merely discusses the same syndicates, alliances, positions, and roles discussed by the persons he criticizes.

More specifically, Albinì is quite indignant about authors who have found it reasonable to give the name "capo" or "lieutenant" to certain authority positions in each American syndicate, even if some of the persons occupying these "middle positions" do not call themselves "capo" or "lieutenant" and are given some other title by their associates. He could have documented any variation and then argued that it suggests organization or organizations rather than an organization. Instead, he joins the persons he regards as dumbbells. On page 316 he presents, in the "an organization" framework, a commonsense explanation of why there should be variation in the titles assigned to men who have achieved "the status of patron" both in Sicily and "among Italian-Sicilian syndicate functionaries located in various parts of the United States."

Perhaps Albinì failed to follow up what appears to be his own hunches about organization because he got carried away by his obvious desire to be the first to speak the truth about crime syndicates. Perhaps it was this "set" that also made it impossible for him to try to build on, to confirm, or to prove erroneous any of the many hypotheses about such syndicates. And there are ways of checking some of these hypotheses. For example, thousands of pages of bugged or wiretapped conversations between "syndicate functionaries" are now as available to sociologists as they are to newsmen, having been released to the public in connection with a New Jersey trial. Albinì does not even refer to these documents, let alone analyze them. Instead, he erroneously claims that some similar and earlier documents contain only FBI summaries of criminals' conversations and comments. These Rhode Island documents, and the more recent ones, do contain FBI summaries. But both sets of documents also contain verbatim transcripts of conversations and comments—valuable data that are going begging, so far as sociological analysis is concerned.

Albinì claims to have had good informants among both policemen and criminals, but he does not in a systematic way use any information acquired from them. Some of his allegations about crime syndicates are, to turn his own words on him, "so unfounded that they are not worthy of mention" (p. 246). His approach often involves merely fuzzing up the issue. On one issue, for example, he states a null hypothesis, although he does not call it that: "An examination of those who attended the Apalachin meeting hardly seems to indicate a meeting of a national organization." One would think that he would then present an "examination," thus testing the hypothesis. Instead of doing so, he changes the subject. After reviewing two sources that are secondary (or even more remote), he seems to accept as fact that the meeting was attended by at least one man each from New York, New Jersey, Pennsylvania, Texas, Colorado, Massachusetts, Ohio, California, Florida, Arizona, and Illinois. He concludes, "We find the bulk of those attending coming from the New York-New Jersey area rather than being a *representative* national group" (p. 249; the italics are mine). To be "national" must a group be "representative"?

The book contains an excellent 18-page bibliography.

Beyond Black or White: An Alternate to America. Edited by Vernon J. Dixon and Badi G. Foster. Boston: Little, Brown & Co., 1971. Pp. ix+140. \$2.95 (paper).

William H. Turner

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If black and white Americans are to find meaningful and workable alternatives to the present racial crisis, we must surely be careful not to base our change alternatives and strategies on the same racist philosophies that led to that crisis. Such is the thesis of this provocative little reader by Vernon J. Dixon and Badi G. Foster. *Beyond Black or White* is a refreshing departure from the empty rhetoric and speculation of the *Soul on Ice* and *Dark Ghetto* days, and it reflects a serious intellectual confidence which black scholar/activists might emulate. In gist, this talented assemblage of black scholars/activists from Princeton attempt to answer Dr. King's searing question: "Where do we go from here?"

The theoretical and problematic thread holding the book together is the contributors' view that the existing material on the racial crisis in America is mainly descriptive; and that in those few cases where strategies for change are offered, the thinking has been couched in the same spirit and philosophy which led to the present crisis. Essentially, the book is a coherent essay, and the intensity of the analyses reflects the contributors' ontological synchronism; that is, no polemics or theoretical clairvoyance—they all agreed from the beginning paragraph, and each demonstrated how Americans should best accept cultural diversity—beyond black or white!

The theme is cultural "unity" in spite (and because) of cultural "diversity" (italics mine; p. 4). The development of that position is organized under seven major headings which range from "Toward a Definition of Black Referents" to "The Dual Aesthetics of Black American Artists" and "An Alternate America." The overall frame of reference includes: (1) that black Americans, although embodying some cultural traits similar to those of whites, collectively embody a reference qualitatively different from that of white Americans; (2) racial conflict accrues from Americans' either-or conceptual approach to race relations (a black "gain" is perceived as a white "loss"); (3) black artists are subject to the influences of black and white Western culture; (4) economic institutions in America reflect the cultural foci of white (and not black) culture; (5) that restating the assumptions of American racism may thereby lead to a reformulation of strategies to combat that racism—black liberation; and (6) the "Diunital Approach" to an alternate America—an America where whites and blacks will acknowledge each other's differences and strive for the unity of their separate experiences.

Dixon's "Two Approaches to Black-White Relations" is, in my opinion, the most valuable single entry in the book. He reviews the literature very

well and offers the behavioral scientist a rather comprehensive model which he calls diunital—the state of being an element and not that element at the same time. Operationally, “We are at once both Black and not Black, we are this contradiction harmoniously” (p. 27). Dixon offers to students (who may need such clarification) an easy-to-follow typology on black organizations and includes a lively discussion on Hegel and Marx; and the progression to Fanon was welcome indeed. Dixon’s other solo, “Diunital Approach to Economics,” seemed to beg the question, however.

The major contribution of the book was Cross’s “Psychology of Black Liberation.” It was an academic snapshot of the nuances of the black psyche and the processes of “becoming” black. The book’s antistrengths are its overreliance on unnecessary scholasticism, and it placed a disproportionate responsibility for the reality on “An Alternate America” on black people. Then too, it was a plug for cultural pluralism—without the attendant racist innuendos, of course. It was too short to be convincing, since it claimed to be an answer; yet I am sure the authors left acceptance or rejection of their thesis to the reader.

These brothers did not worry about value-free social science, and their book needs to be read religiously by graduate students and scholars in race relations seminars. For, all too often in those exercises, students are exposed only to questions and not to any answers. Moreover, the book shows clear and keen insight into the “black thing” as well as its understanding of Western white culture. Right on to Foster, Dixon, et al., they did us all a great service here. Such a work as theirs is invariably used as an auxiliary to “established” readers; it might be well that traditional works serve as amplifiers to this fine little work. I shall certainly use it in my own classes at this black institution.

Colonialism in Africa, 1870–1960. Vol. 3: Profiles of Change: African Society and Colonial Rule. Edited by Victor Turner. New York: Cambridge University Press, 1971. Pp. viii+455. \$17.50.

Ralph A. Austen

University of Chicago

The task demanded of the 12 contributors to this book was truly herculean: to combine the small-scale field experience and social theory of anthropology with the written documentation and diachronic sense of historical research. It is not surprising that few have achieved their objective. The greatest success seems to have come to those six authors whose essays conform to the apparent meaning of the volume title; they are “profiles” of particular African societies undergoing modern change. The most thoroughly worked out of these is Ronald Cohen’s study of Bornu (Nigeria), which is mostly about precolonial history but develops a very full sense of the changes brought by European rule. The remaining

six contributions take up various general sociological themes, all relating to 20th-century European-African confrontations, if not necessarily using common definitions of colonialism. In some of this latter group the level of both theoretical bite and empirical substance leaves a good deal to be desired.

To deduce any general argument from the collection as a whole would be, as Turner himself concludes in the modest editor's introduction, "a fruitless project" (p. 4). The well-worn question of local political authority under colonial rule is treated in Cohen's chapter, as well as in those of John Middleton on the Lugbara (Uganda), Martin Klein on Sine-Saloum (Senegal), and Peter Rigby on the Gogo (Tanzania); surprisingly, it is largely ignored by the general essays. The authors writing on traditional authority agree that the "chiefs" of colonial Africa represented a break with previous political patterns. The degree of perceived change depended less upon European colonial policy (always cautiously interventionist) than upon traditional expectations. Bornu, a more resilient and highly centralized polity, experienced more continuity than the small-scale Sine-Saloum states or the totally acephalous Lugbara and Gogo. These conclusions would tend, indirectly, to support the provocative assertions in Max Gluckman's chapter that tropical Africa became, even after independence, more "pluralistic" than the Republic of South Africa. Pluralism is redefined by Gluckman to mean lack of political and social articulation, which he claims does exist in the oppressive South African situation largely as a function of widespread economic development.

Several essays in this volume treat colonialism in terms of "race relations," an approach which I have generally found sterile. Hilda Kuper, however, offers a precise and extremely useful account of the contrast between European and Swazi (Southern Africa) categories for perceiving alien peoples. Michael Banton, in "Urbanization and the Colour Line," presents some interesting descriptions and comparisons but offers them in a theoretical framework which seems either amorphous or tautological.

"The Impact of Imperialism upon Urban Development in Africa" is treated by Aiden Southall in a manner which sometimes approaches the polemical. Even where presented more detachedly, his views of how traditional African "cities" did or did not survive the European onslaught rests upon rather limited foundations both in urban sociology and African history.

Elizabeth Colson's contribution very neatly delineates how various European misconceptions of African land rights derived from the economic demands of paternalistic colonial situations. The essay would have benefited, however, from greater specificity in the references to "Earth Priests" and transitions to market agriculture.

Education and ideological change are the themes of chapters by Margaret Read on the Ngoni (Malawi), F. B. Welbourn on responses to missionaries, and Lucy Mair on new elites. Read's well-documented study is the most impressive of this group, although her optimism about Ngoni integration of traditional and European values seems at times a bit naïve.

Welbourn wanders over a wide range of varyingly convincing assertions which fail to do justice to his excellent original research on East African religion. Mair's essay covers the same ground as several more interesting pieces in the previous volume of this same series.

Language and Poverty: Perspectives on a Theme. Edited by Frederick Williams. Institute for Research on Poverty Monograph Series. Chicago: Markham Publishing Co., 1970. Pp. xii+459. \$8.95.

Lloyd B. Anderson

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Language and Poverty explores the consequences of linguistic and cultural plurality for early childhood education. Current professional thinking has grown out of recent social changes and a new understanding of sociolinguistics (Roger Shuy, chap. 16) which, in turn is now helping to clarify some mechanisms of social injustice. But too often these authors do not carry their reasoning to its direct policy implications—they strongly resist going against established educational practice. The book is excellent as a basis for rethinking among teachers and related professionals, especially if discussion can go beyond the ideas in the book itself. It is also good as a case study of social bias and professional responsibility. The 19 chapters are well written, present various views on each topic, and include many references for further reading. The book concludes with an annotated bibliography of journal articles and a short topical index. There is one chapter each on American Indians (by Lynn Osborn) and on Spanish-speaking Americans (by Vera John and Vivian Horner), but the main emphasis is on black children.

The central thesis is that, while the United States has children from a variety of cultural and language backgrounds, its schools have not adjusted to this fact. Evaluation and testing of children's abilities has been heavily biased by preference for middle-class norms of response. The real abilities of children with other norms of response are ignored in the usual curriculum, and the lower expectations for these children by teachers and the children themselves become *self-fulfilling prophecies*: the child's self-confidence and motivation are affected and this *causes* failure. These ideas are best presented by Williams (chap. 1), Joan Baratz (chap. 2), William

Labov (chap. 9), Courtney Cazden (chap. 5), Ellis Olm (chap. 11, pp. 223–26), Davenport Plumer (chap. 14), and Williams (chap. 18). These chapters should be read first, as a foundation for the rest of the book.

Baratz presents the clearest example of test bias: on a special language repetition test designed for speakers of black English (BE), speakers of middle-class English (MCE) did at least as badly as BE speakers did on a MCE test (MCE is the reviewers' term). Standard tests, then, do not show that the typical black child has less language ability. He is not "deficient," "deprived," or "disadvantaged" in this regard. Instead, he knows a different, but equally "grammatical" (= regular) form of language, a dialect of English. The most flagrant form of this bias is in the Bereiter-Engelmann program (see Siegfried Engelmann, chap. 6), used notably in Head Start programs; if children do not respond with the exact words intended, they are assumed to have a language deficit and an impaired concept-forming ability! Other measures of scholastic ability (even IQ) are regularly misjudged in this way. "When measures not dependent on knowledge of the English language have been utilized, the Indian child has been found to perform within the national norms" (Osborn, p. 241).

More subtle bias arises from different social expectations in interview situations (Labov, pp. 157–63): the black child has learned to say as little as possible to be safe, especially when the questions are stupid because the answers are obvious. He does not interpret a question as a request for verbal display, as the middle-class child does; he communicates only necessary information and is then judged linguistically deficient. A reasonable conclusion would be that standardized testing methods cannot be used fairly across social and cultural cleavages (see Williams, p. 393). This point, however, is not really considered by Roger Severson and Kristin Guest (chap. 15, "Toward the Standardized Assessment of the Language of Disadvantaged Children").

Discounting myths based on past testing, David Yoder (chap. 19) admits that "the poverty child does not seem to be subject to more than the usual incidence of speech and hearing disorders found in the general population," but he goes on, "Although [this] would influence us against [a] 'speech correction' campaign with the poor, . . . it may not be safe to consider the issue closed, and we should wait for the results of such surveys as . . ." (p. 412). So presumably "speech correction" campaigns can proceed for a time, the children may be assumed guilty of deficit until proven otherwise, and neither Yoder nor Severson and Guest have adequately warned speech therapists and others that they may be responsible for damage to children. The USDHEW is far ahead of them when it has tried to restrict use of tests for ability-grouping of students.

Reading problems may result from linguistic and cultural differences (chapters by Baratz, Plumer, and Doris Entwisle): black children are being asked to read in a form of language other than their first dialect, and so of course display the symptoms of "poor readers." And not only do the phonetic forms and grammar differ: "In kindergarten and first

grade, word meanings, and therefore the cognitive role of words, overlap little for the black and the white child" (Entwisle, pp. 127, 136). So "cues that are presumed to exist, and hopefully aid the teaching of reading and other language skills, may be much more visible to some children than to others." Equality of opportunity requires *at least* teachers who are aware of these differences and can adapt their teaching to what the child brings (see Basil Bernstein, p. 57). Marion Blank (chap. 4) warns that despite our stated ideal of accepting differences among children, we tend to assume that all will learn in the same way, and that differential teaching is anti-democratic and implies illness on the part of those treated differently (pp. 72-73). Instructional materials must thus be translated culturally. This is a bare beginning.

Must they also be translated linguistically? One major obstacle is black parents who feel that the use of black-dialect texts will deny their children mastery of standard English. In clear foreign-language situations, teaching is most effective if it begins in the vernacular rather than in the standard. Children taught this way end up more competent even in the standard language (Osborn, p. 234; John and Horner, pp. 147-49). Of course, in a dialect situation, speech differences are not nearly as great. If the use of several dialects is encouraged in school, there should be few real difficulties of communication. Then special translations may be necessary only for initial reading texts.

The linguistically competent individual adjusts his form of speech *appropriately to various social contexts*, and *communicates* what he intends *effectively*; MCE is not always ideal in these terms. Labov argues "in many ways working-class speakers are more effective narrators, reasoners, and debaters than many middle-class speakers who temporize, qualify, and lose their argument in a mass of irrelevant detail" (esp. pp. 163-71). In fact, English teachers do fight excessive complexity in MCE. Francis Christensen (1968) argues against Kellogg Hunt's purported measures of "syntactic maturity": adding detail to a sentence with grammatically disconnected phrases may be more effective than using complicated subordinate clauses.

Bernstein (chap. 3) clarifies his "elaborated code" and "restricted code": they are *not* equivalent to linguistic proficiency and deficiency. But in a socially sensitive area he continues to assert causal links between social-psychological factors of home life and differences in language and communication behaviors, where in fact only correlations may exist (see Blank's warning, pp. 64-65; also chapters by Olim and Plumer).

Bernstein's description of linguistic factors is probably still faulty. First, "Language need only add what the context cannot communicate. Fragmented speech under such circumstances is efficient, not incomplete" (Williams, p. 394). Nonverbal communication is not "context"; it can be more appropriate than verbal communication and just as explicit. When one word can communicate a lot, this can hardly be called a "restricted code." Second, one may deal with either particulars or universals. The goal

of generalizing is to find "one integrated system which can generate order" (Bernstein, p. 52). This is appropriate to dealing with objects as means, but is inappropriate to dealing with persons as ends, where goals are not uniform. Perhaps formal middle-class language tends to promote a universalizing mode *too widely*. Olim links this to alienation and other current social problems. Third, elaborated explanation can be applied either to personal desires or to physical objects; but there is no reason to assume middle-class children have a superior reasoning power or concept of causality (see Labov, pp. 163-78; Cazden, pp. 89-90). Thus the doctrine of learning disabilities may, at least in part, be based on faulty reasoning. Abstract language is usually treated as desirable in educational contexts. Yet abstract words can sometimes convey less information than concrete words and can allow a dangerous distance between language and concrete experience, which encourages the manipulations of bureaucratese (Osgood 1971).

Editor Williams and several contributors emphasize the need for more research. By contrast, Plumer says: "In the last analysis it is the low social and political priority assigned to equality of opportunity . . . not the absence of adequate research—which is the chief obstacle to effective policy." For future research, we believe researchers are obliged to eliminate social bias more effectively, not only in their factual determinations but also in the questions they choose to ask. (Thus they should, on their own initiative, eliminate language-bias in testing, and should not assume MCE to be ideal.) Why for so long have we not evaluated teaching methods and schools just as critically as we have evaluated students to whom we attribute so many difficulties? Neither innate nor behaviorist positions (Harry Osser, chap. 13) grant the child an active role in enlarging his learning abilities. How to interest children is rarely discussed, though it is admitted as perhaps the most important goal (Plumer, p. 300; Labov, pp. 179-80).

Equal opportunity carries with it not only the acceptance of a multicultural society (Williams, p. 19) but even *limits on the legislation of culture*. If one language or culture is chosen for that of official affairs, including education at the lower levels, *the others should be offered compensation*. The language varieties grouped as "Black English" must be granted not only a long past history (William Steward, chap. 17) but also a future. Black English may show certain natural historical developments (such as loss of grammatical inflections) which MCE is also undergoing but has not yet taken as far. There are forms of BE parallel to other social and regional standards in the United States, and these forms must not be called "non-standard," as even Labov does. In fact the prestige of BE may be rising in New York (Labov, p. 273).

The elimination of linguistic variety may be not only impossible but also undesirable (Fishman 1966). Respect for diversity may be possible only when stereotypes are combated by personal interaction, and cultural differences are positively valued (Williams, pp. 396-97).

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Society in India. Vol. 1: *Continuity and Change*. Vol. 2: *Change and Continuity*. By David Mandelbaum. Berkeley: University of California Press, 1970. Pp. xi+323+37+14; ix+655+37+14. \$10.00 each.

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In Britain and in America sociology has long stood for theory construction and social anthropology for the analysis of observations made in the field. Like all such distinctions this one is far from perfect, but it is largely true. David Mandelbaum's *Society in India* falls within this Anglo-American anthropological tradition. His view of social anthropology clearly is that of a cumulative science, and his conception of society that of empirically observable regularities in social interaction structured by cultural norms and values.

The scope of Mandelbaum's book is ambitious. He has set himself the task of assessing the extent of our empirical knowledge of some aspects of Indian society around 1968-69. A deeper and more significant aim is to attempt to grasp the nature of Indian unity that seems to underlie apparent diversities. The book "is intended as a general survey of basic social principles and patterns followed by most of the people of India. It is more a grammar of social relations than a dictionary of caste practices or a glossary of names" (p. 10). His method consists of examining empirical materials on microorganizations, collected and reported by hundreds of fieldworkers, in the light of the macrothemes of Indian civilization.

Society in India opens with a discussion, all too brief, of the ideological background of Indian society (chap. 2). It is not cultural norms and values, however, which so much emerge as the organizing framework for the examination of data as do some concepts derived from functional-structural-systemic theories of the types associated with British social anthropology and American sociology (chap. 1 and Appendix). The principal notions employed are those of "system" (interchange-counterchange), "structure" (ordered arrangement of relations), and "role" (goal-oriented, normatively defined behavior). "When some participants do not carry out the kind of *interchange* that others in the system anticipate, the others respond in regular ways of *counterchange* to restore some systemic regu-

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larity in their relations" (p. 5). Continuity and change are thus seen as functionally interlinked. The individual role occupant is described by Mandelbaum as "a manipulator of his culture and society as well as a creature and carrier of it" (p. 427). Such a theoretical perspective does not, of course, provide for drastic, sudden changes, but that would hardly seem to be a crippling disadvantage when one is dealing with India, particularly with rural India.

Mandelbaum focuses on village society in India, on its principal "groups," namely, the family (chaps. 3-7), lineage (chap. 8), *jati* (i.e., caste as commonly understood: Mandelbaum wisely avoids use of the word "caste," except as an adjective, because of its several referents) (chaps. 9-18), and village community (chaps. 19-22). The emphasis is on the structure of these "groups" and their interaction. Beyond the village and encompassing it lie Indian civilization and the forces making for modernization. Religion, economics, and politics enter the discussion only fleetingly. Their attenuation is defended thus: such an emphasis "would entail a different treatment. [Further t]his societal emphasis is made by many of the people themselves" (p. 10).

Change is identified as a familiar and recurrent process within Indian society though essentially subordinate to structure. Its typical manifestations are said to be mobility and adaptation within the caste system (chaps. 23-26) and, nowadays, politicization (chap. 27). Also discussed in the context of recurrent change are religious and tribal movements (chaps. 28-32). There is a fairly detailed discussion of tribal peoples and Muslims. The book concludes with a discussion of the nature of contemporary trends, some of which may turn out to systemic shifts rather than merely recurrent changes (chaps. 33-34). Thus, he observes, "The systemic changes that are in view do not point directly toward an unstratified society but toward fewer and more homogeneous social groups" (p. 634). Further, "People throughout India commonly keep to traditional social patterns while adapting them to modern circumstances. Abstract ideals are most readily revised; fundamental motifs of cognition and motivation seem little altered and are evident in the newer arenas of competition" (p. 655). Modernization is thus rightly seen as a process of juxtaposition rather than of displacement.

Mandelbaum draws his data from his own fieldwork among the Kotas (much less than one would have perhaps liked, considering the excellence of his publications on this people) and from almost 700 works—books, monographs, dissertations, papers—nearly all of them published, by various authors. The geographical distribution of the places and groups covered is widespread, though some areas are better represented (e.g., Mysore, Uttar Pradesh, West Bengal) than others (such as Assam, Bihar, and Rajasthan). He has also used materials from West Pakistan while discussing Muslim social structure. With a few exceptions the works on which *Society in India* is based are post-1950 publications. In other words, Mandelbaum has made a real and commendable effort to present us with an "all-India" picture which is contemporary. Judging by his extensive use of my mono-

graph on Kashmiri Brahmins (*Family and Kinship: A Study of the Pandits of Rural Kashmir* [New York: Asia Publishing House, 1966]), he seems to have taken great pains to employ other peoples' works without misrepresenting them. This is no mean achievement considering the length of the bibliography. The physical and mental resources required for such a task can well be imagined. Mandelbaum says that the idea of writing a book like this one occurred to him when he started his field research in India in 1937. Several drafts seem to have preceded the final version (see pp. vii ff.). *Society in India* may be, therefore, truly called the work of a lifetime. It will be a landmark in anthropological literature on India and is bound to be of immense value to those trying to get acquainted with the sociology of Indian castes, religious communities, and tribes.

It should be obvious that the strength of a work like Mandelbaum's must primarily lie in the availability of good materials. Thus, it is not at all surprising, for example, that he should have devoted greater attention to caste than to kinship, to the family than to the lineage, lineage-like categories, or the household. The differences in the attention that he gives to various topics, in fact, reflect the strength and weakness of Indian ethnography. Here it may be added that from among a few major works not considered by Mandelbaum the omission of books by Brij Raj Chauhan on Rajasthan (*A Rajasthan Village* [Delhi: National, 1966]); Ramkrishna Mukherjee on Bengal (*The Dynamics of Rural Society* [Berlin: Akademie Verlag, 1957]), and *Six Villages of Bengal* [Bombay: Popular Prakashan, reprint ed., 1970]); and Dakfin Sivertsen on Tamil Nadu (*When Caste Barriers Fall* [London: Allen & Unwin, 1962]) seems grievous, as these would have helped him to add detail to some dimensions of village life, particularly political and economic.

Then there are the problems of ensuring the quality and comprehensive coverage of data. Often these two aims are in conflict. It is obvious that Mandelbaum has evaluated the available materials to make his selection of evidence, but we know this only indirectly. Within the book itself his concern is to piece together the evidence and not to pass judgment on it. Only occasionally does he permit himself to highlight areas and problems on which more fieldwork is called for. For example, he calls for more research on regional identification in India in an ever-widening circle of loyalties (see pp. 393 ff.). One wishes he had done this oftener: that would have greatly enhanced the value of his book.

As for comprehensive coverage, the task can be heartbreaking when one is dealing with a country as large as India: in Mandelbaum's own words, "The more one draws up, the more there is to draw" (p. 9). In other words, a survey like *Society in India* is bound to become outdated in a few years. Already there are indications that urbanism is no longer a negligible aspect of society in India. A considerable number of sociologists and anthropologists, including—to mention just a handful of names—Yogesh Atal, Richard Fox, M. S. Gore, Owen Lynch, Adrian Mayer, and Satish Saberwal are actively engaged in research in towns and cities, and their publications so far include several books and many papers.

Turning to a more serious problem, is Mandelbaum's method adequate for the purpose he has set before himself? He wants to grasp the unity that underlies social and cultural diversities in India. He seeks to do this by comparing empirical evidence drawn from different parts of India and by arriving at a lowest-common-factor picture. Inevitably, diversities get played down even when they are important as, for example, between north and south Indian kinship. Also, the limitations of the comparative method are not squarely faced. Thus, how far is it permissible to generalize on the basis of comparison between two castes in the same region—say, Brahmans and Rajputs in Rajasthan—or between the same caste (or *varna*) in different parts of India—say, the Brahmans of Tamil Nadu and Kashmir?

It seems to me that since diversities in India are an attribute of regional empirical systems, and are cultural rather than structural, the quest for unity must not be made in terms of them but in terms of some structural principles that are seen as fundamental to Indian civilization. Louis Dumont's attempt to discuss society in India (see his *Homo Hierarchicus: The Caste System and Its Implications* [Chicago: University of Chicago Press, 1970]) in terms of the basic principle of hierarchy is an example. Not that Dumont's approach is completely nonproblematic: the relation between ideology and the regularities in empirical behavior calls for careful consideration because of the discontinuity which characterizes it in certain contexts. Mandelbaum refers to this problem of discontinuity at several places in his book (see, e.g., pp. 628–29); and though he affirms at the very outset the relevance of ideology to empirical behavior, his book pushes it into the background—at least it seems so to me.

Finally a comment on the frills. The end-map giving the location of 97 villages, tribes, castes, etc., which feature prominently in the text, is useful, but it is a pity that it has not been included in the paperback edition which will naturally be used by more readers, particularly students. The bibliography is extensive, and serious omissions are few. The index, however, is far from adequate, in terms of the number, classification, and content of the entries. I have found more references to some entries in the text than are listed in the index. A few printing errors also seem to have crept in. One hopes these blemishes will be rectified in further printings of the book which are sure to follow.

Rural Credit in Western India, 1875–1930: Rural Credit and the Co-operative Movement in the Bombay Presidency. By I. J. Catanach. Berkeley: University of California Press, 1970. Pp. xi+269. \$8.75.

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The role of rural credit in Indian agriculture is by no means straightforward. It is confused, on one hand, by virtue of frequently complex social and

political relationships which exist or develop between the parties involved, and on the other by an implicit interdependence of production and consumption decisions in peasant households. When cooperatives are considered as an additional structure for credit supply, the whole subject of rural credit and production is complicated by a new set of bureaucratic and ideological concerns; concerns which seem, at best, remote from the daily decisions of peasant households and agricultural production.

I. J. Catanach has provided us with some important insights and a well-documented account of the cooperative "movement" in the Bombay Presidency from its inception, following the Deccan Riots, up to the onset of the Great Depression. He promises us a critical examination of "first, the diagnosis of the rural credit situation in the Bombay Deccan which was offered in the years after 1875, and, then, the working . . . of the cooperative societies which were intended to provide a remedy for the supposed problem of agricultural indebtedness" (pp. 1-2). He hopes by this examination that the reader will be able to discover whether or not "the modesty of the success of co-operative societies in the Bombay Presidency [was] primarily the result of inertia within Indian society, or . . . the result of the essential unsuitability of the innovation offered" (p. 4). In his search for an answer to this question, the author deserves praise for an apparently very thorough examination of an immense number of official and unofficial documents and records relating to the establishment and encouragement of rural credit cooperatives in the Bombay Presidency.

We certainly agree with Catanach when he tells us that rural credit cooperatives were created by the state in what appears in retrospect to have been a genuine concern for rural welfare—in contrast to irrigation and transportation developments, seen by some as merely exploitative in intent. Until their introduction in India, rural cooperatives had not been tried in a non-Western country. They appeared to their proponents in the early decades of the present century as a multipurpose weapon against a host of real and imagined ills plaguing India's rural economy. They held forth the promise of increasing—or at least mobilizing—rural thrift; and of relief to the agricultural debtor and the process of land transfer from agricultural classes to nonagricultural moneylenders which was thought to lie behind the riots of 1875. Credit cooperatives could link the interior to the credit sources of Bombay. They could serve as a nucleus about which the real or putative "village communities" might reemerge to serve the new needs of the growing nationalist movement.

As an account of what may be viewed as India's first serious encounter with rural development projects, Catanach's book is a valuable contribution. One cannot but be impressed by the dismal lack of understanding about the nature and regional variations in peasant agricultural organization which was manifest in the initial writings and policy recommendations of Bombay's English "guardians"—Harold Mann excepted. His description of Indian participation in both official and unofficial roles is no less interesting. To students of nationalist politics, caste movements, and administrative reforms, Catanach has illuminated an interesting area of

reference; but to the economist interested in the role of cooperatives as institutions for credit supply and as instruments of rural economic development, the book is deficient.

We read (p. 223) that cooperatives in 1930 supplied about two-fifths of the credit needs of some 10.7% of the province's rural population. The level of successful participation was much higher in such agronomically and culturally disparate districts as Dharwar, Broach, and East Khandesh, with 27.8%, 24.8%, and 18.7%, respectively, of the rural population involved. Unfortunately for the student of peasant history, Catanach's book does not go deeply enough into the reasons and events which lay behind the mixed picture of relative success and failure of rural cooperatives. Conditions of land tenure and weather risk provide ample explanations for some cases, but much greater detail is obviously needed for others. There are no detailed descriptions of the peasant economy of this period; neither is the reader given a much-needed description of how and by whom decisions were made at the level of the village cooperative society. It is perhaps unfair to criticize Catanach's book for these omissions, since he makes no pretensions of offering the reader detailed insights from the village level. Indeed, such insights would require both a measure of good luck and much painstaking labor at the local level. Nevertheless, if students of peasant history are to understand all the reasons behind the "modest success" of the cooperative movement in the Bombay Presidency before 1930, a considerable amount of work must be accomplished at the local level.

Anyone who is acquainted with post-Independence Indian rural development programs with their emphasis on cooperatives, community development, and reconstruction of ancient "democratic" institutions will find a certain sardonic note in Catanach's description of the cooperative movement before 1930. One suspects that peasant agriculture is still not well understood by those Indians and foreign advisers who would transform it into something better.

Singapore Population in Transition. By Saw Swee-Hock. Philadelphia: University of Pennsylvania Press, 1970. Pp. xiv+227. \$12.50.

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This is the only major work published so far that covers so many aspects of Singapore's population growth for a period that extends back into the last century. The book, an enlarged version of the author's M.A. thesis completed in 1960 at the University of Malaya in Singapore (now University of Singapore), is largely a descriptive overview of Singapore's demographic trends from approximately 1870 to 1965. Chapter topics in this book are organized in the conventional way: historical background,

sources of demographic data, population growth and distribution, migration, population structure, fertility, mortality, labor force, family planning, and a concluding section on trends and prospects. However, there are two unconventional features, both of which deserve comment later on: the book does not have an introductory chapter but it contains a chapter on male working-life patterns.

Of course, the value of a book cannot be judged by its table of contents. Without an introductory chapter, the reader is left to himself to determine the conceptual scope of the book and what problems, if any, the author is attempting to bring to light. Having read the book, one has the distinct impression that this is essentially a compilation of what materials there exist on the demography of Singapore in the form of many tables and other statistics, accompanied by a straightforward description of the figures without much analysis either in a theoretical or policy-oriented framework. As such, it is difficult to say what the major thesis of the book is. Each chapter constitutes a highly compartmentalized unit in itself with little or no relation to others. Even in the 10-page concluding chapter ("Trends and Prospects") we found no evidence whatsoever of summarizing or integrating previous discussions. Instead, the last chapter (pp. 161-70) is devoted to a brief discourse on population and labor-force projections plus a three-paragraph discussion attempting to relate, rather incoherently, population pressure on development, growth of per capita income, the government's population policy, and the slackening rate of population growth.

The merit of this book lies in its bringing together into one volume much of the historical demographic data on Singapore with respect to fertility, mortality, and migration. We should be grateful to Saw Swee-Hock for assembling of data from sources which, as Irene Taeuber stated on the book jacket, are often "dispersed, fragmentary, defective to varying degrees, largely inaccessible outside specialized libraries." However, precisely because sources are diverse and data often defective, it is disappointing to find that the author has not added more footnotes to the text and tables indicating the sources of data and how he has tackled the problem of known data deficiency. In fact, with very few exceptions the tables and other statistics in the book show no source at all! The author of such a book is responsible for indicating sources of data so that others who have access to the same sources may be able to verify some of his arguments by reanalyzing the same data. One case in point concerns the age-specific fertility rates for the three main races in 1947 (p. 83). One is puzzled to find these rates because, according to the author (p. 17), statistics on live births by age and race of mother were first available only in 1952. There is no explanation in the book on how the author derived these rates. The puzzle becomes more acute when we observe that these rates are quite different from those given in a paper, "Fertility and the Increase of Population in Singapore," by You Poh Seng which, incidentally, the author fails to list in his bibliography. In any event, Saw Swee-Hock's figures are incorrect, at least inso-

far as he relates the live births of Singapore to the female population of the colony of Singapore, which in 1947 included other islands.

Another related point has to do with fertility changes in the prewar period. Basing his discussion only on the changing crude birthrates, the author is careful to emphasize that the increasing birthrates should not be readily interpreted as signifying an upward trend in fertility because of increasing normalization of the sex ratio of the population. Rightly so. One wonders, however, why the author chooses to examine only the crude birthrate when statistics on sex ratio are evidently available—evident from the author's own discussion of the changing ratios (pp. 58–62). Given the statistics, we can readily eliminate the distorting effect of the changing sex ratios by simply relating live births to the female population instead of the total population. As a matter of fact, we can even compute general fertility rates for the years, 1911, 1921, and 1931 because more detailed statistics are available from the reports of the censuses conducted in these three years. As the general fertility rates of all three main racial groups in Singapore showed significant increases between 1911 and 1931, we are more inclined to think that there had indeed been a genuine rise of fertility before the war.

The foregoing illustrations serve to indicate the lack of depth in what little analysis that the book provides. This can also be seen in the chapter on labor force, which is nothing more than a brief description of the 1957 census data, and the chapter on male working-life patterns, which comprises a series of simple computational procedures without any discussion of the results in relation to the structure of the labor force or the larger socioeconomic system. In the chapter on family planning and population control, the author simply summarizes the history of family planning associations in Singapore and outlines the essence of the government's first five-year program scheduled to begin in 1966.

Although the book was published in late 1970, almost all series of statistics in it do not extend beyond 1965. This is unfortunate, because a great deal more demographic data and analyses have become available since 1966 when a nationwide sample household survey was conducted. More importantly, during the past several years Singapore has undergone tremendous social and economic changes. The implementation of substantial public housing and urban renewal programs and the rapid industrialization since 1965 have brought forth very significant changes in population distribution and labor-force structure. In the second half of the last decade, fertility continued to decline more sharply than the author's most optimistic assumptions in his population projections given in Appendix Three (p. 205). The government's first five-year family planning program has been completed, and more systematic evaluation of data will soon be available. In the light of all these developments and the publication of 1970 census results, those parts of the book which attempted to sketch a contemporary demographic view were already out of date when they appeared in print.

Until a better volume comes along, we would recommend this book to libraries and others interested in the historical demography of Singapore. One final note on the bibliography: while the author gives himself generous quotations on publications having to do with Malaya, several known articles directly relevant to the population of Singapore have been omitted, something which we hope is merely an oversight.